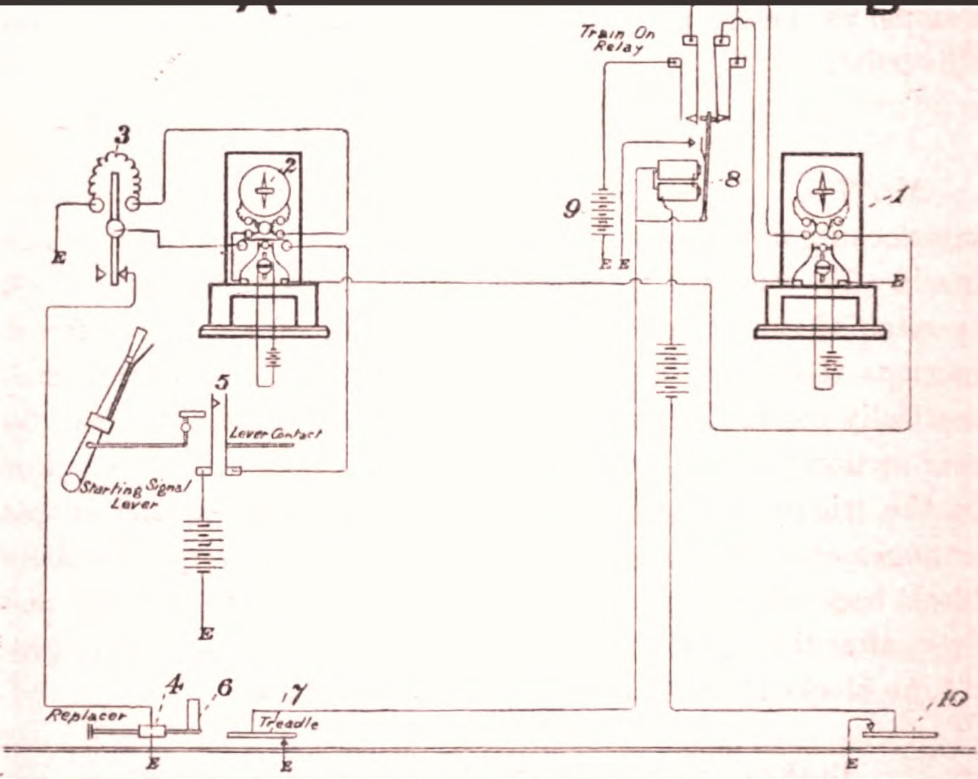

Proceedings of the Institution of Electrical Engineers

Institution of Electrical Engineers

JOURNAL

OF THE

INSTITUTION OF ELECTRICAL ENGINEERS,

INCLUDING

ORIGINAL COMMUNICATIONS ON TELEGRAPHY AND ELECTRICAL SCIENCE.

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Institution of Electrical Engineers.

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The Two Hundred and Ninety-fourth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, January 14th, 1897—Dr. John Hopkinson, F.R.S., late President, in the Chair.

The Minutes of the Annual General Meeting, held on December 10th, 1896, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Tom Scott Anderson.
John A. Dawson.
G. F. Metzger.
William L. Preece.
Gilbert Scott Ram.
John Henry Tonge.

From the class of Students to that of Associates—

Charles D. Burnet.
John Drayson Dymond.
Allan B. Field.
Edmund John Fox.
Albert Alexander Horn.
Charles D. le Maistre.
Charles Stanley May.
F. H. Merritt.
Drogo Montagu.
Eliot Charles Pringle.
Hastings Victor Sadler.
Adolf Shoder.
Frank C. Thomas.
Frederick William, Baron de Tuyll.
Richard Norman Vyvyan.
Edmund Basil Wedmore.

Messrs. H. Human and C. E. S. Phillips were appointed scrutineers of the ballot.

The CHAIRMAN (Dr. Hopkinson): It is my sad duty to have to announce to you the death of Mr. Jacob Brett, an Honorary Member of this Institution. His connection with electrical engineering goes back to a time before many of us were born, and far beyond the time when most of us had any connection with this profession. Most of us only knew him by repute, but we did know this—that he was one of the first founders of a very great and important industry, which has had already, perhaps, more than any other industry to do with the advance and development of civilisation. It is my duty to move—"That the President, "Council, and members of the Institution of Electrical Engineers "hereby record their deep regret at the decease of Jacob Brett, "Esq., Honorary Member of the Institution, who was, in con-"nection with his brother, the late John Watkins Brett, Esq., one "of the earliest pioneers of submarine telegraphy; and that a "copy of the foregoing resolution be forwarded to the Rev. F. H. "Brett, with the expression of the sympathy of the President, "Council, and members with him on the loss of his brother."

Mr. LATIMER CLARK: I beg to be allowed to second this motion. Mr. Brett has been a very old friend of mine—not, perhaps, since the days when he performed his first achievements in submarine telegraphy, but certainly for the last 30 years; and it was with deep grief that I had the honour, together with the President and the Secretary of this Institution, of following him to-day to the grave. He died on the 9th January, in his eighty-ninth year. I think it was on November the 12th, 1891, that an admirable portrait of Mr. Brett was presented to this Institution by the artist, Miss Alice Bolton; and I then made some remarks on Mr. Brett's career, and endeavoured to point out the great importance of the work which the two brothers had performed in connection with submarine telegraphy. In the course of those observations I said I thought Mr. Brett had conferred a greater benefit on mankind than any man then living. That was a rather strong expression, but I still adhere to

it. I pointed out that, as regards the Dover-Calais cables of 1850 and 1851, the first submarine cables in the world, there is no manner of doubt that the Messrs. Brett were the sole originators and performers of this great feat of electrical engineering. I have studied the subject with care, and I do not think there is any attempt at rivalry on the part of any nation or any person. But that is not their only claim to merit. It is not so generally known that the elder brother, Mr. John W. Brett, also founded and originated the Atlantic Telegraph Company as well as the Submarine Telegraph Company. He and his brother registered a company in June, 1845, called the "General Oceanic Telegraph "Company," which was the first embodiment of the idea of submarine communication. That was so early that it was even before "The Electric Telegraph Company" had become incorporated. It was the first registered telegraphic company in the world, and —what is curious—it was formed to lay, not land lines, but submarine cables. Later on, and working without any intermission, they formed, in October, 1856, a company for laying an Atlantic cable. That was the "Atlantic Telegraph Company," and Mr. John Brett was one of the directors, and subscribed £25,000 towards it. His name stands at the head of the list to-day as the first subscriber, and the name of Mr. Cyrus W. Field is next to it—also for £25,000. Sir Charles Bright was the engineer of the company, and he attained great success; he laid the cable safely, and transmitted messages through it, and thus proved the entire practicability of the undertaking, although it was not permanently successful. The insulation of the cable had been, unfortunately, destroyed by the heat of the sun after manufacture, and while lying in the contractor's yard.

I think therefore, the brothers Brett are distinctly entitled to lay claim to be the founders and fathers of submarine telegraphy. I am glad to add that Mr. Jacob Brett has entrusted to me all his papers on the subject, which I think you will find extremely interesting. I hope to have the pleasure of presenting them to the Institution in the course of a few weeks, when I shall perhaps have the opportunity of making some further observations on the subject. The brothers Brett, who were both unmarried, were

very much attached to each other. After his brother's death Mr. Jacob Brett lived for a long time in Paris. I am told that his horses and carriages were the cynosure of everybody on the Champs Elysées. He managed just to escape from Paris in time to avoid the siege. After that he came upon bad times. Mr. J. W. Brett was a very business-like man, and apparently conducted his affairs skilfully and well. Mr. Jacob Brett, on the contrary, was not a business-like man, and he contrived to lose a considerable fortune in Turkish bonds and other doubtful investments. He ended his life in comparative poverty, and, I am sorry to say, in some bodily suffering. By the aid of this Institution we succeeded in getting for him £100 a year from the Civil Service Fund in 1886, and that is all that he has had to live upon for several years. We had, with the kind co-operation of Lord Kelvin, Lord Rayleigh, and others, applied quite recently to the Government for a further sum to be added to that pension, and I have reason to think that our petition would have been listened to very willingly. But, unfortunately, at the moment when he most would have enjoyed it, he has been taken from us. Happily, he was well aware of the steps which were being taken on his behalf, and very much approved of and highly appreciated the names of those who had consented to sign the memorial. It was a great solace to him, in his last days, to feel that help was coming. In addition to this, some individual members of this Institution—and I include especially Mr. Alexander Siemens—subscribed a sum of money last year, which was collected and paid to him by the Secretary, and thus kept the wolf well from the door; so that he never really was in actual want of anything, although he could have no society, and for many years past had lingered on in a state of semi-poverty—a condition of things very unworthy of a great and rich telegraphic country like ours, and of a civilisation which has benefited so largely by his inventions. He left no effects except his papers.

The motion was carried unanimously.

The CHAIRMAN: My next duty is a very agreeable one. It is to present the premiums of the Institution to those who have won them. The first is the "Institution Premium," which, on the

present occasion, has been increased from £10, the usual amount, to £15, as there are three gentlemen who have won it by a joint paper. They are Mr. Stanley Beeton, Mr. Charles Percy Taylor, and Mr. James Mark Barr. The first two are prevented by their professional duties from attending this evening.

I have great pleasure, Mr. Barr, in presenting this premium to you.

The other premiums presented on this occasion are the "Students' Premiums," two of 3 guineas each. One has been awarded to Mr. Basil Wedmore, to whom I have great pleasure in presenting these books on behalf of the Institution.

The other premium has been awarded to Mr. Victor Watlington and to Mr. Edward Ray. Mr. Watlington is unable to be here, as he is suffering from the effects of a serious accident. With regard to Mr. Ray, it is my painful duty to have to inform you that he has died since the award. I will ask Professor Ayrton to say a word or two in reference to Mr. Ray, as he was one of the Professor's students.

Professor AYRTON: With every desire not to delay the important business we have before us this evening, I am glad to have this opportunity of saying just two words about poor Ray, who died last week of typhoid fever, at the early age of 20.

He was so young that he could hardly have been known to many outside his comrades and fellow-workers; but he was so able—one of the ablest and brightest students who ever came to me—that it is quite certain, had he but lived, he would in time have become known to you all. His ability was very marked; but the sweet charm of his manner, which endeared him to myself and to everyone else with whom he came into contact, was equally striking.

A relative of one of your early Presidents, Mr. Charles Walker—a well-known pioneer in submarine telegraphy,—the winner of the John Samuels scholarship in 1895,—the winner last year of the medal which was founded in memory of your first President, the late Sir William Siemens,—poor Ray has not lived long enough to receive, at the hands of your President to-night, this last honour which you have awarded him. But I feel sure that the blow will

be lessened to his parents by the consolation that the merits of their son—boy though he was—have been already recognised by this Institution.

The CHAIRMAN: I have now the pleasure to hand to Mr. E. W. Marchant, of the Central Technical College, a cheque for £50, representing the Salomons Scholarship, which has been awarded to him.

I have no doubt that it was a peculiar satisfaction to Professor Ayrton that the first cheque he had to sign as Treasurer of the Institution was a cheque on behalf of one of his own students.

It is now my lot to perform the last duty as your President, and that is, to vacate this chair to my successor. It is a matter of great satisfaction to me to feel that I have, in Sir Henry Mance, a successor who is worthy of the best traditions of the position which I am now vacating; and it is particularly a satisfaction to me to know that he is a gentleman who has made his reputation and attained to his distinction in the older branch of our profession, namely, telegraphy, and particularly in a specially scientific branch of that profession, that of submarine telegraphy. I will not take up more of your time this evening, as we have had more than the usual amount of business to get through, but will at once vacate the chair to Sir Henry Mance, and make way for him to read to you the Address which you are all waiting for with peculiar interest.

Sir Henry Mance then took the chair.

General WEBBER: It has been deputed to me to occupy your time for two or three minutes longer, on a subject the omission of which you would extremely regret. I am asked to move—"That "the cordial thanks of the members of this Institution be given to "Dr. John Hopkinson for the admirable manner in which he has "discharged the duties of President during the past year." Dr. Hopkinson having effected the happy despatch which has placed him on the left hand of our new President, I do not propose to toll his curfew knell longer than to draw your attention to the fact that he has for the second time in the life of this Institution filled the responsible position of its President. I daresay some o you will remember that his Address at the commencement of last

year was devoted largely and very usefully to technical education, but there is one incident of his tenure of office in the year 1896 which shows his remarkable versatility. While devoting himself to the onerous duties of his profession, while spending as much time as we know he does in original research and investigation of science, he actually led this Institution, at a time when the whole country was stirred with military ardour, to propose that the services of electrical engineers should be offered to the Government as an assistance in time of her defence in the various capacities in which members of that profession might be useful to the country. I think, Sir, that you, with your long acquaintance with India and the military services there, will thoroughly appreciate the kind of energy which Dr. Hopkinson had to expend when he followed this particular line. It is always a pleasure to be supported by Mr. Siemens; and I have to ask this meeting, after he has addressed you, to give their cordial thanks for the services, which we on the Council so thoroughly know and approve, of Dr. Hopkinson as President during the past year.

Mr. ALEXANDER SIEMENS: I have great pleasure in seconding this motion. I suppose that I was selected for the duty because it has been my sad lot to have hardly ever been here during the year 1896. So my opinion of Dr. Hopkinson is entirely unbiassed, and I can only endorse all that General Webber has said.

The PRESIDENT (Sir Henry Mance): The resolution I have to put is one of the earliest and pleasantest duties which falls to the lot of the new President. You have all heard the resolution; it is one which is usually voted by acclamation, and I presume this will not be an exception to the general rule.

The resolution was carried by acclamation.

Dr. HOPKINSON: Gentlemen,—I have really had a very pleasant time as your President during the past year. That time has been made pleasant to me by the universal kindness I have received from the members of the Institution, and also from my fellows in office—from the Vice-Presidents, Past-Presidents, and Members of the Council, and particularly from our excellent Secretary, Mr. Webb. Without that help and without that sympathy the office would indeed have been anything but agreeable, but, with it, has been an exceedingly pleasant duty to

perform. I can only thank you for the support you have given me, and say that if, in my humble way, I have been of any service to the Institution, that has been a very ample reward to me.

INAUGURAL ADDRESS.

By Sir Henry Mance, C.I.E., President.

Gentlemen,—In electing me your President for the coming year, and thus according me the blue ribbon of the electrical profession, you have conferred on me an honour of which any man may be justly proud, and for which I thank you sincerely. If I approach my task this evening with diffidence, it is because I fully appreciate the responsibilities of office. But, gentlemen, I hold the opinion that that mortal is wise who accepts without demur the gifts bestowed upon him by the gods. It is not for me to question the wisdom of your choice, but it is for me to do my best to justify it. My term of office on your Council has extended over some years, and I know the sympathy and help which the President of this Institution may always rely on receiving, not only from his colleagues, but also from the general body of members. You cannot always expect to have an ideal President; but I trust that when my period of office is over, I shall be able to look back and congratulate myself that the dignity of the office and the interests of the Institution have not suffered in my hands, and that whatever developments may occur meanwhile, the Institution will continue, as hitherto, to lead the way in electrical science.

There are periods in the lives of Societies, as well as in individuals, when it is excusable to pause for a moment and indulge in mutual congratulation. It may have escaped your notice that this Institution is now entering into its twenty-sixth year of existence. It has, therefore, completed its first quarter of a century. I have recently been glancing through the many volumes of our Proceedings, and I think you will agree with me that they form a record of 25 years of useful work. During this period the nominal roll of the Society has increased from 100 to nearly 3,000. Founded originally by electricians and telegraph men, it has adapted itself to modern requirements, and is now the oldest and largest Institution of Electrical Engineers in the world. It represents all branches of the profession, and will, I trust, in

the future as in the past, aim to include in its ranks the names of *all* who are interested and devoted to the development of electrical science. Some of the founders of this Institution are still with us, and I think it is a matter of congratulation that General Webber, who took such a prominent part in the foundation of the Society, is here to-night.

Although I have been closely associated with important applications of electricity to heavy engineering and electro-chemistry, I feel that the only claim that I have to the position that I now occupy is the fact that for the best part of my life I have been actively connected with submarine telegraphy. I suppose I am one of the few who can say that they have assisted at the manufacture of a cable, the laying of it, the working of it, its subsequent repair on nearly 200 occasions, and, finally, more than 20 years afterwards, had entrusted to them the duty of laying a new cable in its place; that is to say, I have been closely associated with a submarine telegraph cable from its infancy to its old age.

For 40 years I have never lost touch with the telegraph; so that, after a few general remarks on other topics, I shall naturally turn to submarine telegraphy as the chief subject for my Address this evening.

If this subject has to some extent dropped into the background of late years in the discussions of this Institution, it is not by reason of its want of importance, but because of the rapid and extensive development of other branches of electrical engineering; with regard to these I do not mean to trouble you with statistics, as figures and facts in connection with

ELECTRIC LIGHTING AND TRACTION

must be so fresh in the memories of those who study the weekly technical journals that I abstain from intruding them on your notice. In the matter of traction we have been outstripped by our transatlantic friends, but there is abundant evidence that this has not been caused by want of knowledge or skill on the part of our engineers or manufacturers; it is, I think, an acknowledged fact that in excellence of design and workmanship our manufacturers cannot be beaten. Our electric light stations are unsurpassed

in the excellence and efficiency of their plant, electrical and mechanical, and unequalled in their suitability to the end in view. The best features of the methods preferred by us are all of home growth; and if we have to borrow from American practice in connection with the electric traction movement now upon us, we shall at least be able to point to our tunnel railways as unique, and to the Liverpool Overhead Railway as the first elevated electric railway in the world.

We have to deal in this country with local bodies which are somewhat difficult to move. This has handicapped us; moreover, our rivals have had the advantage of natural gas and water power. In America there is, no doubt, more room for expansion in every direction; here at home we have been suffering from a period of trade depression from which we have only recently emerged. We are now beginning to reap the benefits of the Electric Lighting Act of 1888, both in the number of towns seeking powers for electric lighting, and also in the extensions required for those already in existence, the magnitude of these latter promising to exceed that of the original scheme. It is to be hoped that in the future the electrical industries will not be fettered too much by officialism, and hard-and-fast regulations, which, formulated with the best intentions, with the view to protect the public, tend sometimes to interfere with the public convenience. It is satisfactory to note that manufacturers are turning their attention to the development of cells especially suitable for traction purposes; the advantages already claimed are, that for any fixed output the weight is reduced in the ratio of 28 to 15, and the space occupied reduced from 19 to 10. Should these expectations be fulfilled there is every reason to anticipate an extensive use of storage batteries. There can be no doubt that, from the point of view of cleanliness, freedom from smell, and steadiness in development of power, storage batteries compare favourably with steam or oil engines. Where suitable localities and proper facilities for recharging are available, it may reasonably be hoped that traction by means of storage batteries has a future before it.

Now for my next subject—

Submarine Telegraphy.

Although the ground has been well traversed on one or two occasions by my predecessors in this chair, I hope to find many points of sufficient interest to justify my bringing them to your notice.

To deal comprehensively with the history of submarine telegraphy up to recent years would be quite beyond the scope of a Presidential Address. To those who desire to render themselves thoroughly conversant with the subject, I recommend the perusal of a work, now in course of publication, by one of our members.*

The earliest record of a subaqueous line is said to be that of the experiment made by Baron Schilling, who, in 1812, exploded mines across the river Neva, using wire insulated with india-rubber. It would seem, therefore, that Schilling deserves the credit of being the first to suggest the employment of insulated conductors under water. In 1838 Colonel Pasley, R.E., made experiments at Chatham to demonstrate the practicability of telegraphing through lines under water. In 1839 Dr. O'Shaughnessy made a series of experiments across the Hooghly. In 1840 Professor Wheatstone demonstrated before a committee of the House of Commons a method for establishing telegraph communication from Dover to Calais. In 1842 Professor Morse laid down in New York Harbour an insulated wire for telegraphic purposes. In 1845 Ezra Cornell laid a 12-mile cable in the Hudson River; this cable worked well for several months, until it was broken by ice.

The earliest record I have been able to discover at Somerset House of any submarine telegraph company is dated the 16th June, 1845, when Jacob Brett obtained a certificate of registration for the General Oceanic Telegraphic Company. The first concession connected with international submarine telegraphy was granted to Jacob Brett and his elder brother, John Watkins Brett, in 1847—that is to say, 50 years ago; so that although advantage was not taken of this particular concession, we may in a sense be said to

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* "Submarine Telegraphs: Their History, Construction, and Working," by Charles Bright. F.R.S.E., A.M. Inst. C.E. (Crosby Lockwood & Son, London.)

be celebrating this year the jubilee of the inception of *international* telegraphy.

The adoption of insulated conductors for *underground* wires, both in Prussia and England, may have contributed to the development of submarine telegraphy. The value of gutta-percha as an insulator began to be recognised about 1847; and we find that in 1849 an English company obtained a charter from the French Government, granting to them the exclusive right of sending telegraphic intelligence between England and the French coasts. The concession was conditional on communication being established by September, 1850; and, with the object of preventing its loss, a single wire, invested with a thick coating of gutta-percha, and sunk by means of leaden weights, was successfully laid, and messages transmitted from coast to coast. I have in my possession some of the core, recovered several years afterwards. I may say that, as a simple core, it is quite as good as any made at the present day. Of course, such a slender wire could not be expected to stand the chafing to which it would be subjected against the rocky bottom; but the immediate cause of failure was its being grappled by some French fishermen, who promptly secured a considerable length of it, in ignorance of the real nature of their find.

In December, 1850, a new concession was obtained by the Bretts from the French Government; the outcome of this concession was that a cable between England and France was laid during the following year. The establishment of the old Submarine Telegraph Company practically dates from this time. The iron wire-sheathed cable used on this occasion is said to have been suggested independently by Küper, Reid, Newall, and Willoughby Smith. The cable was laid by Mr. Crampton. In the present day our copper is purer, our sheathing wires of a better quality, and we have learned to adapt different types of cables to different depths; but we must admit that the 1851 type has served as a model for subsequent shallow-water cables.

We who are accustomed to hear of the laying of a fresh Atlantic cable with hardly a passing comment may be apt to underrate the skill and perseverance which brought about the

laying of the first Channel cable; it may, therefore, be opportune to mention, in justice to the successful pioneers of submarine telegraphy, that more than one subsequent attempt to connect Ireland and England ended in failure.

Considerable correspondence has of late appeared in the public Press regarding the inception and development of submarine telegraphy. I have devoted some time to the investigation of the records at Somerset House and the Patent Office, which are sometimes amusing and generally instructive. I have come to the conclusion that to no one individual can fairly be granted the credit of the inception and development of the submarine cable. The work has been the work of many: men of science, men with engineering skill, and men of capital, have all contributed to laying the foundation of and building up the network of telegraphs which is slowly but surely being extended to all parts of the world.

It is not for this Institution to deal with the rival claims of financier and engineer; but if we, as engineers, desire to do honour to any one individual who pre-eminently distinguished himself in the development of oceanic telegraphy, we have simply to refer to the list of our Past-Presidents, and select the name of Sir Charles Tilston Bright.

The earlier history of submarine telegraphy is marked by frequent failures. 20,000 miles of cable were laid during the first 16 years, but over 9,000 knots of this quantity had been abandoned before 1865. The costly experience thus purchased was derived from the failures of a series of weak and light cables, in many of which the insulation was probably defective or insufficient from the first. Some of these cables were lost during the operation of laying.

The story of the first Atlantic cable has been told by many. The experiment was a costly one, but worth the money, for it demonstrated the feasibility of safely submerging great lengths of cable in depths of over two miles. Although a commercial failure, it was a great engineering success, and during its brief existence gave a practical and striking illustration of its value. The War Department was able to countermand the movements

of certain regiments, and thus avoid an expenditure of some £50,000.

Some of you may remember the Indian Mutiny, and how keenly the want of telegraphic communication with India was felt. A subsidy of £36,000 per annum was granted to the Red Sea Telegraph Company, whose cables, laid in 1859, proved a deplorable failure, notwithstanding the fact that previous to their submersion upwards of 80 different cables had been laid in various parts of the world. The sections were laid too tight, the iron sheathing was insufficient, the depth and nature of the bottom had not been properly considered, and the supervision during the manufacture was probably not of that searching character which we now find to be necessary to ensure success.

The record of the Malta-Tripoli-Bengasi-Alexandria cable, laid in 1861, forms an agreeable contrast to the dismal failure of the Red Sea line. This cable was, I believe, the first sent to its destination in tanks, and systematically tested from the time of its manufacture up to its submersion. The costly failures previous to 1863 gave little encouragement to the British public to favour enterprises having for their object the extension of telegraphic communication to the East. At the same time, it was felt that communication with India was an imperative necessity, and Colonel Patrick Stewart, R.E., was deputed by Government to arrange for the construction of a land line through Mesopotamia, and the carrying out of the cable portion of the line *viâ* the Persian Gulf. A good deal of the original cable laid in 1864 is still in existence. The expedition was completely successful, and communication with India was soon after established by alternative land lines through Turkey and Persia.

It was in connection with this cable that my associations with submarine telegraphy commenced. Five sailing vessels were employed to convey the cable to the Persian Gulf, and as many as 12 vessels assisted during the work, which would now be easily and rapidly executed by one well-equipped telegraph steamer. Communication between the cable ship and towing steamer was constantly maintained during the operation of laying the cable by

means of semaphore or signalling lamps. Six years afterwards the cables of the Eastern Telegraph Company were laid down between Suez and Bombay, and in 1871 China and Australia were brought into communication with Great Britain.

The life of a submarine telegraph engineer is somewhat of a Bohemian character—half a landsman, half a sailor, sometimes working almost continuously day and night for weeks together, at other times enjoying long periods of enforced idleness. The localities chosen for cable stations are frequently isolated and uninteresting, the climate indifferent, and the life monotonous. A more desolate place than Mussendom, near the entrance of the Persian Gulf, it would be difficult to imagine: a small island in a land-locked bay, surrounded by mountainous rocks over 3,000 feet in height; not a vestige of green to relieve the wearied eye.

The Persian Gulf and its vicinity is, however, rich in historic associations. Three hundred years before the birth of Christ, Nearchus, after conveying a portion of the army of Alexander the Great from the mouth of the Indus, disembarked at Bunder Abbas, near the entrance of the Gulf. The main body of the army, conducted by Alexander, made its way westward behind the range of hills which skirts the coast of Beloochistan. It is said that from time to time Alexander indicated his position to Nearchus by means of polished steel mirrors and reflected sunlight. This is the first recorded instance of visual telegraphy, and it is a singular coincidence that the experiments which mainly led to the introduction of sun signalling in the British and other armies should have been made upon the very same ground more than 2,000 years afterwards.

In the Persian Gulf one occasionally witnesses natural phenomena which to the untravelled may appear incredible. In the midst of these mountains near Mussendom I have witnessed during a thunderstorm such displays of lightning as baffle description. I have at certain seasons of the year observed the water in the bay, which was large enough to hold all the fleets of the world, presenting exactly the appearance of blood. At such times, after nightfall, the silvery emerald green phosphorescent effects produced by the moving boats were indescribably beautiful.

Not many miles from Mussendom I have witnessed those mysterious fire circles flitting over the surface of the sea at a speed of 100 miles an hour—a phenomenon not often witnessed, and which no one has yet been able to explain. While steaming along the coast of Beloochistan I have been called from my cabin at night to witness the more common phenomenon of a milky sea, the water for miles around being singularly white and luminous.

In the same locality I have observed the sea to be for short periods as if putrid, the fish being destroyed in myriads, so that to prevent a pestilence measures had to be taken to bury those cast up on the beach. This phenomenon was doubtless due to the outbreak of a submarine volcano and the liberation of sulphuretted hydrogen. In these waters jelly fish are as large as footballs, and sea snakes of brilliant hue are met with in great numbers. On one occasion a swarm of sea snakes forced their way up one of the creeks in Karachi Harbour, apparently for the purpose of having a battle royal, for the ground between high and low water mark was thickly covered with their bodies, in positions which betokened a deadly struggle.

I have seen such a flight of locusts on the coast of Beloochistan that with every swish of a stick two or three locusts would be brought to the ground. The column extended for miles, yet it had all disappeared by the following day, most probably into the adjacent ocean, towards which they proceeded under the influence of the north-west wind then prevailing.

From Jask, on the coast of Beloochistan, I have, in the hot season, observed the most perfect mirage effects. An exact image of a steamer then entering the bay has appeared inverted in the sky; and, still more wonderful, the image of a steamer has been observed over the mountains inland, when the nearest vessel could not have been less than 120 miles away.

Submarine Telegraphy in 1897.

There are to-day more than 1,300 submarine cables in existence, the aggregate length of which cannot be less than 162,000 nautical miles; they vary in length from a quarter of a mile to,

say, 2,600 nautical miles, and may be roughly classified as follows:—

Cables under 5 miles in length			...	761
Exceeding 5	,,	and under 50	...	223
,, 50	,,	,, ,, 100	...	65
,, 100	,,	,, ,, 500	...	155
,, 500	,,	,, ,, 1,000	...	64
,, 1,000	,,	,, ,, 2,000	...	29
,, 2,000	...	... ...	...	8
				1,305

Of these cables about 18,000 knots belong to the various Governments, while the balance of 144,000 knots, mostly composed of long sections, have been laid by private companies. They represent a total expenditure of about 40 millions sterling, of which probably 75 per cent. has been contributed by English capital.

Up to quite recently nearly every telegraph cable of importance has been manufactured in this country; but England can no longer boast of a monopoly in this business. The development of French submarine telegraph enterprises is no doubt the direct outcome of the liberality with which the French Government has in recent years subsidised submarine communications. There are now established well-equipped cable works at Calais, and a smaller establishment at St. Tropez, on the shores of the Mediterranean. In addition to these, there are the Government works at La Seyne, near Toulon. The Italians have cable works at Spezzia, Germany has recently subsidised a direct cable from Emden to Vigo. Our telegraph supremacy may be considered by some as ranking next in importance to the supremacy of our navy. This natural rivalry is not without its significance, and shows that foreign Governments are fully alive to the importance of fostering submarine telegraphy.

The sounding apparatus of Lord Kelvin has added immensely to the possibilities of ocean telegraphy. A sounding which in the old days would have taken two or three hours, and the value of which, even then, would have been doubtful, can now be

ascertained in 20 minutes, and be considered reliable. Notwithstanding the heavy weights employed, hemp lines could not be relied upon. In the Gulf Stream it was found almost impossible to get bottom with them. The use of ordinary pianoforte wire has proved a complete success. With a breaking strain of 270 lbs., it offers an extremely small resistance to the water, and gives accurate results in the strongest currents. Some 2,000 fathoms could be wound in in less than half an hour, even when the ship is steaming slowly ahead.

A knowledge of the contour and nature of the bottom of the ocean is of the utmost value to the cable engineer. He does his best to protect the sheathing of a cable from corrosion; but, so far as my experience goes, 2 inches of good substantial mud is the best preservative. I have on many occasions recovered cable which had evidently been resting in grey mud for nearly 20 years, and I have found it in as good condition as the day it left the factory. Deep-sea cables, when well away from shore, have this advantage: they generally rest on a fairly level and uniform bottom—an advantage not always enjoyed by shallow-water cables.

Professor Geikie, writing on the subject of ocean depths, remarks: "To be told that mud gathers on the floor of these " abysses at an extremely slow rate conveys but a vague notion " of the tardiness of the process; but to be told that it gathers " so slowly that the very star dust from outer space forms an " appreciable part of it, brings home to us, as nothing else could " do, the idea of undisturbed and excessively slow accumulation."

For all practical purposes, the telegraph engineer may regard the oozy bottom of the sea in mid-ocean as in a state of eternal rest; the deeper the water the better the bottom, the safer the cable. In shallow water, however, the bottom, being subject to the action of current tides and waves, is frequently irregular, and we cannot ensure that the cable will rest fairly on the ground throughout its whole length. It is certain that there will be undulations and depressions even though the bottom may be formed of hard mud; the cable rests on the ridges of these undulations, and is suspended in a series of

long or short spans. In the course of years the cable becomes encrusted at intervals with shell fish and marine life. It sways gently with every turn of the tide. Marine monsters, such as sawfish, consider it their larder, and, while raking off the marine life, either rupture the cable or penetrate the core with one of their formidable teeth. In any case, sooner or later, the sheathing corrodes, and the cable parts altogether; or, as the outer wires give way one by one, the cable elongates, the core stretches until the copper conductor breaks, the ends being frequently kept insulated by the dielectric until the cable is lifted for repairs. Some of the faults that occur in submarine cables are of the most extraordinary character. Lightning, earthquakes, landslips, submarine volcanoes, have all to be reckoned with; and the leviathan of the deep has more than once been responsible for an interruption. This sounds incredible, but it is nevertheless true. But probably the most dangerous enemy is the teredo. To ward off the attacks of the latter, the plan is resorted to of covering the core with a thin taping of brass. Submarine cliffs, where the soundings rapidly vary from, say, 50 to 500 fathoms, should be especially avoided, as, if the cable is not taken well inside or outside of the line of soundings indicated on the chart, but rests near the edge of the cliff, long spans will be inevitable. The deep channels at the mouths of rivers have to be negotiated very carefully, or the cable is pretty sure to be fractured sooner or later by submarine landslips.

One of the most curious faults I have met with was that caused by the cable rolling down a submarine slope. In one direction, for a considerable distance, the iron guards straightened themselves out, while in the other direction the length of the lay had been reduced one half, and the cable had twisted itself into the most fantastic shapes.

In order to effect the repairs which are necessary, a fleet of 41 telegraph ships is maintained; the gross tonnage of the telegraph marine being nearly 60,000 tons. The vessels are stationed in all parts of the world, and are fully equipped with he usual machinery for grappling, picking up, and re-laying. This machinery is substantially very similar in design to what it was 30 years ago.

The type of cable now generally adopted for deep water is a great improvement on that used in the earlier deep-sea cables. Five-and-twenty years ago the best wire used for cables had a breaking strain of, say, 50 tons per square inch. To-day wire is easily obtainable having a breaking strain of 90 tons per square inch. The application of a preservative tape and compound to each of the wires forming the sheathing—a plan, I believe, originally adopted at the Silvertown works—must tend to prevent or retard corrosion. As compared with a cable in which the iron guards rest one against the other, there is a saving in weight and in cost of materials; and, although the breaking strain may be slightly reduced, the cable will bear as great a length of itself in water as an apparently stronger type. The deep-sea portion of the latest Anglo-American cable and of the South American cable are of this type; and I consider the design an important improvement upon the old plan, where, to lessen the specific gravity, the sheathing wires were separated by intermediate yarns.

The cost of repairs is a most serious item in the accounts of a telegraph company; but there is a great element of luck in cable maintenance, and, fortunately, with deep-sea cables there is comparative freedom from interruption, though when breaks do occur in great depths the cost of re-establishing communication is likely to be very considerable. Repairing one of the Anglo-American cables is said to have cost on one occasion £95,000. This is probably the most expensive repair on record.*

The following brief notes regarding the amount of spare cable which may be necessary to effect repairs during the life of a cable laid in comparatively shallow water may be useful. You will, of course, understand that such a return can only be approximate, as much depends upon the type of cable, and the care with which it was laid. My experience shows that during the first five years of the life of a shallow-water cable it is prudent to allow for ½ per cent. per annum of its total length to provide for repairs.

* This is, of course, an exceptional case. I have just received a Direct United States cable report, from which it appears that during the past 20 years the cost of repairs, including ship and cable, has averaged about £8,000 per annum.

From the	6th to 10th year, say	$\frac{3}{4}$	per cent.
	11th to 16th „ „	1	„
	17th year „	$1\frac{1}{4}$	„
	18th „ „	$1\frac{1}{3}$	„
	19th „ „	$2\frac{1}{3}$	„
	20th „ „	$2\frac{1}{2}$	„
	21st „ „	3	„

In addition to this expenditure of fresh cable, we have also to consider the cost of the engineering staff and the maintenance of the repairing steamer, should an independent vessel be employed. Three per cent. on the total length of the cable per annum after 21 years does not at first sight appear to be considerable for a shallow-water cable; but there comes a time when, in consequence of the great public inconvenience, loss of revenue, and cost of repairs, it is cheaper either to make extensive renewals or to lay a new cable. Assuming the cable to be 1,000 miles in length, and that the foregoing estimate is approximately correct, then 30 miles of cable would be expended annually. This would probably mean 10 or 12 interruptions. As a matter of fact, 16 interruptions was the actual number that occurred in the Persian Gulf cable of 1,740 knots during the twenty-first year. The inconvenience of such frequent repairs was felt to be so great that the alternative of picking up, stripping, and re-sheathing nearly the whole of the weaker sections at the Government cable factory at Karachi was resorted to, with most advantageous results.

Many years ago (in the seventies) I had the privilege of being allowed to inspect the working on the Atlantic cables at Valentia. For several days I watched the signalling conducted by operators, who were splendid "mirror" readers. The siphon recorder had not then come into general use. A rate of over 20 words, or 100 letters, was obtainable during a trial; but I noticed the speed obtained with ease in practice, when messages containing a fair proportion of foreign and code words were being transmitted, was 15 and 16 five-letter words per minute. At this rate, 50 or 60 ten-word tariffs were transmitted within the hour. This included preambles, collations, and repetitions, the latter being extremely rare. By abbreviating the preamble and

collations in accordance with modern practice, much greater commercial efficiency is obtained, and the proportion of words paid for becomes greater. As an instance of how the commercial efficiency can be worked up under pressure, I may mention that during an occasion when the whole of the traffic to and from the East was thrown on the Persian Gulf line, the number of telegrams passed through the cable during the 24 hours was rapidly increased from about 650 to nearly 1,000 daily.

Great Britain supplies the majority and the best of our telegraphists for long submarine cables; in reading the mirror, recorder and manipulative skill, they far surpass the operators of any other nation. In working land-line Morse circuits by "sounder," the Americans are the only people who can compare in rapidity with our countrymen. It is to be regretted we do not see more of our telegraph friends at the meetings of this Institution. Telegraph administrations should, I think, encourage their *employés*, especially those that are to serve at foreign stations, to take every opportunity to render themselves thoroughly proficient, not only in a knowledge of the apparatus in daily use, but also in a knowledge of testing. A clerk is none the worse operator because he happens to be an intelligent electrician. The life at isolated cable stations must often be a dull one; and if the *employé* can be encouraged to devote his spare time to improving his mind by the study of a most fascinating and interesting subject, the result, I am convinced, would be beneficial in many ways, not only to the man, but to the company he serves.

In connection with testing I may be permitted to give a word of advice to young beginners. Use few formulæ—the fewer and simpler the better—but apply them with judgment. Test as soon as you can after the occurrence of a fault; see that your plugs and contacts are clean; and, if you are in a cable ship, keep your head cool. The average distance between the assumed and actual position of faults in the Persian Gulf cable, where the sections vary from 150 to 500 miles in length, is, I find from a recent report for that department, less than one-third of a knot, so that the electrician can localise the breaks closer than the captain can navigate his vessel.

Thirty words per minute is a high speed for a writer, and on average signaller could maintain a greater speed with the mirror key for any length of time. The results obtained from the cables last laid by the Commercial and Anglo-American Telegraph Companies show that it is possible to transmit nearly 50 words of five letters each per minute; this, of course, has to be effected by means of an automatic transmitter. The introduction and universal adoption of this adjunct will no doubt considerably increase the carrying capacity of our cables. An ordinary automatic transmitter will not appreciably increase the speed of working over hand signalling on very long cables where the retardation (or K R) is over 5,000,000; but by the use of the automatic curb transmitter with an adjustable curb commutator, the speed of working has been increased on some of our more important cables at least 30 or 40 per cent.

The invention of the telegraph, and its subsequent development, are closely associated with the reign of Her Most Gracious Majesty. But the work of the telegraph engineer is in one respect incomplete. It still remains for him to establish a girdle round the earth. The question of a Pacific cable has again come to the front. A committee has been sitting at the Colonial Office for the purpose of considering the matter, and I understand the feeling of the committee is favourable to the scheme being carried out. The longest section in the proposed line, including 9 per cent. for slack, would be 3,530 knots. The core would probably consist of about 500 lbs. of copper and 320 lbs. of gutta-percha. In this case the retardation would be nearly 12,000,000; the theoretical speed through such a line with ordinary signalling is about 55 to 60 letters, but through an artificial cable arranged to give this amount of retardation I have recently seen 85 letters per minute transmitted with ease by means of the automatic curb sender. The points which suggest themselves for consideration are: Is the Pacific cable desirable for strategic reasons? Is the scheme feasible? To these questions any statesman or telegraph engineer would, I think, unhesitatingly answer in the affirmative. It would be difficult to over-estimate the strategic value of the Pacific line. At the same time, I do not attach too much importance to the

much-talked-of telegraphic isolation of England which is feared immediately we are involved in warfare with any great European Power. This is held up to us as a sort of "bogey." If our fleet does its duty—and it generally does do its duty—any cable-cutting craft which may adventure into the shallow waters near our shores, or near those of our Colonies, may be expected to meet with an unpleasant reception. On the other hand, the wholesale destruction of cables in deep water will not prove an easy task to the comparative amateur. It takes a certain amount of time to grapple a cable in deep water, even when the exact position is known. The popular imagination pictures the work of destruction as being carried out by 20-knot cruisers in something like the twinkling of an eye. Cable-catching is not to be learned in a day. I do not think the malicious damage to cables in deep water need be greatly feared, and it may be taken for granted that our cable ships would quickly repair the damage inflicted in shallow water, unless the enemy held command of the sea. Nevertheless, the more telegraphic routes we have to our Colonies, the less the probability of their being isolated in the event of war, and the less the chance of panic in time of peace. Nothing more disheartening for the would-be destroyer of cables can be imagined than the knowledge that there are so many routes available for sending messages that the destruction of any one cable would hardly be worth the risk. When our network of telegraphs is complete, with cable ships judiciously placed for repairing purposes, and fast cruisers to watch the more exposed portions of the route, there ought to be little risk of Great Britain being completely isolated from her Colonies in time of war.

There has been a good deal of discussion of late with regard to fast-speed cables; I do not pretend to prophesy, but may we not reasonably hope that we are on the eve of substantial developments in connection with the methods of submarine electrical communications? I am not now speaking so much of improvements such as the duplex or the high-speed automatic curb transmitter, but of fundamental alterations in the structure of cables which we may anticipate will result from the consideration the subject is receiving from our highest authorities. I do not suppose that we

shall see an ideal cable manufactured straight off the reel, or that cable companies will be too ready to incur the risk of acting on the suggestions of inventors. Of course mistakes will be made, but I look with confidence to the day when these efforts shall bear fruit. A day may come when our electricians will design and our engineers construct a cable which will enable us to hold converse by telephone with our transatlantic brethren.

In Mr. Preece's proposed cable, designed for telephone work, and described in a paper read before the British Association, there are two conductors, semicircular in section, in each core, thus providing a complete metallic circuit. The conductors are first separately insulated with brown paper, then arranged with the flat sides pressing closely against each other, with only the brown paper between them. The twin conductor thus formed is insulated with gutta-percha in the usual way; it is claimed that as the conductors are made to approach each other, the electro-magnetic induction increases at a greater rate than that of the electrostatic induction, until at a certain point the one will neutralise the other.

Professor Silvanus Thompson aims at increasing the speed of working on long ocean cables by using a double core, forming a complete metallic circuit, and establishing at intervals permanent inductive leaks or bridges of very high resistance between the two wires. These intermediaries are well insulated, and may be 50 or 100 miles in length; they would be laid in with the double core and connected at their extremities, so as to form a by-pass between the two conductors. In an Atlantic cable three or four of these derived circuits would probably be required. The advantage of introducing leaks at intervals, where practicable, on a long cable is well understood; I have used them with advantage myself where we had intermediate stations available, so that ordinary resistance coils could be used to connect the cable with earth.

Professor Thompson anticipates that with a cable of this type, with several leaks, he would be able to transmit signals across the Atlantic at six or eight times the speed at present obtainable. It must, however, be remembered that in increasing the number of

cores we should considerably increase the cost of the cable; as a matter of fact, we should be putting, say, two eggs into one basket. The cost and telegraphic capacity would, therefore, have to be compared with that of two good modern single-core Atlantic cables.

A third system has been suggested to me by Messrs. Phillips and Barr.

They propose to employ what is practically a double conductor, but the return circuit for each wire, or group of wires, would be the earth. It is intended that the insulation between the two conductors, or group of conductors, should be comparatively low, so that an appreciable amount of leakage would occur from one circuit to the other. By means of a suitable key, one conductor would be simultaneously charged with a similar current, as the other is discharged; and it is claimed that the fall of energy on the one circuit, as it is disconnected from the battery and put to earth, would be balanced by the rise of potential on the other, so that the retarding effects of static induction would be reduced to a minimum.

In quitting the subject of submarine telegraphy for this evening, I would again remark that it has not been my intention to give you a paper embracing the history up to date of this important branch of electrical science. I have, on the contrary, endeavoured to keep off the beaten track; my aim, I confess, being to interest, rather than to weary you with dates and events which have been recorded over and over again.

In lighting, traction, and transmission of power over short distances, electricity has to encounter formidable rivals; perhaps it is as well that it should be so, as wherever two branches of science, still in the stage of vigorous growth, meet, there may we look for striking developments, and all the more so when the two sciences in question have met with important industrial applications. The use of electricity in connection with the various processes pertaining to electro-chemistry has made satisfactory progress during the past year. The results already obtained, and the wide field that presents itself for further operations, suggest that chemistry will in future be one of the most important studies

for the student who wishes to advance to the front rank of the electrical profession. The ease and accuracy with which electrical action can be regulated under its extensive utilisation in chemical and metallurgical processes, suggest great possibilities in the immediate future. The processes for the most part are electrolytic. The production of alkali and chlorine by the decomposition of salt naturally first engages our attention.

Large works are being erected near Widnes for the purpose of carrying out the Castner-Kellner process for the production of caustic soda and chlorine. The Holland process for a similar object is being developed by the Electro-Chemical Company at St. Helens, where, I understand, 4,000 H.P. will shortly be at work.

One of the most successful processes in which electricity is employed is the production of aluminium; this is in operation on a large scale at Pittsburgh, the Falls of Niagara, and also at Foyers, in Scotland.

Large works are in operation in Sweden, Switzerland, and France for the production of chlorate of potash, and further installations are contemplated for the United States.

The decomposition of gold from cyanide solutions is being successfully carried out on an increasing scale in South Africa by the Siemens-Halske process; and this firm has also developed a process for the deposition of zinc from zinc ores, which is likely to come into extensive use.

The researches of Monsieur Moissan have given a great stimulus to the methods which rely on the use of an electric furnace. The processes involve for the most part the formation of new combinations, such, for example, as carborundum, a carbide of silicon, which can be used in place of emery powder. 1,000 H.P. is employed in producing this material at Niagara. Other carbides have been produced on a large scale, more especially carbide of calcium, which by the action of water yields acetylene gas. Phosphorus is also now manufactured on a commercial scale by means of the electric furnace.

Serious attention has been given by chemists to the fascinating problem of obtaining electrical energy direct from carbon. To Professor Ostwald must be given the credit of having, at a

meeting of German electrical engineers, in Leipzig, during 1894, stimulated the activity of the genuine electro-chemist. As a result we have had the Borcher's cell, in which it was thought at first that the electrical energy obtained was derived from the combination of carbon monoxide and oxygen, cuprous chloride acting as the intermediary. Then came the Coehn cell, with an apparently soluble anode of carbon, a cathode of lead peroxide, and sulphuric acid as the electrolyte. A primary battery claiming to produce electrical energy direct from carbon must, like Cæsar's wife, be above suspicion. Although the elements of practical success are wanting in the instances I have mentioned, it is pleasant to bear testimony to the *boná-fide* attempts made to solve this great problem, and it is a matter of congratulation that the investigations are being conducted by men of scientific repute.

In my Address this evening I have chiefly dealt with facts which are beyond question. In my concluding remarks (which I promise you shall be brief) I may refer to matters regarding which there is a divergence of opinion. You will understand, therefore, that I am speaking on my own responsibility. We are all agreed it is imperatively necessary that as an Institution we should keep in the front rank; but, to ensure this result, we must stand shoulder to shoulder, and do what we can to retain the sympathy of all sections of our members, and keep in touch with every branch of our profession. If it is inevitable that from time to time there should spring into existence offshoots from the parent Society, or associations with similar objects, they should, if possible, be brought to act as buttresses, and not partake of the nature of rifts in the foundations of the Institution. I feel sure your Council would be glad to see their way to gratify, as far as possible, the natural aspirations of any section of the Society, and meet the reasonable requirements of all. Possibly, as the basis of the Institution broadens (and I do not see any finality to our sphere of usefulness), the solution of the question will eventually be the formation of local committees and the simultaneous reading of papers, the various discussions on which could afterwards be printed in the Journal of the Institution.

In making a forecast as to our future, I may be permitted to refer to one item in the accounts recently presented at our General Meeting, namely, the amount standing to the credit of the Building Fund. Whatever divergence of opinion may exist as to the advisability of taking early action in the matter, there can, I think, be no question that the provision of a building for the purposes of the Institution is a matter of paramount importance. It is our duty to seriously consider whether the interests of the Institution are not prejudicially affected by our inability to offer to our members, or any subsidiary associations which may be affiliated with us, the convenience of a central place of meeting and research. We have at present a name, but not a habitation of our own.

No one appreciates more than myself the generous treatment we have received from the Institution of Civil Engineers, or the advantages of this magnificent place of meeting; but the time may come, and it assuredly will come, when our frequent occupancy of this hall may not be convenient; and it behoves us as business men, remembering that Rome was not built in a day, to endeavour to make timely provision for such a contingency. We must not shut our eyes to the fact that, notwithstanding our growing importance, we have fewer meetings now than we had eight years ago. If this Institution is to continue to lead the way in electrical science, it must be fed by contributions in the way of papers from its members; and, to do justice to these contributions, it is equally obvious that we must have more meetings at which to discuss them.

Let us look for a moment at the immediate future. First, there is the Pacific cable, the dream of telegraph engineers, who desire to share in the honour of completing the girdle round the earth. Next, there is the establishment of telephonic communication through long submarine lines, and its corollary, fundamental alterations in the form of our future cable. Turning to another branch of the electrical industry, we have two important underground electrical railways in course of construction in the metropolis, and several others the Bills for which will be presented to Parliament during the coming session. London will be

pre-eminent throughout the world in regard to electric railways, as she is in every other respect. The new applications of electricity in connection with electro-chemistry and electro-metallurgy are most important. Who can prophesy as to the next position from which electric science and enterprise will exercise their beneficent influence? The chemist, the electrician, the engineer, ever seeking, ever striving, to discover new natural agents, and to apply them to practical use. To me it seems the outlook is full of promise. The prospect of continued activity in connection with electric lighting, electric traction, and mining machinery is most satisfactory. Electric light mains, lamps, and storage batteries will assuredly receive continued attention. The telephone business is steadily increasing, and the service improving by the adoption of the complete metallic circuit. The developments of the last 15 years are only an earnest of what is to come. Under these circumstances, I think we may enter our second quarter of a century with feelings of hope and confidence; and there is some justification in congratulating the junior members of this Institution upon their choice of a profession which is not only growing and progressive, but full of interest.

The boy is father to the man; the student of to-day is the raw (sometimes very raw) material from which we are manufacturing the electrical engineer of the future. It is to them the fortunes of this Institution will be entrusted. It is not in our power to give the clear head, cool judgment, steady nerve, and the faculty of acting promptly in cases of emergency, which is so invaluable to those who are placed in responsible positions; but it is quite within our province—indeed, it is our duty—to make any suggestion which may be of service to those who intend to adopt electrical engineering as a profession.

I do not know whether my experience is similar to that of others; but I have frequently found it as difficult to find the right man for the appointment as to find the appointment for the man.

Students now command far greater facilities for acquiring a knowledge of their profession than were available 25 years ago, but they should remember that much more is expected from them. The standard of excellence is rising: the training must

be both theoretical and practical; and it is not until some practical experience has been acquired that the young engineer becomes of real value to his employers.

In conclusion, I would remind you that your duty towards the Institution does not altogether cease with the payment of the yearly subscription. Surely few of us have gone through life without learning something in connection with our profession, the knowledge of which would be useful to our members, and which might be imparted either in the form of a paper, or communicated during the discussions which follow the papers of others. Throw diffidence overboard; the proper place for silent members is over the way—in the House of Commons. Try, if you can, to contribute one little pearl of truth to the common stock of knowledge; for if, by the proper use of the talents entrusted to you, you are able to leave the world a little better —a little wiser—than you found it, believe me, gentlemen, you will not have lived in vain.

Mr. CROMPTON: It is my pleasant duty to rise to propose a hearty vote of thanks to Sir Henry Mance, your President, for the interesting Address which he has just delivered to us, and which I am sure you, in common with myself, must feel has added to our stock of knowledge on subjects of supreme interest to the electrical profession. We hear far too little of telegraph engineering in this Institution, and it is a fact much to be regretted. I hail the advent of Sir Henry Mance to the chair as an earnest of better things to come in regard to our papers and stock of knowledge on that branch of electrical engineering of which Englishmen must be so proud—a branch in which we are so pre-eminent, and in which, as Sir Henry Mance has reminded you, up to the present time we have practically held the monopoly of the world. I have now to move—"That the thanks of this "Institution are due to the President for his interesting Address; "and that, with his permission, it be printed and published in "the Journal of the Proceedings of the Institution."

Professor PERRY: I beg to second this proposal that Mr. Crompton has made. When Sir Henry Mance spoke a moment or two ago of the importance of everybody contributing to the

proceedings and giving his experience, I began to think that at our technical colleges we ought to do something more than merely teach our students *Mance's method* of finding the E.M.F. of a cell. In all our text-books we speak of Mance's method—Mance's method for cells; Mance's method of finding faults in cables. But we ought to teach our students how to speak: it is very important that they should learn to express their meaning in public; and I think you will all agree with me, after to-night's experience, that we cannot do better than teach them Sir Henry *Mance's method of speaking.* We have had an exceedingly interesting Address—an Address which gives rise to much thought. Amongst other things, I could not help thinking of the large amount of information these submarine telegraphy people are giving to us about the earth. Deep-sea sounding is a sort of thing that, in all probability, we should hear very little about if it were not for deep-sea telegraphy. For my part, I think that almost the most important thing that a man can do at the present time is to try and get information about this globe on which we live, and about which we only know something in connection with half a mile or so of thickness of its skin. The telegraphist is following the civil engineer. The civil engineer having given the muscular constitution, one may say, to the surface of the earth, the electrician is creating the nervous constitution. There is another most important thing that is perhaps not sufficiently well known. It has always been the necessity for working out physical problems which has given rise to new developments in mathematics. There is a new development of a part of mathematics which is now getting quite easy and well known, and it has been almost altogether developed by the problems that came before physicists and electricians in connection with submarine signalling. A certain philosopher, known to us all by reputation, who lives down in Devonshire, will be exceedingly glad to know from this Address that his notions with regard to the proper construction of deep-sea cables are being recognised by so many inventors. He has worked out for us the relations which ought to hold between capacity, resistance, self-induction, and leakage, in cables intended for rapid signalling and telephony, and his results are

perfectly well known. Surely it ought to be easy enough for the constructive genius of England to carry out his plans. If there is anybody that I envied more than Sir Henry Mance, when those beautiful phenomena came before him when he was laying the cable in the Persian Gulf, it was the solitary operator that he spoke of who was not compelled to read anything but novels, or to think of anything whatsoever. These solitary posts ought to be given solely to overworked Professors of technical colleges.

The resolution was carried by acclamation.

The PRESIDENT: I thank you very much for the kind manner in which you have received this resolution, and also for the kind attention which you gave to me during the delivery of my Address.

I have now to declare, as a result of the ballot, that the following candidates have been duly elected:—

Members:

Sherard Osborn Cowper-Coles.
Howard Dudley Norman.

Associates:

Skidmore Ashby.
Edward Sutton Bradburne.
Alfred Charles Brown.
W. Forbes Bruce.
Ernest R. Carr.
G. A. Cowles.
Andrew Fraser.
J. R. Woodruffe Gardam
Ernest T. Gosling.
David McDowall Grosart.
Patrick Hamilton, B.Sc.
Harry Hewlett.
Lawrence John Lawrence.
George Henry Llewellyn.
Cecil E. Lugard.
Gustavus McAlpine.
George E. Penboss.
Robert Grosvenor Perry.
Herbert James Read.
John Roberts.
Theodore Stevens.
Denner John Strutt.
G. E. V. Thomas.
Sydney Woodfield.
John Wrigley.
Merrik R. A. Wyatt-Edgell.

Students:

W. E. Barker.
Francis James Benton.
Thomas Bowden.
William Walter Bradfield.
William Leonard Carter.
Charles James Cunningham.
Harry Melville Dowsett.
Cyril Charles Toulmin Eastgate.
Charles Samuel Franklin.
Mostyn Randall Gardner.
Arthur Frederick Malyon Gatrill.
Edward Treloar Gifford.
Arthur Harry Greenwood.
Benjamin Handley.
Frank John Hawkins.
Alfred James Hersant.
Bernard Hopps.
Frank Knight Jewson.
John Kingston.
John Hayes McDowell.
Robert Valentine Macrory.
Charles William Messenger.
Christopher Oscar Milton.
John Leslie Morris.
George Morrison.
Frank Powell.
Edward Alfred Tunbridge.
Wilfrid H. Ward.
Andrew Wilkes.
Peter Remus Wray.

The Two Hundred and Ninety-fifth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, January 28th, 1897—Sir HENRY MANCE, C.I.E., President, in the Chair.

The minutes of the Annual Meeting, held on January 14th, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been made by the Council:—

From the class of Students to that of Associates—

Robert Birkett.	Henry Edward Mordaunt Kensit.
T. Dawson Clothier.	John T. Redmayne.
Kenelm Edgcumbe.	Charles Percy Taylor.
Sydney Edwd. Thacker Ewing.	L. C. Trevor-Roper.

Mr. T. Rooke and Mr. W. H. Winter were appointed scrutineers of the ballot for new members.

Donations to the Library were announced as having been received since the last meeting from Mr. W. T. Ansell and Messrs. Spon, Ltd., to whom the thanks of the meeting were duly accorded.

The PRESIDENT: I have to announce, with much regret, that since our last meeting we have lost one of our oldest members—Mr. John Aylmer. He had recently, owing to ill-health, been obliged to resign his appointment as Honorary-Secretary and Treasurer to the Institution in Paris—a post which he had held for The President.

The President. many years. He was a most popular member, and of very great assistance to our English members at the Exhibitions held in Paris. I may mention that the Secretary has been instructed to write a letter of sympathy to his widow—a course of which I am sure you will all approve.

Agreed to.

The PRESIDENT: The Secretary has an announcement to make with regard to the Willans Premium. This premium is to be awarded alternately by this Institution and the Institution of Mechanical Engineers every three years. About 10 or 11 guineas will, I think, be available at the end of 1897, and it is the intention of the Council to add to that sum so as to make it up to 25 guineas, which premium will be awarded for the best paper which has been given during the three years ending in December next. I may also mention that the Council have under consideration a proposal to considerably augment the premiums which are given annually by the Institution, and as soon as they come to a decision the particulars will be announced. I have thought it right to mention this matter, so that those who are thinking of sending in papers may submit them as soon as possible.

The Secretary. The SECRETARY made the following announcement:—

WILLANS PREMIUM.

In their Annual Report on December 13th, 1894, the Council announced that a fund had been raised to commemorate the memory of the late Mr. P. W. Willans in connection with that branch of engineering in which he had so greatly distinguished himself, and that this Institution and the Institution of Mechanical Engineers had been requested, and had consented, to become joint trustees of the fund, the interest on which it was proposed to devote to the establishment of a premium to be called the "Willans Premium," and to be awarded alternately by the Councils of the two Institutions every third year.

Accordingly, a Declaration of Trust was executed in January, 1895, by the two Institutions, and the fund, amounting to £165, has been invested in India 3 per cent. Stock.

The following are the chief conditions of the Trust:— The Secretary

WILLANS PREMIUM FUND.

Extract from Declaration of Trust by the Institution of Mechanical Engineers and the Institution of Electrical Engineers, dated 16th January, 1895.

3. The Willans Premium shall be awarded alternately by the Council of each of the above-mentioned Institutions, and first by the Institution of Electrical Engineers in December, One thousand eight hundred and ninety-seven.

4. The Council of the awarding Institution in each alternate period shall award the Willans Premium for the best original paper communicated to their Institution dealing with such a general subject as the utilisation or transformation of energy treated especially from the point of view of efficiency or economy; provided that the premium shall not be awarded unless a paper of sufficient merit in the judgment of the awarding Council shall have been so communicated since the preceding award of that Council.

5. The premium shall be awarded triennially in and after December, One thousand eight hundred and ninety-seven, unless otherwise determined by resolution of the respective Councils of the two Institutions.

6. The premium may be awarded either in money or books or medal, or in any other form which in the instance of any individual award the awarding Council may then determine.

7. In case of no award at the end of any triennial period, the premium available for that award shall be added to the capital of the fund.

Prof. PERRY: I understand that the prize is not confined merely to steam engines and mechanical and electrical power? It may be for any paper, I suppose, read before the Institution?

The SECRETARY: No, I do not think so. The Trust Deed, at all events, defines the terms.

Prof. PERRY: I think it is beyond the wit of man to read a paper on our subject of electricity which does not deal with energy and efficiency.

The PRESIDENT: A copy of the conditions will be circulated for the information of the members.

I have now to call upon Mr. Hollins to read his paper.

ELECTRICALLY INTERLOCKING THE BLOCK AND MECHANICAL SIGNALS ON RAILWAYS.

By F. T. HOLLINS, Member.

So far as I am aware, there has not been a paper read before the Institution for a number of years, (if at all,) dealing with the Mr. Hollins

Mr. Hollins. general question of block signalling and electric interlocking on railways.

This fact, and the importance of the subject; the improvements in the systems in use; the various arrangements for special purposes; the constant extensions of the application of electricity for expediting, and more safely controlling the traffic on our railways, is, I trust, a sufficient apology for bringing the subject forward on the present occasion.

It is obviously impossible, within the limits of this paper, to give minute and detailed descriptions of *all* systems of block and interlocking apparatus, but, so far as I am in a position to do so, I will *briefly* describe those systems of electric interlocking in practical use on other railways, and deal more in detail with the system, (Sykes's,) of which, being responsible for its adoption, construction, and maintenance, on the Great Eastern Railway, I am naturally in a better position to speak with some authority.

I trust that those members who are more closely associated with other systems, will give the Institution the benefit of their better knowledge and experience, by adding such details, and other information during the discussion, as may seem to them to bring out the advantages, or otherwise, of such systems.

I take this opportunity of heartily thanking my colleagues (the Railway Telegraph Engineers) who have been good enough to afford me so much information with regard to these systems.

A little prefatory explanation may be desirable for members unacquainted with railway work.

For the purpose of controlling the traffic passing over a railway, it is necessary to divide up the line into short sections, varying in length in proportion to the traffic, the point of division being indicated by the provision of a signal box, having fixed outdoor signals. The signals are usually a distant signal and a home signal, with, at busier places, a starting signal, and, at very busy posts, an advanced starting signal, for each road; and for sidings, junctions, and cross-over roads there are additional signals for the protection of these connections. These are manipulated by the signalman, so as to indicate to the drivers of trains if they may pass the point where such signals are fixed.

The distant signal is merely a caution signal to advise the driver, (if it is at danger,) that the next signal, that is, the home signal, (which is a stop signal,) may be also at danger, and that he must be prepared to stop; but if the distant is "off," he knows that under ordinary circumstances the stop signal is also "off." The mechanical locking ensures this. Mr. Hollins.

Prior to the invention of the telegraph, (and, I am afraid, for some time afterwards,) the running of trains was merely regulated by a time interval. Later on, a system of electric bells was established, to indicate when a train might be allowed to enter a section, and when it had passed through that section.

In 1840, an arrangement of electric signalling of trains was first used on the opening of the Blackwall line, between Minories and Blackwall; and at about the same time a needle block was made use of on the Midland Railway through Clay Cross tunnel. In 1844 a slightly different arrangement was brought into use on what is now the Great Eastern Railway, (each instrument showing all the sections for the whole line,) between Norwich and Yarmouth; and in 1853 a perhaps more efficient system of needle block and bell was introduced by Mr. Edwin Clark on the London and North Western Railway.

The term "electric block signalling" is generally applied to a combination of electrical apparatus, which, by means of an acoustic and visual signal, gives such indications to the signalman, as enables him, by a proper manipulation of his outdoor signals, to prevent two or more trains getting into one block section upon the same line of rails at the same time.

The block systems in general use may, practically, be divided into two separate groups—those operated by, and the indicators held in position by, a constant current on the line, (the single-needle, semaphore, or disc instrument,) to either indicate, in accordance with the polarity of the current, "Line clear" or "Train "on line," and normally, (by gravity,) "Line blocked;" and those systems, (principally Tyer's,) the apparatus of which is operated to indicate "Line clear," or "Train on line," by momentary currents, and the indicators of which are held in that position by the residual magnetism remaining after the cessation of the current

Mr. Hollins. which actuates the needles and reverses the polarity of the magnets.

There are several variations of both systems; but for the constant current, three line wires, (one for the up line, one for the down line, and one for the bells), are generally made use of; and for Tyer's apparatus only one wire is required for the up and down line, and the bell communication also.

The term "electric interlocking," (that is, interlocking the outdoor signals with the indoor block signals,) is generally applied to a combination of electrical and mechanical apparatus, which *compels* the signalman to work his outdoor signals absolutely in accordance with his electrical block signals, (both of which are electrically under the control of the signalman at the box in advance), and with the most complete systems so controlling, by means of a rail contact, the signalman at the box in advance, that neither the block apparatus nor the outdoor signals at the rear box can be operated to admit a following train until the preceding train is absolutely clear of the section.

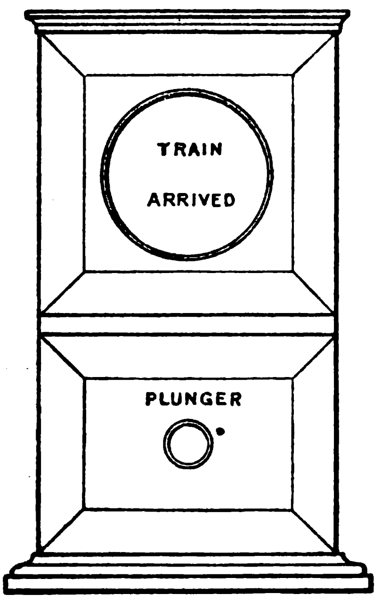


FIG. 1.

Next to the Sykes system there is no doubt Mr. Spagnoletti's has been the most largely adopted. It is in use throughout the Metropolitan Railway; at perhaps a dozen important places on the Great Western Railway; at the junctions of the Great Northern and Midland Railways with the Metropolitan Railway; and several other points.

SPAGNOLETTI'S ELECTRIC INTERLOCKING.

The transmitting or accepting instrument is provided with a plunger, two movable discs, and one fixed disc, together with an electro-magnet. The outward appearance of the apparatus is shown in Fig. 1; and Fig. 2 is the lock indicator.

The three indicator discs in the transmitting instrument respectively indicate "Train arrived," (a fixed disc), "Line clear Mr. Hollins.

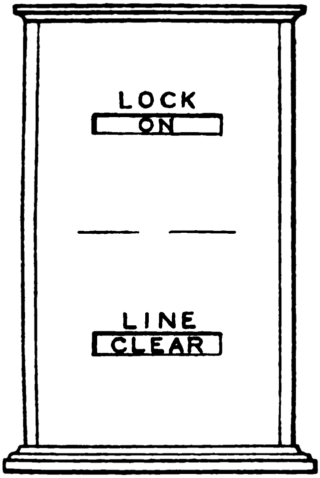


FIG. 2.

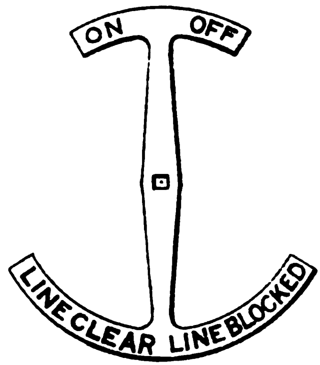


FIG. 3.

"sent," and "Train on line coming." The lock-indicating instrument has two discs carried upon the needle of an ordinary single-needle coil, as shown in Fig 3.

The indications are "Lock on," and "Line blocked," given by gravity, and "Lock off," and "Line clear," by a current, and are made to appear before the two apertures in the screen, in accordance with the position of the lock. The coil giving this indication is merely in a local circuit of which the lock acts as a relay. Whilst the lever is in the "off" position, a train cannot be accepted; and at junctions of conflicting roads of any kind, it is arranged that, having plunged to accept a train from one road, it is impossible to plunge to accept one from the other, until the train accepted has arrived and the signal is put to danger. The operation is this: "A" has a train for "B," and gives the "Is "line clear?" signal on the bell. "B" responds, (if the line is clear,) by plunging on his accepting instrument (Fig. 1), and his plunger is locked, giving to himself the indication, mechanically, (but held by the current electrically), "Line clear sent." This takes out the lock of the section signal at "A," and indicates "Lock off" and "Line clear." When the signal has been taken off, the train passes

Mr. Hollins. into section A B, and, after clearing the signal, it actuates a rail contact, which indicates on "A's" instrument "Train arrived," unlocks his plunger for the rear section, and shows "Train on "line coming" at "B" by interrupting the current on the line, and causing the "Line clear sent" disc to rise from before it. On the train arriving at "B," and passing over his rail contact into section B C, the "Train arrived" disc is disclosed—the "Train on "line coming" disc having been released, and moved by gravity from before it by the same current and coils that unlock the plunger to allow a following train to be accepted. The electric lock is really upon the trigger of the lever, and not upon the lever, or a tappet of the lever, itself. The lock therefore controls the trigger, and the trigger controls the lever.

The system described, I am told, is not quite the same as that on the Metropolitan, or the Great Western Railway, but the arrangement described is, as it is recommended by Mr. Spagnoletti, for lines to be fitted with entirely new apparatus. The arrangement at present in use has been adapted to the existing form of block.

If I might presume to suggest, I should say that the coils for giving the block indications "Line clear" and "Line "blocked" ought to be in the line circuit, and quite independent of the indications of the "lock," for several reasons.

Langdon's Electric Interlocking.

On the Midland Railway Mr. Langdon has in work, I believe, over some 12 or 14 block sections, an arrangement of electric interlocking which was devised by him for use in combination with the single-needle block apparatus, so that the system of block working should remain unaltered.

Instead of the usual pin, or locking trigger, to peg over the handle to "Line clear," or "Line blocked," a small pedal, on either side of the handle, is provided. To give "Line clear," the pedal, to hold the handle over in the right direction, switches in an additional battery, sufficient to operate the lock of the controlling signal in the rear, as well as the needle of the block instrument—the two coils being in series. On the "Train entering section"

Mr. Hollins.

signal being given, the pedal, pressed down to hold the handle to "Line clear," is released, and the opposite pedal depressed, to lock over the handle to give "Line blocked" to the rear. In pressing this pedal down, the handle, or commutator, is locked over into this position, and can only be released by the action of an electro-magnet, which is in circuit with a rail contact fixed a suitable distance ahead of the controlling signal for the section in advance. The transmitting instrument therefore remains locked up, giving the "Line blocked" indication, until the train actually arrives and passes over a Siemens mercury rail contact. The signal must be put to danger behind the train (when it is automatically relocked), or the circuit arrangements in the instrument remain such that "Line clear" cannot again be given.

The electric lock consists of a sliding bar or tappet fixed in a slot, and suitably attached to the tail-piece of the signal lever. A notch across the sliding bar and the slot serves to receive a small steel bolt, (engaging in both bar and slot,) carried by a light lever. At the opposite end of this is an armature to be operated by a non-induced electro-magnet, suitably wound so that the ordinary current of four cells used for the block indications alone will not operate it; the additional battery switched in by the "Line clear" pegging pedal being necessary.

Evans's Electric Interlocking.

On the Manchester, Sheffield, and Lincolnshire Railway there is a modified system of electric interlocking on trial, which is designed to allow of either "absolute" or "permissive" block working. The starting signals are duplicated (both locked), one for the "absolute" and the other for the "permissive," to indicate to the driver if he may proceed under the permission of "absolute" or "permissive" block working. Either signal, as required, can be worked by the same lever.

Constant-current needle instruments are employed, and a switch is turned which indicates to the operator whether a train has been accepted from the rear on the "permissive" or "absolute" block system. This switch controls the working, and when it is

Mr. Hollins. in one position the "absolute" signal only is released; when in the reverse position, the "permissive" signal only is released.

The switch in the instrument at the box in advance being turned to either "permissive" or "absolute," and the handle being pegged to indicate "Line clear," a current is transmitted to the rear box which, by means of a relay in the line, and three pairs of electro-magnets acting as an electrical selector and lock, releases either one or other of the signals as required. The man in advance lowers his home signal; the train arrives, and is protected by that signal; and the back motion of the lever locks up the block instrument, and is itself locked until the starting signal has been lowered for the train to proceed, and is again put to danger. Instead of a rail contact, by which the train alone can free and clear the section in its rear, the back motion of the starting signal lever is the final motion permitting another train to be accepted up to that signal. Under ordinary arrangements, so long as the "Line clear" signal is appearing, the lock would stand released, but a special device is provided to break down the lock circuit immediately it is unlocked, and it is only joined up when "Line blocked" is given from the station in advance. This ensures the signal only being used once with the one "Line clear" current, and compels the sending of "Train on line"—an excellent arrangement simply effected.

On the Caledonian Railway a modified system of electric interlocking has been adopted over about 50 sections of their new City and Suburban lines in connection with the ordinary Tyer's three-plunger block instrument, with the addition of a plunger lock, a polarised relay, an electric lock on the starting signal, a Sykes's signal reverser (see Figs. 22, 23, and 24), and a rail contact.

The "Line clear" plunger can only be used once to free the section signal in the rear when it becomes electrically locked, and this lock can only be taken off when the train arrives and passes over the rail contact in the section in advance. The same rail contact that clears the "Line clear" plunger, to accept another train from the rear box, also, by means of the Sykes's signal reverser, puts the signal to danger behind the train as it enters the next section. This apparatus was designed, I understand, so as to

allow of the old system of block working being strictly adhered to. Mr. Hollins
The method of dealing with sidings, junctions, and cross-over roads would, I am sure, be interesting, and I invite Mr. Dunn, the Company's Telegraph Engineer, to give us some information on this point.

Tyer's Electric Interlocking.

Mr. Tyer has completed a combination of block and electric interlocking which is, (as might have been expected,) of considerable merit. The outward appearance of the best form of his old block is retained, and there is every indication of all the different motions that could be desired, using a signal reverser to automatically put the signal to danger, and a rail contact to clear the rear section by unlocking the "Line clear" plunger. The lock is on the trigger of the signal lever, and is a very neat and simple arrangement. The signalman in advance may very effectually block back after accepting a train in case of an emergency, and even after the signal at the rear box is off; in fact, he may give "Line blocked" and put the signal to danger too.

Blakey & O'Donnell's Electric Interlocking.

On the Great Northern Railway, Blakey & O'Donnell's electric lock and block is on trial over, I believe, two sections near London. The apparatus, which is as under, seems to be mostly "Sykes's," applied to the single-needle block. There is a Sykes's signal reverser, or replacer, normally discharged, so that the rod does not engage with the arm until the current is received; a lever contact for closing the circuit to the reverser; a polarised relay in circuit, with the block instrument at the rear end of the section; a relay in connection with the block instrument at the advance end of the section; and Sykes's rail contacts or treadles.

Diagram Fig. 4 shows the connections.

When the advance station, "B," pegs the block instrument to "Line clear," this actuates the block needle, 2, and the polarised relay, 3, at "A," and partially completes a local circuit through the signal reverser, 4, on the starting signal. The signalman at "A" in pulling the signal lever joins up the

Mr. Hollins. lever contact, 5. This completes the local circuit; the reverser is geared, and the signal arm, 6, lowered. It will be seen that

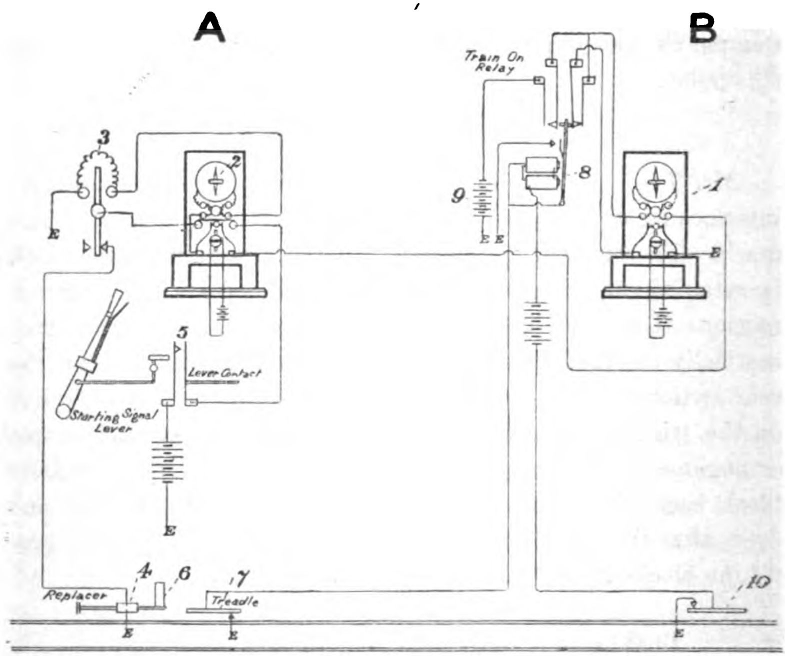


Fig. 4.

without this current the lever, if pulled, would not lower the signal. The train passing into the section actuates the rail contact or treadle, 7, and the relay, 8, attached to the block instrument at "B," disconnects the commutator of the block instrument, and joins up the battery, 9, which brings the needles of both block instruments to "Train on line." This reverses the tongue of the polarised relay, 3, at "A," and the local circuit being disconnected the signal reverser puts the starting signal to danger behind the train. This state of things remains until the train reaches the second rail contact or treadle, 10, at "B," which, momentarily disconnecting the circuit of the relay at "B," restores the block needles to zero, and the apparatus is ready for accepting another train from "A."

The Great Northern Railway Company have also, at King's Cross tunnel, an interesting arrangement of electric interlocking.

utilising the rails as conductors, in such a manner as to ensure that any engine with its train occupying the line shall be fully protected by the signals, and such signals shall not be capable of being lowered until the line is clear. (There is a somewhat similar arrangement to this, minus the signal reverser, on the London, Chatham, and Dover Railway, at St. Paul's.) Mr. Hollins.

Diagram Fig. 5 (Plate 1) shows the arrangement. Sykes's signal reversers are here again made use of. At King's Cross West the lever working the "main," and the "main to goods," advance signals, (either signals worked with one lever as geared,) is normally electrically locked, such lock being connected with a polarised relay attached to the block instrument. The signals named are fitted with Sykes's reverser, actuated by commutators at the north end of the tunnel. The rails of the down main line through the tunnel are insulated, as a separate section from the adjoining rails, at each end, and at the north end both rails are in circuit with a low resistance and low E.M.F. battery, as shown; and at the south end the circuit is completed by a relay, the armature of which forms the earth connection of the lever lock and signal reverser.

Assuming the down main line to be clear through the tunnel, when King's Cross West gives "Be ready" to Belle Isle down (the signal box at the north end of the tunnel), the latter pegs his block instrument to "Line clear;" the polarised relay attached to the block instrument at King's Cross West is actuated, and the lever lock released. Belle Isle next either lowers the "down main" or "main to goods" home signal, and the commutator on such signal then earths one pole of the battery. The centre of this battery is connected by wire through the tunnel to a polarised relay in connection with the electric slots on King's Cross West down advance signal, one of which is actuated, and admits of the corresponding arm being lowered. On a down train passing into the tunnel, the wheels and axles of the engine short-circuit the battery connected to the rails; the armature of the relay in connection therewith at King's Cross West, being no longer held by the current, moves from the poles of the electro-magnet, and thus disconnects the

Mr. Hollins. lever lock and signal reverser circuits, causing the signal arm to go to danger, and the lever lock to fall in position to lock the lever when put back. The contact spring in connection with the signal lever disconnects the lever lock circuit when the lever is pulled over. So long as the engine, therefore, remains on the section of line shown, the signal is locked in the danger position to protect it.

SINGLE LINES.

The traffic capacity of single lines of railway has been so largely increased, and the otherwise unavoidable doubling of such lines for some long time deferred, by the adoption of Tyer's tablet system, and the Webb and Thompson electric train staff, (which is the same thing in another form, but, so far as I can see, minus the block), together with the arrangement for interlocking the same with the signals, that this apparatus is well worth consideration.

Formerly, each section of a line had its exclusive "Train "Staff," which, delivered into the hands of a driver, was, except the ticket for the *same* direction, the sole authority for entering that section of line. If several trains were known, however, to be passing consecutively in one direction, the person holding the staff could obtain from a box, locked up by the staff, a ticket to give to following trains, and the staff to the last. If the staff happened to be at the opposite end to a train requiring to pass through, there was nothing for it but to send for the staff, and wait.

Tyer's tablet instrument is, so to speak, a combination of the block telegraph and the train staff and ticket, the last two being combined as a metal tablet, controlled by, and its use controlling, the block, the indications of which are given at both ends of the section. A tablet once obtained, the block indications cannot be altered until it is restored to one or other of the instruments controlling the section. Although there may be as many as 30 tablets for the section (half at each end), only one tablet can be obtained at one time; and this requires the concurrent action of both signalmen. But, as a tablet can be obtained at either one end or the other so long as all are in the instruments, there is no

avoidable delay in passing trains through the section. The capacity of the line to carry traffic is then only restricted by the length of the section, and the speed of the trains. Mr. Hollins.

Having effectually, and safely, combined the staff, or tablet, always ready at either end of the section, with the block telegraph, it only remained, in order to give the maximum of safety, to electrically interlock this apparatus with the outdoor signals controlling the entrance to the section at both ends to ensure that they could never be contrary the one to the other, and so that the starting signals at each end could never both be off at the same time.

The Tyer's No. 6 tablet instrument is too well known to need description or illustration. Suffice it to say, before obtaining a tablet for a train, the usual "Is line clear?" signal is given on the bell, to which, if clear, the signalman at the other end replies by pressing in the plunger of his tablet instrument to release the

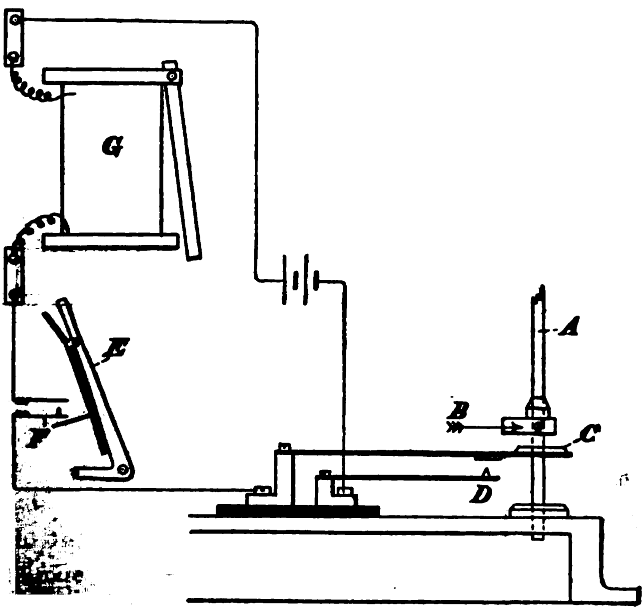


Fig. 6.

opposite end commutator, and the signalman offering the train turns the commutator of his instrument, which also unlocks the

Mr. Hollins. drawer, and he obtains the tablet. It is the turning of this commutator in the position to obtain a tablet which also releases, by a momentary current, the Sykes's lock on the starting signal. This contact (it is a knuckle-jointed contact) is not made by the back motion of the commutator, but only by the front motion, to obtain the tablet.

Mr. Tyer, however, with his signal lever lock, prefers a constant current, with a key (it may be a lever trigger contact) to close the circuit when the lever is wanted. Fig. 6 shows this. The act of turning the commutator to obtain a tablet causes the rod, A, to be pressed down, and the pin, B, then presses into contact battery springs, C, D, which, when the trigger of the signal lever, E, is pressed to pull over the lever, joins the battery by springs, F, to the lock, G, and thus releases the signal.

As a tablet can only be obtained at one end, the starting signal can only be taken off at that end of the section where the tablet is obtained.

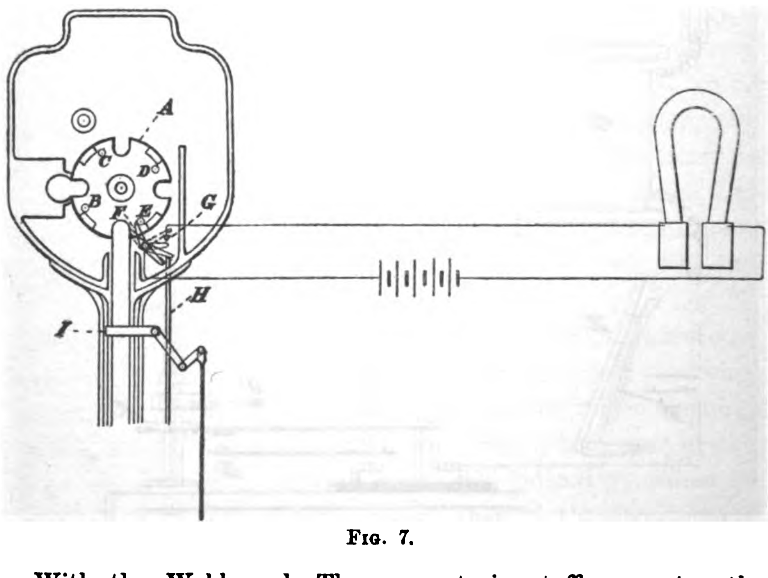


FIG. 7.

With the Webb and Thompson train staff apparatus the locking is effected by a Sykes locking instrument, applied as shown in Fig. 7. The upper portion of the staff instrument only is shown. The disc, A, is moved round a notch each time a staff

is obtained. The small pins, B, C, D, E, on the disc A, are for the purpose of engaging with the small contact lever, F, as the disc is moved round from right to left in getting out the staff; and this lever F then makes contact with G, closing the local circuit, operating the Sykes instrument, and thus unlocking the lever. Putting a staff into the instrument turns the disc from left to right, and this moves the small contact lever F in the opposite direction without making contact with G. When, however, the lock has been discharged, and the lever is free, the staff cannot be put in, because the rod, H, would prevent the

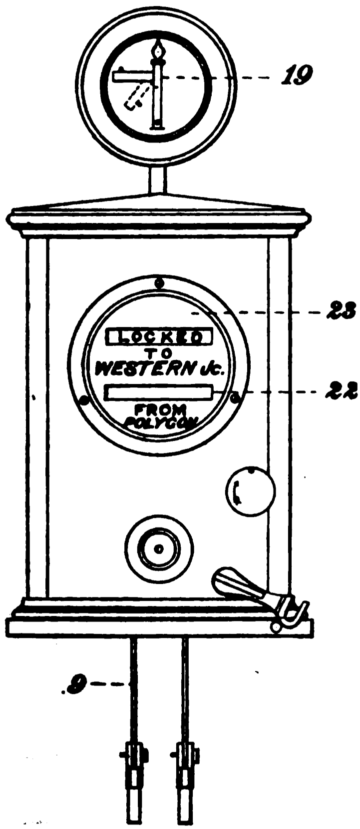


FIG. 8.

small contact lever F being moved by one of the pins, B, C, D, E, on the disc A. If the starting signal were left off, a staff

Mr. Hollins. could not be obtained, as the bolt, I, geared to the signal lever, blocks the passage of the slot in the staff instrument so long as the signal is in the "off" position.

Sykes's Electric Interlocking.

I now come to the description of the system which the Directors of the Great Eastern Railway, (recognising the rapid growth of their enormous suburban traffic, and being determined to afford every protection that the combination of electrical and mechanical apparatus can give, to safely and expeditiously deal with this traffic,) have thought fit to adopt, at a cost of little less than £25,000, over practically the whole of their suburban lines.

Sykes's complete system of double line lock and block requires three line wires, in addition to the wire for the treadle, or rail contact, circuit.

Fig. 8, and Figs. 10, 11, and 12 (Plate 1), show the instrument in the form used upon the Great Eastern Railway for ordinary through working. There are three strong rods (see Fig. 9, Plate 1) connecting the locking instrument with the signal lever. One is the actual locking rod; the second is the switch rod, which, by the motions of the lever, connects, in one position (signal "On"), the *line wire* to the locking coils; and, in the other (signal "Off"), the *rail contact wire* to the locking coils, and puts the line direct to earth; whilst the third is the rod which actuates the "Train "accepted," and "Train on" discs, and prevents plunging to accept another train until the signal lever has been taken off and again put to danger.

Fig. 9 (Plate 1) shows diagrammatically the connections between the instrument and the lever, so that the effect of the different motions may be made clear. The electrical connections, minus the semaphore coil, are also shown, to better illustrate the method of operation.

The locking instrument coils, 1, are fixed upon soft iron prolongations of a powerful compound permanent magnet, 2, and the armature, 3, in the "locked" position (as shown) is held up to the cores of the coils by magnetic attraction. The coils are

Mr. Hollins.

wound, or the circuit joined up, in such a direction that a strong current from the line neutralises the magnetism induced by the permanent magnet, and the armature, 3 (which forms a portion of an angle piece, 4, with its axis at 5), pressed away from the cores by a strong adjustable "push-off" spring, 6, is discharged, and the small wheel, 7, on the opposite arm of the angle piece, 4, sliding from underneath the catch-piece, 8 (attached to the locking rod, 9), allows the rod to fall, and in doing so it raises the lock, 10, out of the slot, 11, and thus releases the signal lever, 12.

With this diagram, Fig. 9 (Plate 1), all the operations may be followed. We will assume that the lever, and the locking instrument (which is really the receiving portion of the apparatus) are at station "A;" and the plunger, 13, battery spring, 14, switch hook, 15, battery, 16, and "Train on" apparatus, 17 (in a similar complete instrument), are at station "B," the rail contact, 18, being a train's length ahead of the starting signal at "A."

"A" has a train, and gives the "Is line clear?" signal to station "B" on the block bell. If the section is clear, and his starting, or section, signal has been put to danger (he cannot do so without), "B" plunges, indicating to himself "Train accepted" by the disc, 21, at the lower aperture, 22, in the screen, 23, and his plunger, 13, becomes locked. This plunge joins the battery, 16, to the line, but without breaking down the "Train on" circuit, the latter, during the act of plunging, getting earth from the stop, 17A. The current transmitted releases the lock, 10, at "A" as explained, and indicates by a disc, 18, carried on the rod, 9, "Free." The signalman at "A" then pulls over lever, 12, and lowers his starting signal, and this operation has the effect of raising the locking rod, 9, lowering the switch rod, 20 (Figs. 9 and 12, Plate 1), connecting the rail contact circuit to the locking coils, 1, and the line to earth direct. At the same time the lock, 10, has again dropped into another slot, 21, in the tappet, 22, of the lever, back-locking the same in the "off" position, and indicating "Locked" on the instrument.

I may here explain that on the Great Eastern Railway we have so arranged this slot in the tappet that, although the lever cannot be put right back, so as to allow of another train being

Mr. Hollins. accepted, it can be put sufficiently back to throw the signal to danger in case of emergency.

On the train leaving "A," "Train entering section" is given on the bell to "B," and on its passing over the rail contact, 18, the circuit is closed and the back lock is released, the signal put to danger and relocked, and the *line* wire again connected to the locking coils, 1, the rail contact circuit being left disconnected. In response to the "Train entering section" signal, "B" turns the switch hook, 15, which, breaking the line momentarily (by separating the two springs, 14 and 14A, and thus, coil, 17, releasing the armature and allowing disc, H, to fall in front of the "Train "accepted" disc, 21), indicates to himself "Train on," and to "A" "Line blocked," by raising the small semaphore arm. Should, however, "A" have omitted to give "Train entering section" to "B," the act of putting his signal to danger behind the train at "A," gives "Train on" to "B," through the tripping piece, 24, on the switch rod, 20, pressing against the spring, 25, in passing it, and thus momentarily breaking the line circuit.

After the train has passed "B" and the signal has been put to danger, the switch hook, 15, is turned back off the plunger, 13, and the putting back of the signal lever, 12, again raises the rod, 27, carrying the "Train accepted" disc, 21 (see Fig. 11, Plate 1), and the "Train on" disc, H, is also raised, by the little rod, 28, on the top of rod, 27, (Fig. 9, Plate 1); the "Train on" disc is afterwards held up electro-magnetically by the coils, 17, and the small battery, 26.

The Sykes apparatus was originally joined up to transmit a positive current to unlock the lever; but, in view of *possible* lightning effects, I have recently had this reversed, so that a strong negative current unlocks the lever, and a constant positive current is required to indicate "Line clear." A constant negative current such as might be due to a contact, if strong enough to unlock the instrument, would indicate "Line blocked." Lightning currents are, I think, generally positive currents, and, therefore, in the wrong direction for unlocking the instrument when thus joined up. If, however, a momentary current, such as might be due to contact, or possibly lightning, *should* unlock the

instrument, it would only alter the *block* indication momentarily, Mr. Hollins. and this would immediately resume its previous indication. A disconnection of the line, or the line to earth, would indicate "Line blocked," and of course leave the lock unaltered. If, whilst the line is indicating "Line clear," and after the lock has been released at the box in the rear, and the instrument in advance indicates "Train accepted," the line wire was disconnected or put to earth, it would alter the indications of the instrument to "Line blocked" at the rear box.

It will be seen that, although these are merely minor points, they are all in the direction of safety.

Now it must be obvious that if, in putting the lever back in the frame, the armature is not lifted close up to the poles of the magnet, (and the small wheel, 7, under the catch-piece, 8, of rod, 9,) the rod will fall, and the lever be unlocked. This is a somewhat rare fault; but, in order to prevent it, the rod must be adjusted to give a certain overlift so that the wheel goes completely underneath the catch, 8, and the armature be held tightly up to the magnet by the lifting piece, 29, which is provided with a buffer spring to prevent any violent impact of the armature on the cores of the coils. If this overlift was *always* given such a fault would never occur.

But I was not satisfied with this, and I have had all the instruments fitted with a simple device on the locking rod, 9, *compelling* this overlift, by making it impossible to plunge to accept a train unless not only the signal is at danger, but locked in that position. If, therefore, it falls to "Free" when the lever is put back, the signalman is compelled to raise the rod with his hand, and so lock it. It was pointed out that the signalman could then, if he chose, by pressing the plunger whilst in the act of raising the rod, plunge without being locked. I should hardly think he would try to do this; still, a small cavity was then cut in the plunging piece, to so coincide with this safety device that this cannot now be done.

Fig. 12A (Plate 1) shows diagrammatically how this is effected, and a specimen is on the table. A is the locking blade attached to rod, 9 (Figs. 10 and 11, Plate 1), held up by the small wheel,

Mr. Hollins. 7, engaging under the catch-piece, 8, at the same time that the armature is held up by the permanent magnet. To the locking blade, A, is attached a metallic friction piece, B, binding against, A, but free to move independently to the extent allowed by slot, C. The friction piece, B, has a projection, D, to engage in the cavity, E, of the angle piece, F, which is part of the plunger for transmitting the current to unlock the controlling signal at the box in the rear.

Now, as shown in Fig. 12A (Plate 1), the instrument is in the locked position, and the angle piece, F, coinciding with the cavity between, D, and the upper portion of a slot, G, in the locking blade, A, is free to allow the plunger to be operated—that is, so far as the locking blade is concerned. If now the lock is discharged, the blade, A, and the friction piece, B, fall into the position shown in Fig. 12B (Plate 1); when they are again raised to the locked position, the overlift is given as shown in Fig. 12C (Plate 1), where, D, is shown engaged in cavity, E, of the angle piece, F, and the catch-piece, 8, is raised a corresponding distance above the wheel (which is also the measure of the overlift), and is a little beyond the point where the armature "clicks" on the pole-piece of the magnet. Blade, A, is then allowed to fall so that catch-piece, 8, again rests upon the wheel, and the projection, D, of the friction piece, B, is then clear of the plunger angle piece, F, as shown in Fig. 12A (Plate 1). If the cavity, E, were not cut in angle piece, F, the upper portion of, D, would engage with the bottom of the angle piece when the plunger was pressed in by the hand, and thus, by wriggling the plunger and the rod, plunging would be possible whilst the lever was still unlocked.

The Rail Contact.

In any complete system of electric interlocking there can be no doubt that the rail contact, or treadle, is the most important factor to be considered, because upon its reliability the safety of the system mainly depends. It is the means that shall indicate the presence of the train clear of the section to which it refers, and is really the positive permission to the signalman to admit a

following train into the section. It is essential that a rail contact should only be actuated by the weight of an engine or carriage. It will not do to be merely actuated by the flange of a wheel. It must not be capable of being actuated by a trolley, or being walked over, or jumped on, or by a bar, or other reasonable weight, falling upon it. It must not be capable of being operated by hand, or other means but the deflection of the rail. The battery must be fixed near the contact, *and not at the signal box*, so that any failure of the circuit must be, as the Government inspector remarked at a certain inquiry, in the direction of safety. Mr. Hollins.

Both Sykes's and Siemens's contacts are doubtless excellent examples, and both are carried on the rail itself.

Sykes's contact is shown in Figs. 13, 14, and 15, and a model is on the table.

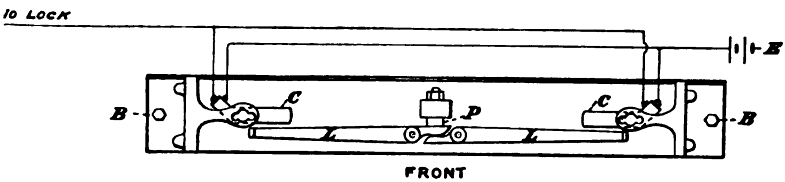


Fig. 13.

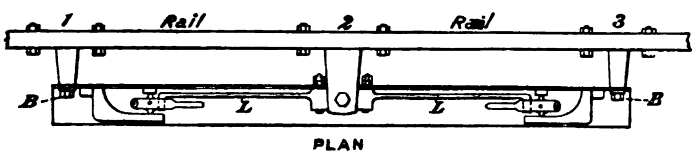


Fig. 14.

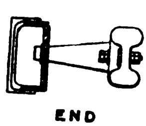


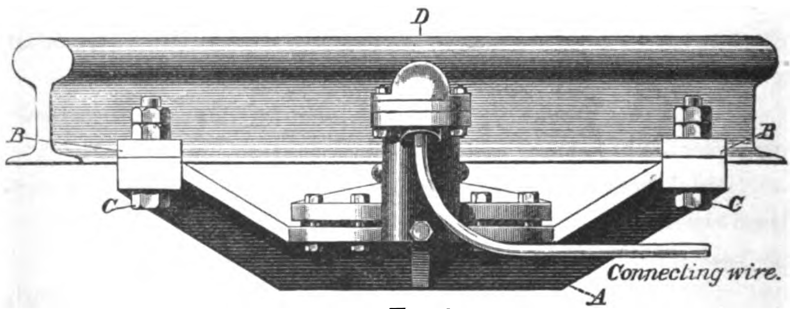
Fig. 15.

Three cast-iron brackets, 1, 2, and 3, are secured to the rail, and the treadle bolted to 1 and 3 by bolts, B, B. The centre bracket has an adjustable pin, P, which passes through a slot, and rests on the tails of two levers, L, L, the extreme ends of which are

Mr. Hollins. under the mercury cups, C, C. The weight of an engine or coach over the spot P results in the tilting of the mercury cups, thus making or breaking contact, as the case may be.

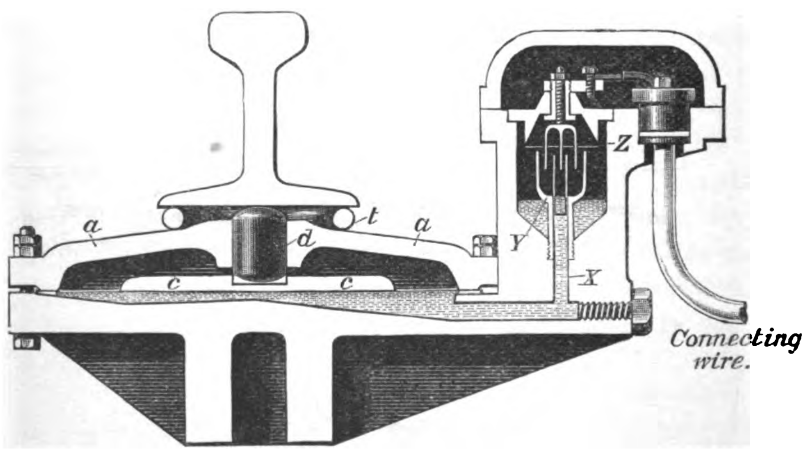
Siemens's contact is shown in Figs. 16 and 17.

Fig. 16 shows a side view of the complete apparatus attached to a rail, and Fig. 17 is a section, showing the working parts of the apparatus.



Front.

FIG. 16.



End Section.

FIG. 17.

The contact maker is supported by a strong cast-iron beam or small girder, A, which, by means of claws, B, B1, and screws, C, C1, is firmly screwed to the bottom flange of the rail, D. In the

centre of the beam is a broad shallow dish, the rim of which is covered by a thin steel plate, similar to the diaphragm or membrane of a telephone. On this iron diaphragm rests an iron disc, *c c* (Fig. 17), which is held at the centre by the plunger, *d*. This plunger is so adjusted that it just touches the under surface of the rail when the apparatus has been firmly screwed to the latter. The dish, steel diaphragm, and the disc are covered by a dome, *a a*, in the centre of which moves the plunger *d*; and on this cover, and under the foot of the rail, is a ring of solid india-rubber, *t*, laid round the plunger, *d*, to protect it from the penetrating sand, which otherwise might impede its free action. The pressure upon this steel diaphragm by the deflection of the rail, due to a passing train, forces some of the mercury in the dish underneath up the tube, *x*, and the cup, *y*, into which the contact points, *z* (to be joined together), are suspended, and thus closing the electrical circuit. Mr Hollins.

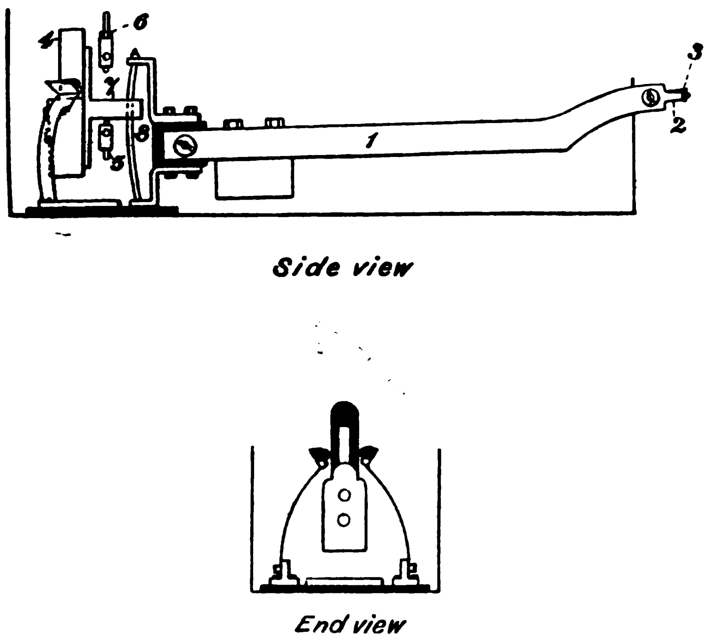


Fig. 18.

Neither of these forms have, however, been made use of on the

Mr. Hollins. Great Eastern Railway, but it would be interesting to learn from users of them to what extent they are in operation, and what is the result of their experience.

The form used on the Great Eastern Railway (and we have about 400 of them in use) is shown in Fig. 18, and a model is on the table. This was designed by myself, in order to obtain a rubbing solid contact, in preference to mercury, and to be self-adjusting by the passage of each train.

It is secured to a strong timber, bolted to, and underneath, the ends of two adjoining sleepers.

As will be seen, it consists of an iron containing box, with a long heavy lever, 1, at one end of which is a tail-piece, 2, to fit into a corresponding hole in the web of the rail. A steel key-piece, 3, which is adjustable, is used to avoid undue looseness in the hole owing to wear and tear. At the other end of the lever, and insulated therefrom, is a self-adjusting friction clutch, 7, and contact piece, 4, which slides somewhat tightly up and down on the steel bar, 8; and the range of the movement of this is controlled by the steel stop pins, 5 and 6, without interfering with the free movement of the lever itself. It will be obvious that this arrangement provides that, should there be a tendency to make contact through the road sinking, the next train readjusts it, and, therefore, each train may be said to readjust the contact for the following train.

This friction clutch rail contact has given great satisfaction. During the six months ending September 30th last (probably our busiest months in the year), although the 400 rail contacts were operated in the aggregate for 5,856,000 trains, there were only five failures; and this, I think, is a remarkable result. During the months of July and August, however, though actuated in the aggregate for 1,953,000 trains, there was not a single failure.

Although it is exceedingly rare for a failure to take place on the wrong side (that is, the lock to be taken out before the train arrives), there is always the element of possibility that in any form of rail contact it may do so. In order to avoid this, I have devised a simple means for ordinary through rail contacts, which should practically extinguish this form of failure. At the spot where the rail contact is fixed, one of the rails is insulated

at each end from the adjoining rail (see Fig. 21), the usual metal fish-plates being insulated from the rails (or they may Mr. Hollins.

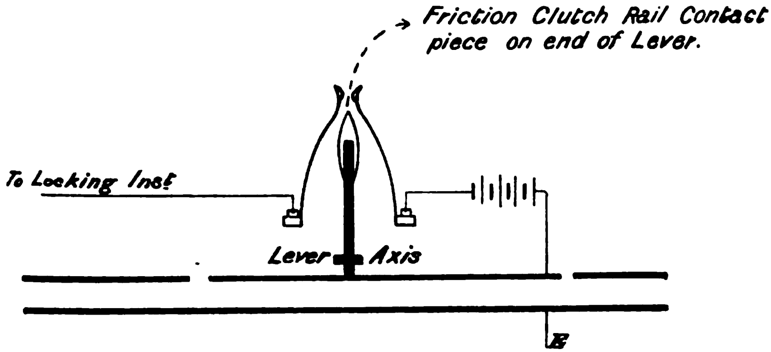


Fig. 21.

be replaced by oak blocks), and the positive pole of the rail contact battery, which is in a box on the spot, instead of going direct to earth, is attached thereto. The battery is then only joined up to earth by the axle and wheels connecting the insulated rail to the opposite rail (which is making good earth), at the exact moment when it is wanted—that is, just as the wheels are passing over the rail, the deflection of which manipulates the rail contact. It should be remembered that with Sykes's locking the rail contact is not connected to the instrument at all, until the signal is taken off for the train.

The Electric Fouling Bar.

At certain points, after passing through a section, a train may not require to go into the section in advance; or it may be required to shunt into a siding to let a following train pass, and in this case it would not pass over the sectional rail contact.

To provide for this at the entrance of, but inside, the sidings, a Sykes's electric fouling bar (Figs. 19 and 20) is fixed, of sufficient length to ensure that any vehicles entering shall always have at least one wheel on the bar to depress it. A few yards further in the sidings a rail contact is fixed. The depression of the bar, by actuating the switch, 1, breaks down the rail contact circuit, so that, although the latter is operated as the engine passes over it,

Mr. Hollins. it does not release the back lock until the last vehicle has passed over the bar. This ensures the tail of the train being clear of the main line before a following train can be accepted.

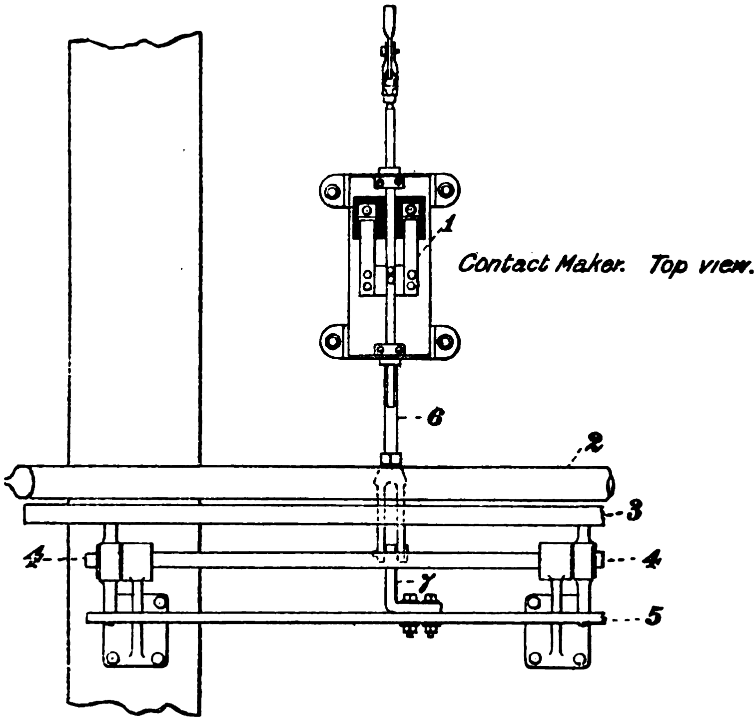


Fig. 19.

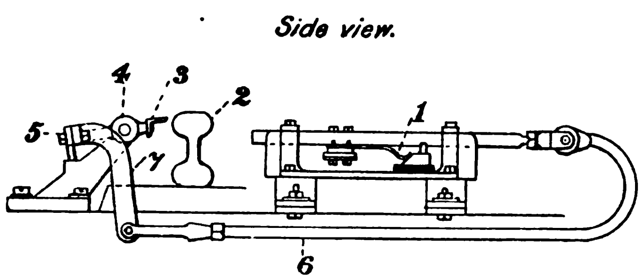


Fig. 20.

In Fig. 20 the rail is shown at 2, and the flange of each wheel depresses the bar, 3 (the axis of which is at 4), raising the heavy

balanced end, 5, and pulling the rod, 6, connected thereto by the angle piece, 7, and actuating the switch, 1. Mr. Hollins.

At very busy points it is sometimes desirable, (in order to get the traffic through quickly,) to allow more than one train in a block section, and this can be done with safety with Sykes's system. An advance starting signal is placed well ahead in the section, between two cabins, and is electrically controlled by the signalman ahead. A train, being accepted from the rear, may be passed out of that section, over a rail contact to release the starting signal back lock, up to the advance signal, and the starting signal put to danger behind it. Another train can then be accepted from the rear. The starting signal, (apart from the mechanical locking,) cannot again be pulled off, for the second train, until the advance signal has been lowered, and the first train passed over a rail contact, ahead of that signal, and the signal put to danger and relocked. Then the starting signal is free for the second train, and so on. We may, therefore, have a train between "A" and "B," another between "B" and his advance signal, and a third between that signal and cabin "C," all fully protected.

Sykes's Signal Reverser.

In some cases it is desirable for the train *itself* to automatically place the advance signal to danger, independently of the signalman. A Sykes's signal reverser is fixed between the balance-weight lever and the signal arm, and moves with the upright rod when the arm is lowered. On the train passing over the rail contact, part of the current transmitted therefrom is used to discharge this signal reverser, (and the arm goes to danger,) at the same time as the back lock of the lever is released in the signal box. On putting back this lever (which is then locked), it re-engages with the reverser, ready for lowering for the next train.

Figs. 22, 23, and 24 show this reverser in its three positions. In Figs. 22 and 23 the signal arm is lowered, and, on the train passing over the rail contact ahead, a current is transmitted through the electro-magnet coils, M, which attracts armature, A, and releases the hammer, B, and this, falling on rod, C (see Fig. 23), disengages the catch, D, from roller arm, E; and slide, F, being

Mr. Hollins. bevelled, the top weight of the signal arm, the rod and the apparatus, falling, by gravity, and raising the signal arm, forces

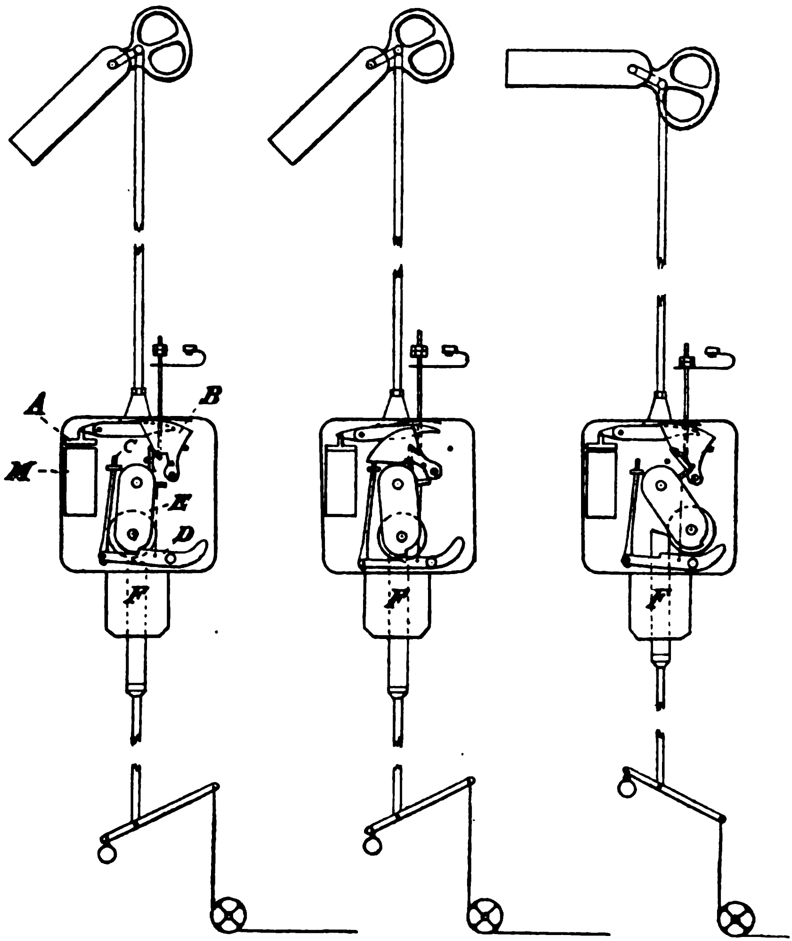


Fig. 22. Fig. 23. Fig. 24.

the roller arm, E, to assume the position shown in Fig. 24, where the hammer is again caught up by the armature, A. When the signalman replaces his lever, the slide, F, moves downward, and the roller arm is caught up by the catch, D.

The apparatus is simple, works satisfactorily, and deserves to be more largely used.

Siding, Junction, and Cross-over Working.

Mr. Hollins.

No system of electric interlocking can hope for very wide application that does not specially commend itself, not only for ordinary through working, (which is comparatively easy), but as equally safe and applicable for siding, junction, and cross-over working.

On the Great Eastern Railway we have found Sykes's system specially suitable, and we have largely taken advantage of its facilities by providing, I believe, the most complete arrangements yet devised for avoiding mistakes at such points.

Junction Interlocking.

At junctions of converging lines, with very short sections, whilst the ordinary electric interlocking prevents trains being simultaneously accepted from both lines, the locking instruments are so interlocked with the points, that the points for a down road must be set so that an up train accepted could not possibly cross the path of a down train, and *vice versâ*.

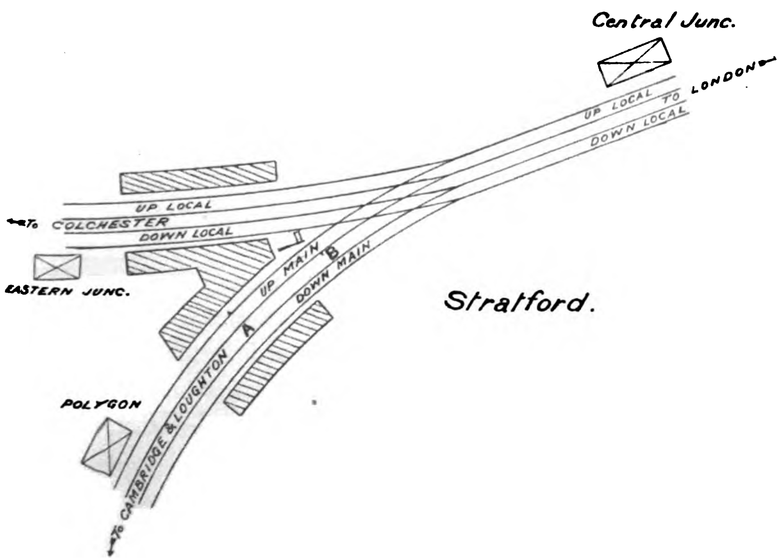


Fig. 25.

Diagram Fig. 25 will best illustrate what is meant. All the through lines, sidings, carriage roads, &c., are left out; only the

Mr. Hollins. lines concerned being shown, and the one signal referred to. Now, (with reference to Fig. 25), before Central Junction can plunge to accept, and unlock the section signal for, an up train, from Polygon, he must set his down local points for down local to main, so that a down train could not possibly pass across the path of the up train accepted from Polygon.

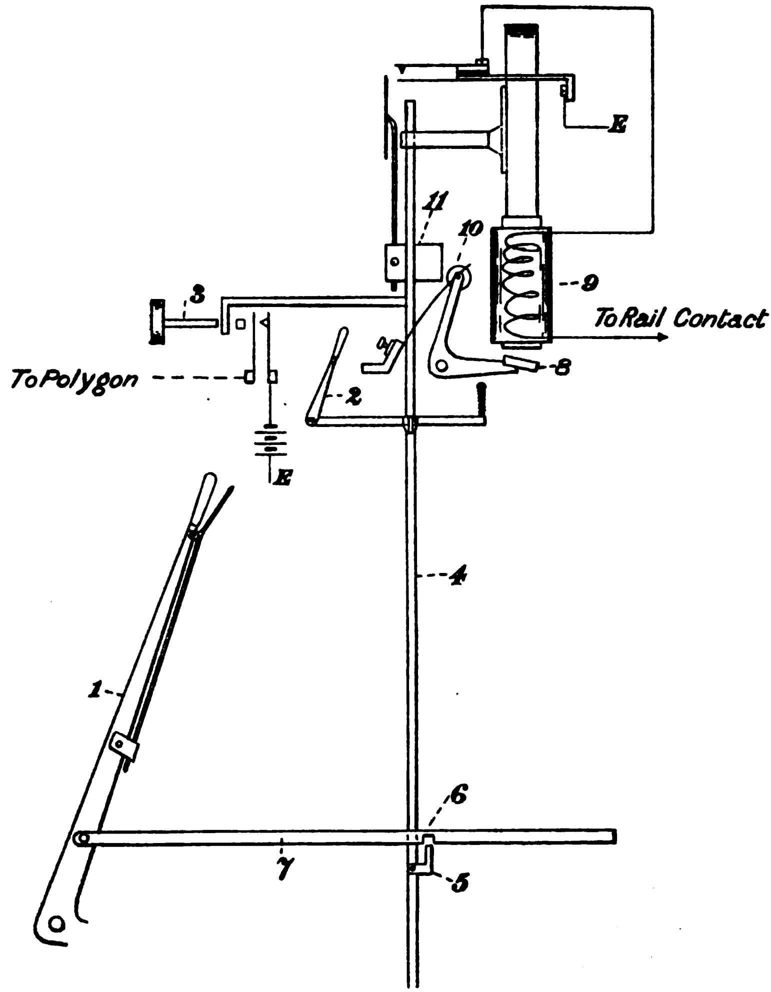


Fig. 26.

When this point lever, 1 (Fig. 26), down local to main, is

Mr. Hollins.

back in the frame, the small lever, 2, on the instrument shelf is unlocked. Pulling over this small lever unlocks the plunger, 3, by raising rod, 4, enabling the up train to be accepted; but the same motion of the small lever, raising the locking piece, 5, on the rod, 4, into the slot, 6, in the tappet, 7, mechanically locks the down points as set; and at the same time the armature, 8, is raised up to the coils, 9, and the small wheel, 10, passing under the angle piece, 11, on the rod, 4, this small lever is itself, as well as the points, electrically locked. He may now plunge to accept the up train, and he can neither alter the points, the small lever, or the plunger, until the train has passed a rail contact at point, A, (see Fig. 25,) on the platform line, and is either standing at the starting signal, B, at *danger*, or, if the latter is "off" (which would then mechanically lock the same points, and be itself back-locked electrically), until the tail of the train has arrived well over the down road, and the signal again put to danger behind it. The small lever, being unlocked by the current from the rail contact at A, and the starting signal at B being to danger, the points may be altered to down local; but this, by preventing the rod, 4, from being raised (it cannot be unless the locking piece, 5, coincides with the slot in the tappet, 7), again locks the small lever, and prevents another up train being accepted from Polygon. This arrangement is essential, because, being at a platform, we may require the train to stand there, with the signal at danger, so that a train *may* be allowed to cross its path on the down local.

At other nearly similar converging lines, but not at a platform, the signalman is compelled to lower his home signal for one line (which, of course, mechanically locks the lever of the home signal for the other converging line, and the signal for the opposite road, that would cross its path) before he can plunge to accept a train in either direction. And such home signal being lowered enables the signalman to plunge, and unlock the starting signal, for the same line, at the rear box; but the plunger and the signal alike remain locked in that position until the train so accepted has arrived and passed over the rail contact at the clearing point.

Mr. Hollins.

Sidings and Cross-over Connections.

Again, in the case of sidings and cross-overs, this same electrical and mechanical locking between points and plungers comes in. If a train has been plunged for—that is, the rear box starting signal electrically released—the points leading from the sidings to the up line are locked. But, if the points are already over for a train from the sidings, then an up train cannot be accepted from the rear. And if the train from the sidings requires to cross over to the down line, if the points are so set, a down train cannot be accepted; but if a down train *has* been accepted, the cross-over points to the down road cannot be got until the accepted train has passed, and the road is clear.

Crossing Trains from Down to Up or Up to Down Lines.

Here, again, is another application of interlocking between points and section plunger. A small Sykes's lever is fixed in the up plunging instrument, which, pulled from right to left, locks the up plunger and unlocks the points. Pulling over the points locks this small Sykes's lever, as previously explained, in this position. If a train is to be crossed from the up to the down road, the signalman must go through the reverse motions. If, however, an up train *has been* accepted, he is unable to move the cross-over points from down to up; and if a down train *has been* accepted, he is unable to move the cross-over points from up to down, until such trains have passed clear of the section concerned. The small lever normally controls the points. Moved over, it controls the plunger and releases the points, and, the points being moved, locks the small lever in that position.

And here I think I ought to point out why, it seems to me, Sykes's system lends itself most effectually, in the simplest manner, and at the least expense, to sidings, junction, and cross-over working. I have heard it said, even by those for whose opinions on the subject I have the greatest respect, that they have not, and do not require, in their interlocking any back lock for keeping the signal off, until the train itself arrives, and releases it.

They can, it is claimed, always put the signal to danger with their systems. Quite so! and, unfortunately, the lever right back

in the frame. It is, I submit, just that which they ought not to be able to do, because, as the lever can be put back in the frame, it releases conflicting point levers and signals otherwise held locked at danger by the ordinary mechanical locking. Mr. Hollins.

For instance, at a junction of converging lines, and where an up train for one road would cross the down road for the other, and *vice versâ*, and the locking of which is on the two home signals, it only requires one of the rods or blades in each Sykes's instrument to be about 1½ inches longer than usual to prevent plunging until the home signal is taken off. When it *is* off, it is electrically back-locked in that position, and you may safely plunge for a train, because it has itself locked, by the mechanical locking, the other contrary signals.

There are many variations of this combination of the locking instrument, the plunger, and the points, together with the small Sykes's lever, to effect different purposes, and facilitate traffic generally, whilst ensuring that every train is within the protection of one or other of the stop signals; sometimes between home and starting signals, and sometimes between the latter and the advance starting signals, the back motion of the leading signal taking the front lock out of the rear signal, and separate rail contacts taking the back locks out of both.

Within the limits of this paper it would not be possible to give, however interesting it might be, the details of the electro-mechanical devices in use for safely facilitating the traffic in busy stations like Waterloo, Cannon Street, and Liverpool Street Stations. A lucid description of the electrical devices and combinations for checking and controlling, advising and reminding, and generally assisting the signalmen in safely working the enormous amount of traffic at the different London terminal stations, would be amply sufficient to warrant a most interesting paper on this subject alone.

Cancelling Trains and Resetting Apparatus.

Unfortunately, in all systems of electric interlocking, the exigencies of the traffic seem to necessitate the provision of a key, or switch, or other means, to release in cases of emergency. A

Mr. Hollins. train which has been accepted may not have to pass through the section, and requires to be cancelled; a defective plunge may not have released the lock (through no defect in the apparatus), or the rail contact may occasionally fail to release the plunger, releasing key, or back lock of the signal lever; or for other reasons. Then the instrument requires to be reset. Undoubtedly, the possession of this key in the hands of signalmen, whatever form it may take, is a distinct menace to the security of electric interlocking. Used for its legitimate purpose, only when justified, it is, of course, an advantage in facilitating traffic; and some means *must* be provided to meet such cases, if disastrous delays are to be avoided, on busy lines.

It seems to me that neither the key, nor its equivalent, should be available for use at any one box, without the sanction, and concurrence, of the signalman in advance, or the signalman in the rear, just according to the purpose for which it is wanted. There are two advantages in this—the concurrence of two men at a distance equally responsible, and *proof of the fact of its being used.* I suggest it can be done without extra line wire, by using the bell or locking wire, or both. In the case of cancelling where the plunger or key only is locked, and requires releasing, after accepting a train, it is the sanction of the signalman in the rear that should be necessary. Where the train does *not* pass through the section, and over the rail contact, or the rail contact fails to release the back lock after the signal has been lowered, it is the signalman in advance whose sanction, and concurrent action, should be essential.

With the Sykes instrument, to release the plunger a small electro-magnet may be fixed, normally locking the shutter over the key-hole, and thus preventing the key from being put into the instrument, to raise the "Train accepted" disc, without a current from the rear box. In addition to this, the arrangement provides that, unless the key is withdrawn from the instrument *and the shutter placed over the key-hole,* the block bell circuit, or the locking circuit, shall be broken down. This arrangement, necessitating the permission by current from the rear, also limits the use to the occasion, and the particular instrument for which permission is

given. To ask for the release of the back lock (or the plunger, or accepting key, in other systems), a bell signal may be given to the box in advance, indicating that a current is required to release the back lock or accepting key. The signalman asking would then depress his receiving key, to switch the current to the locking coils. The actual current would not only unlock the lever, but ring the bell, indicating that the current had passed to the locking instrument. A record of this at both places could be made in the train book. Mr. Hollins.

Diagram Fig. 27 (Plate 2) indicates the complete electrical connections of Sykes's system, and the arrangement applied thereto. The coils to be acted upon for the back lock being polarised, these permissive currents on the bell wire would be of opposite polarity to the bell currents, as the bell battery is joined up with the reverse pole to earth, so that its current would not actuate them. This ensures that the current transmitted shall be the one *intended for the purpose.*

I have brought a model installation of Sykes's system, representing three stations, "A," "B," and "C" (it was used for teaching our signalmen the system of working); and it is fitted with the arrangement for "B" to be respectively released, as regards the plunger, from "A," and, as regards the back lock, from "C," so as to practically demonstrate its applicability. It will be seen by Diagram Fig. 27 (Plate 2) that the plunger releasing is done on the locking wire, and the back lock releasing on the bell wire. The whole additional fittings for each box, with Sykes's system, is a small coil for each locking instrument, and one or two good keys or three-way press buttons.

The keys, or press buttons, or switches, to *transmit* a current to the rear to release the back lock, (equivalent to actuating the rail contact), are marked A; the keys or switches requiring to be depressed to *receive* and divert that current to the coils required, are marked B; and the ordinary bell keys—which are, of course, in actual practice a structural part of the bell itself—are marked C, all in the bell wire. The keys or switches to *transmit* the current to the box in advance, to unlock the shutter, so that the plunger may be reset in case of cancelling,

Mr. Hollins. or a defective plunge, are marked D, and are in the locking circuit. The corresponding keys or switches to be depressed in order to *receive* and divert that current to the little shutter locking coil are marked E, and the coils themselves are marked F. In practice, unless the shutter is completely covering the key-hole (in which case the key must be out), the locking circuit is broken down at X. Of course this is only one way of applying the principle, and it is applicable to any system, and by a separate wire, or otherwise, as desired.

Sykes's Selector.

Mr. Sykes has recently brought under my notice an electrical selector for signals, (one form of which is in use at Waterloo Station), which is a device to be applied where several arms are attached to one post, so that one lever, and one signal wire, can operate any one of the arms upon that post, according to the points which have been set. It is thus a point detector, combined with a signal selector, and none of the arms can be operated except in the signal box. Neither pulling the signal wire outside, or lifting the rod at the signal, moves any of the arms; *the points must be set and the lever in the box operated.* Pulling the particular point lever that is wanted (providing the points are properly closed) operates the selector, and pulling the signal lever lowers the signal for that road only for which the points are set. The one upright rod on the signal, operated by the signal lever, is coupled to the middle of as many sway beams as there are arms, one end of each of which is attached to the rod belonging to each arm. Normally, pulling the lever, without first operating the points, lowers the arm for the through road only. If any particular points are operated this actuates the corresponding selector, the loose end of the sway beam belonging to the signal arm for that road is held down, and, if the upright rod is now moved up, it becomes the fulcrum which raises the end of the sway beam attached to that arm, and lowers the signal.

Diagram Fig. 28 further clearly shows the electrical arrangement, and a model is on the table.

In conclusion, the question may be asked, "Who was the first

"and true inventor of electrical interlocking?" The extended Mr. Hollins.

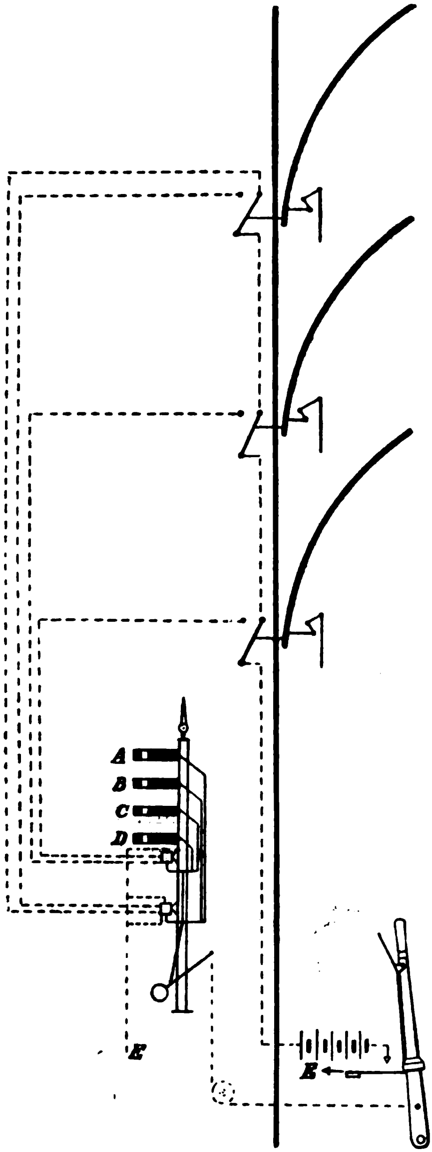


FIG. 28.

application and importance of the subject suggests there being more than one claimant.

If we may assume absolute block working, to be the only real

Mr. Hollins. block working, and that electric interlocking is not absolute without a rail contact, then Mr. Edward Tyer has an early claim. In 1852, and 1854 also, he devised, and had in use, a rail contact, depending upon the deflection of the rail, by an engine or train, to automatically and electrically indicate the passing of such train over certain points. In 1869 he patented a complete system of electric interlocking and block, for controlling the signal, and point, levers, in combination with the block; and he proposed to do electrically, what is now done by mechanical interlocking, namely, prevent levers of all kinds from being used except such as were required for the train being signalled; *and for the signalman in advance to have control of the section signal in the rear.*

In 1870 Mr. W. H. Preece (and I think Mr. Langdon was associated with him in the invention) introduced an electric lock on the South Western Railway, to be worked in connection with his system of block.

Mr. W. R. Sykes claims that he, in 1872, introduced a rail contact to electrically manipulate railway signals; and in 1875 he introduced the first complete system of electric interlocking, making it necessary to have the concurrence of three stations— "A," "B," and "C"—to get two trains into any one section.

Mr. Spagnoletti was also early in the field with his arrangement of electric interlocking on the Metropolitan Railway, which I think was put in about 1882. Mr. Spagnoletti, however, in 1873, patented his system of clearing the road, or resetting the instrument, by a rail contact.

Simplicity, efficiency, and reliability are to us, however, of more importance than questions of priority of invention of such apparatus; and the point may be left, I hope, to the friendly rivalry of the gentlemen named, all of whom deserve the most ungrudging, and generous, acknowledgment of what they have done, in the cause of electric block signalling, and electric interlocking, on our railways.

APPENDIX I.

Mr. Hollins.

SYKES'S SYSTEM OF ELECTRIC INTERLOCKING IS IN USE AS UNDER:—

Railway.	Number of Levers Locked.	Railway.	Number of Levers Locked.
Dublin, Wicklow, and Wexford	8	London, Brighton, and South Coast	648
Great Eastern	1,022	London, Chatham, and Dover	1,320
Glasgow and South Western	26	Metropolitan District ..	136
Hull and Barnsley ..	254	Mersey	121
Liverpool Overhead ..	21	South Eastern	283
London and South Western	1,051		

APPENDIX II.

DESCRIPTION OF BLOCK AND INTERLOCKING IN USE ON PRINCIPAL ENGLISH AND SCOTCH RAILWAYS.

Line of Railway.	Description of Block.	Electric Interlocking, and to what extent in use.
Caledonian	Tyer's three-plunger semaphore block.	Modified system of interlocking in combination with Tyer's three-plunger semaphore block on 50 sections of new City and Suburban lines.

Mr. Hollins.

Line of Railway.	Description of Block.	Electric Interlocking, and to what extent in use.
Great Eastern ..	Tyer's one-wire double line. Tyer's one-wire single line. Tyer's tablet block. Sykes's lock and block. Single-needle block on joint line, March to Doncaster.	Sykes's. Metropolitan District — Liverpool Street and Fenchurch Street to Ilford; on Colchester line; to Tottenham on Cambridge line; Woolwich and Enfield branches, and to Leyton on Loughton branch: in all, 78 signal boxes and 1,022 levers.
Great Northern ..	Three-wire single needle and bell. Blakey & O'Donnell's apparatus. Sykes's electric slots & signal reversers.	Electric locking applied to 13 sections in various places.
Great Western ..	Spagnoletti's three-wire disc. Electric train staff, Webb and Thompson's. Tyer's one-wire block.	Spagnoletti's at about 12 different points. Spagnoletti's one-wire on Hammersmith and City Joint lines.
Glasgow and South Western	Tyer's one-wire. Tyer's train tablet.	None.
Lancashire and Yorkshire	Three-wire absolute and Tyer's one-wire.	Nicholson's electric locking at one or two boxes.

Mr. Hollins.

Line of Railway.	Description of Block.	Electric Interlocking, and to what extent in use.
London, Brighton, and South Coast	Tyer's semaphore, 373 miles. Sykes's, 43 miles. Webb and Thompson's train staff, 50 miles.	Sykes's system, 43 miles. 11 miles (country district) Saxby & Farmer's.
London, Chatham, and Dover	Sykes's double-arm bell. S.N. instruments with bells (Metropolitan and Extension, and main line to Swanley).	Throughout. Sykes's, with 80 treadles. 67 Sykes's fouling bars. 57 Sykes's electric shunting signals. 76 electric facing point detectors, &c., &c.
London and North Western	Three-wire constant current, and small portion one-wire.	None, except (in a few instances) electric interlocking between sidings at a distance from station, with controlling signal.
London and South Western	Three-wire (Preece's). One-wire ,, Tyer's tablets. Needle block and bells, three-wire.	Three-wire block and Sykes's interlocking, 111 miles. Tyer's tablets and Sykes's interlocking, 141 miles.
Hull and Barnsley ..	Webb and Thompson's tablet on branch line, 12 miles. Sykes's, about 60 miles.	Sykes's, 60 miles.

Mr. Hollins.

Line of Railway.	Description of Block.	Electric Interlocking, and to what extent in use.
Manchester, Sheffield, and Lincolnshire	Single-needle three-wire.	Evans's interlocking for about 10 miles. Sykes's at Hawarden Bridge, and special arrangement of Mr. Hampson's at the two swing bridges at Keadley.
Metropolitan District	Sykes's	Sykes's system in use nearly throughout.
Metropolitan.. ..	Spagnoletti's three-wire.	Spagnoletti's throughout.
Midland	Single-needle three-wire.	Langdon's interlocking, about 12 to 15 sections.
North British ..	Three-wire needle (Tyer's). One-wire needle (Tyer's). Tyer's tablet.	None.
North Eastern ..	Three-wire single-needle. Webb and Thompson's train staff.	None.
North London ..	Pryce and Ferreira's three-wire throughout.	None.

Mr. Hollins.

Line of Railway.	Description of Block.	Electric Interlocking, and to what extent in use.
South Eastern ..	Walker's one-wire semaphore improved.	Sykes's throughout Charing Cross and Cannon Street lines. Safety bars at Cannon Street.
North Staffordshire ..	Tyer's one-wire, and Neale's one-wire.	None, except at intermediate sidings, where points and signals are electrically locked and interlocked by Neale's electric locking.

The President.

The PRESIDENT: Before inviting you to commence the discussion on Mr. Hollins's very valuable paper, I think you will wish to accord him a very hearty vote of thanks. It is to me an extremely interesting paper, and it is most important that we should have from time to time such papers as this to put on record in the Proceedings of our Institution. It brings us up to date. At one time it was a subject that I took great interest in; and I think it must have been in the sixties, at the time when the Clayton tunnel disaster occurred, that I addressed a long communication to the London and Brighton Railway, suggesting a system of signalling which included these rail contacts. I had no idea until I read this paper to-day that rail contacts had been recommended even some years before that, namely, in 1852. I presume you will accord with acclamation a hearty vote of thanks to Mr. Hollins for his paper.

Carried with acclamation.

Mr. HOLLINS: I beg to thank all of you for your kind vote of thanks.

Mr. Wyles. Mr. F. WYLES: As you have just expressed your gratitude to Mr. Hollins for bringing this paper before you, I will content myself with endorsing what has been said—that this is a very able paper, and one which must have entailed upon Mr. Hollins a great amount of labour in writing.

I see from the appendix at the end of the paper—which appendix, by the way, may be very useful as a reference—that, omitting the London, Chatham, and Dover Railway, who are completely interlocked, the London and South Western Railway stand first as regards the number of levers locked, having 1,051, as against 1,022 on the Great Eastern Railway. Mr. Hollins mentions that he has about 400 treadles fitted. As against that we have nearly 350. Taking the paper in order from the beginning, the first point is the question of insulated rails as fitted at St. Paul's and at King's Cross. We have no places on our railway where we have rails insulated. The method at Waterloo is to have a series of fouling bars, which bars when depressed by a train govern the "In" signals. At Waterloo there is the double system of signalling in. If a train is standing on what we call a "caution "bar," the driver can only have the "In" signal off to 45°; but if it is standing on the danger bar—that is to say, well up to the end of the platform, and filling the bay—the signal cannot be pulled off at all. These bars can be applied to any terminal station.

I do not know what the insulation resistance of the insulated rail circuits may be, but I should say that in our variable climate it must be extremely small.

Coming to the question of single lines, I think the locking of the starting signals in connection with the Tyer's tablet system is of quite recent date.

It is about four years since we started tablet locking. Mr. Sykes, with myself, in conjunction, adapted locking to the starting signal in connection with the No. 3 Tyer tablet instrument. It was first tried, I believe, at Farnham, and it has proved thoroughly satisfactory. Nearly all our tablet stations are now fitted with that locking.

Of course it is perfectly easy to fit any other tablet instrument,

although it requires some thought, especially with those tablet instruments that have not visual block signals, to see at first sight how the work should be done; the same remarks apply to the Webb and Thompson train staff, the interlocking of which is clearly shown in the diagram in the paper. The diagram in connection with Mr. Tyer's arrangement for locking a starting signal does not show clearly that, having drawn the first tablet, before obtaining another the starting signal lever must be replaced; but of course it is quite necessary that this lever must be put back before a second tablet can be drawn. Mr Wyles.

With regard to the Sykes system generally, there is very little difference and very little criticism can be made between the systems on the Great Eastern Railway, and on the London and South Western Railway, the South Eastern Railway, and the various other companies.

They all have very much the same signs, but the instruments are a little different in most cases. The London, Chatham, and Dover Railway is completely fitted, and the instruments, as worked there, are very simple. The semaphore arm, instead of being worked by the small switch on the right-hand side of the plunger, is thrown to danger by the action of the plunger in advance, which releases the lock on the signal in the rear, allowing the train to enter the section, and it is "cleared" automatically by the putting back of the signal lever at the other end of the section when the train passes out.

That saves labour on the part of the signalman, and absolutely ensures the arm being put to the danger position. The switch, however, controls the arm, and may be used to obstruct the line.

The system on the London and South Western Railway is practically identical with the Great Eastern Railway, except that we do not have the "Train on" and the "Train accepted" signs. The switches are worked by hand, and we practically retain the old Preece signs of Mr. Preece's system, plus the Sykes locking. It makes an exceedingly simple instrument, which does its work very well.

I suppose the paragraph in the paper with regard to lightning is fairly open to criticism.

Mr. Wyles. I do not know that it is perfectly certain that a lightning discharge is always of a positive nature. In any event, if a positive current from a lightning discharge strikes a wire, it induces in a neighbouring wire a current of an opposite polarity; and possibly, if a Sykes instrument could be freed by a lightning current, it might be done in that indirect way equally as well as by a direct discharge; but I can fairly say that I know of no instance whatever where the Sykes instrument has been discharged by lightning. If the current were too strong, in all probability the instrument would not free; the magnetism would be very rapidly reversed, and would, I think, hold the armature up. Of course, if the current is too small, it will not free the instrument.

Now with regard to treadles: The type that we are using is the ordinary Sykes treadle.

I cannot give figures as Mr. Hollins does, and I do not know how our statistics would come out; but certainly the treadle shown on Fig. 15 is a very admirable one, and gives us very little trouble.

The working parts are all well covered up, so that nothing can get at the interior, and the treadle is comparatively easily fitted and adjusted. The mercury contacts are always open to criticism, but I do not think there is any objection to the use of mercury, more especially as the contacts are duplicated as shown in that treadle. With regard to fouling bars, a very useful place where these may be put down is in the stations where trains are divided, so as to ensure that the plunger shall not be used until the second portion of the train has gone away. Necessarily, if the train is divided, and the front portion goes over the treadle, the back lock is taken off, and the object of the fouling bar is to prevent the signalman plunging until the second portion has gone away also.

With regard to the reverser, or replacer, I think it is some six or seven years since Mr. Sykes brought this out. I remember the original instrument from its shape; it was known as the "mouse trap."

It was invented one Sunday morning in the old "A" box at Waterloo, and served as a pattern to be improved upon, inasmuch

as it took about 18 Leclanché cells to discharge it. The next instrument we used worked with about four cells, and was very similar to that exhibited. It was known as the "banjo," and that name may still be heard on the South Western Railway. Mr. Wyles.

The reverser shown on the table should work, I think, on short circuit with about two Leclanché cells; so you see it is an extremely good instrument, and yet one not likely to get out of order. The reversers are extremely useful at advanced starting signals; they make those signals safe which perhaps otherwise could not be utilised.

One word with regard to the back lock.

I see Mr. Hollins mentions the back-lock system as being almost peculiar to the Great Eastern; but this is not so, as it is an integral part of the Sykes system.

We use the back lock wherever treadles are put down to protect the plunger, and it forms also a most admirable system for holding the road until the train has arrived at the treadle—that is to say, a route lock.

If you have a big junction, and the road set, the points cannot be reversed until the train arrives at the treadle, allowing the signal lever to be put back and the points to be reversed.

The pattern selector shown is an interesting one, and the time will probably come when that system may be used extensively.

Mr. LANGDON: Mr. Hollins in his paper has dealt, as he indicated at the commencement of the paper, chiefly with the Sykes system, as that is the system of which he has most general knowledge, it being used entirely on his line. But reference has been made to other systems, and it occurs to me that possibly those gentlemen whose systems have been referred to might desire, on the occasion of the next meeting, to place instruments on the table or diagrams on the wall, in order that they might have the opportunity of explaining their systems more fully. Mr Langdon.

The PRESIDENT: That is a very good suggestion, and I will see if it can be done.

We will now adjourn this discussion until February the

11th. I have to announce that the scrutineers report the following candidates to have been duly elected:—

Members:

John Henry Barker.
Ernest A. Claremont.
Ross Cotton.
Charles Herbert Gadsby.
James Moss.
A. C. O'Bryen.

Associates:

J. W. Boucher.
Alfred T. Clarke.
Charles Furness.
William Gore.
Harold Gray.
Josiah M. Guttridge.
Basil Bell Heaviside.
Arthur J. Hodgson.
William James Larke.
Ernest Denn Long.
G. A. Maquay.
Lawrence J. A. Mills.
Henry C. Newton.
Ernest W. Sawyer.
Matthew Short.
Sidney Smith.
Charles William Speirs.
John C. A. Ward.
Duncan Watson.
Charles West.

Student:

Charles W. Fourniss.

ABSTRACTS.

A. HOLLARD—THE ANALYSIS OF COMMERCIAL COPPER BY ELECTROLYTIC METHODS.

(*Comptes Rendus*, *Vol.* 123, *No.* 23, *p.* 1003.)

The electrolytic method adopted by the author, allows of accurately determining the purity of copper, and of estimating the smallest quantities of impurities such as arsenic, antimony, nickel, cobalt, iron, silver, lead, and sulphur. The electrodes consist of a truncated cone of platinum foil, and of a spiral of the same metal, fixed to a base, the weight of each being about 20 grammes.

The upper diameter of the cone is 18 mm., and the lower diameter 45 mm. A platinum rod is soldered with gold to the trunk of the cone. The vessels containing the electrolytes are of Bohemian glass, 6·5 cm. diameter at the base. The electroylsis, in acid solution, is then carried out in the following manner:—Ten grammes of bright copper shavings are carefully weighed, and freed from iron particles by means of a magnet. These are then placed in a glass vessel of 350 to 400 cubic centimetres capacity. Into this are poured 15 cubic centimetres of sulphuric acid, and then 40 cubic centimetres of nitric acid, at 36° B.; the copper having been covered with water in the first instance in order that the action should be moderate. The top of the glass is covered in with an inverted funnel, to form a dropper. Heat is applied towards the end of the action. With refined copper no residue is left, but unrefined copper leaves a sulphur residue. Some brands of raw copper, rich in antimony, may leave a residue consisting of compounds of antimony. If this residue is small, it will not be injurious to the electrolytic deposit, and it may be left in the liquid. If abundant, the residue is separated by filtering, and then dissolved in aqua regia, rich in nitric acid. The solution is then evaporated to dryness, and to the residue is added hydrochloric acid, with an addition of tartaric acid and water. This new solution is added to the liquid previously obtained, and from which the antimony is to be precipitated by sulphuretted hydrogen.

The copper solution is diluted to about 350 cubic centimetres, and the platinum electrodes are dipped into it, the spiral being connected to the positive pole and the cone to the negative pole of a battery. The distance between the lower end of the cone and the foot of the spiral should be about 6 mm. The cone should be completely immersed in the solution, its summit being 1 cm. to 2 cm. below the level of the liquid. When the solution becomes colourless, a few drops are placed in a test tube, and an ammonia test made, to ascertain that it is quite free from copper; after no trace of blue colour is observed, the current is allowed to continue for several hours. The complete electrolysis of copper under the above conditions requires two to three days, and yields a very adherent, smooth, pink-coloured deposit. Without breaking the current, the cone and spiral of platinum

are rapidly removed and washed twice in distilled water. The cone is then dipped into concentrated alcohol, placed in a stove at 90° for 10 minutes, and then weighed. This weight, less that of the cone, represents the weight of copper plus that of the silver. From this weight can be deducted the previously ascertained weight of silver.

If the copper under analysis contains lead, only a part will be deposited on the spiral in the form of dioxide, the remainder of the lead remaining in solution. The author hopes to shortly publish a method of estimating metallic and metalloid impurities in commercial copper.

MARIUS OTTO—ON OZONE AND PHOSPHORESCENCE PHENOMENA.

(*Comptes Rendus, Vol.* 123, *No.* 23, *p.* 1005.)

Whilst engaged in researches on the properties of ozone, the author discovered that this gas may, under certain conditions, give rise to luminous phenomena When exhausting ozonised air by means of a water pump, a brilliant glow was observed in the pump. This light originated at the contact between the water and the ozone; the water continued to glow for five or six seconds after removal from the pump. This luminous water, after being poured into glass vessels, could be observed in the dark.

Several explanations are suggested for the phenomenon:—

1. Ozonised bubbles of gas may, owing to the action of the pump, dissociate at the contact of water with the production of light.
2. Or ozone in combination with water may form a very unstable and phosphorescent combination.
3. Or the above phenomenon may result from the vigorous oxidation of certain organic substances contained in the light.

In order to further study this question, the author employed a glass apparatus by means of which different substances, either pure or in aqueous solution, could be submitted to the action of ozone at varying pressures. The apparatus consisted of a simple glass cylinder, 50 cm. long, 5 cm. diameter, closed at its two ends, and fitted with two taps. The following is a summary of the experiments which were made with ozonised oxygen, containing from 40 to 50 milligrammes of ozone per litre, produced by means of ozonisers:—

Ordinary Water.—A cylindrical vessel was filled with ozonised oxygen, and then 100 cubic centimetres of ordinary water carefully introduced. The vessel was then taken into a dark room and vigorously shaken. A brilliant glow was observed, which lasted several seconds. On again shaking, a much weaker glow was observed. The phenomena can be repeated five or six times by successive shakings After this, all glow disappears, although the greater portion of the ozone still remains in the tube. The phenomena can, however, be reproduced by simply changing the water contained in the apparatus. These experiments were carried out at the normal pressure, but slight variations in the pressure had no sensible effect on the intensity of the phenomenon.

Alcohol.—When using alcohol at 90° temperature a much weaker glow is observed, but which, on the other hand, lasts longer.

Benzine.—With benzine a very weak effect is observed; the ozone appears, however, to be completely absorbed.

Thiophene.—This substance liberates abundant luminous vapours at the contact with ozone. It is the only example of the sort observed by the author.

Milk.—A much stronger effect is observed with milk than with ordinary water.

Urine is the substance which has yielded the most clearly marked phosphorescence phenomena.

Pure Water.—When working with absolutely pure water the above phenomenon was absent, even with very concentrated ozone.

The author is pursuing the study of these phenomena. He draws the following conclusions from the above experiments:—

1. The phosphorescence phenomenon produced when ozone and water are in contact is due to the presence in the latter of organic material of animal or vegetable origin.
2. The greater number of organic substances are capable of giving rise to phosphorescence phenomena when in the presence of ozone.

M. COLARD—ON THE LONGITUDINAL TENSION OF CATHODE RAYS.

(*Comptes Rendus, Vol.* 123, *No.* 24, *p.* 1057.)

The conditions of the hypothesis under consideration are :—

1. The beam consists of a transference of negatively charged molecules.
2. The electric field is negligible in the space considered.

M. Poincaré (*Comptes Rendus*, vol. cxxiii., p. 530) has shown that in the above hypothesis the trajectory of charged molecules in a magnetic field, radiating from the origin of the axes, is represented by the equation,

$$\frac{\frac{d^2 x}{dt^2}}{y\frac{dz}{dt} - z\frac{dy}{dt}} = \frac{\frac{d^2 y}{dt^2}}{z\frac{dx}{dt} - x\frac{dz}{dt}} = \frac{\frac{d^2 z}{dt^2}}{x\frac{dy}{dt} - y\frac{dx}{dy}} = \frac{\lambda}{r^3},$$

λ being a constant.

If M be the material mass of the beam per unit-length, and $-i$ the electric current corresponding to the transference of negative charges along the beam, Q the magnetic mass at the origin, then it results that

$$\lambda = \frac{-Q\,i}{M}.$$

By calling l, m, and n the directive cosines of the intensity of the magnetic field, and H the intensity of the field at the point considered, equations are formed which are found to hold for any values of the magnetic field.

It follows, then, that

$$\Sigma \frac{dx}{dt} \frac{d^2 x}{dt^2} = 0,$$

$$v = \text{constant};$$

v being the velocity of the charged molecules.

If the arc of the trajectory be called s, then $ds = v\,dt$, and by taking ds as variable instead of dt the equation for the beam becomes—

$$\frac{\frac{d^2 x}{ds^2}}{M \frac{dz}{ds} - n \frac{dy}{ds}} = \frac{\frac{d^2 y}{ds^2}}{M \frac{dx}{ds} - l \frac{dz}{ds}} = \frac{\frac{d^2 z}{ds^2}}{l \frac{dy}{ds} - M \frac{dx}{ds}} = \frac{-\,i\,H}{M\,v}.$$

It is therefore seen that a cathode beam propagated in a magnetic field, bends itself in such a manner as to take the form of a perfectly flexible conductor carrying the same current; and this conductor would be the seat of a longitudinal tension equal to the amount of motion of the cathode ray per unit of length. Besides, as the amount of motion is numerically equal to the material mass traversing the section of the ray during unit of time, it is constant throughout the trajectory, by virtue of the law of continuity of the molecular current.

M. VASCHY—ON SOME ERRORS ADMITTED AS TRUTHS IN ELECTRO-MAGNETISM.

(*Comptes Rendus, Vol.* 123, *No.* 44, *p.* 1059.)

The application of the principle of conservation of energy to a physical phenomenon offers no difficulty when the work done, and the variations of energy taking place in the phenomenon, are completely understood. The formula to be applied in this case is reduced to $\delta \tau + \delta w = 0$; δw representing, for example, the sum of the increments of energy of a system of bodies, A, and τ the sum of the work done by this system on external bodies during the same time, there being no other exchange of energy but $\delta \tau$ taking place between the system A and external bodies.

It is in this manner that the expression for electrical energy is calculated. These two particular cases should, however, not be taken as typical cases, for when a magnetic field is produced by a current, the heat developed has to be taken note of; and yet many errors have been made in this respect.

The author then considers the following instance.

1. Relative displacement of a magnet and of a constant current I; calculation of their relative energy:—This displacement produces an increase, $d\,w$, of the relative energy, equal to, and opposite in sign to the work of the electro-magnetic forces. Therefore, the relative potential energy of a current I, in a magnetic field is the same as that of a magnet system with respect to the current—*i.e.*, if $-\,I\,\phi$, ϕ represent the flux of force due to the field in the negative direction of the current.

Then $W = -\,I\,\phi$. In this reasoning the heat, $R\,I^2\,t$, produced by the current is neglected, and the formula for W is incorrect.

Second Case.—Same displacement; calculation of the induced E.M.F.:—Here,

on the contrary, the above-calculated relative variation of energy, W, is neglected. The excess of chemical energy on the heat energy, or the available energy, represents the external work, $d\tau$; from which one obtains the expression,

$$d\tau = (E I - R I^2) dt.$$

As $d\tau = I\, d\phi$, the formula for the induced E.M.F. is

$$e = - \frac{d\phi}{dt}.$$

This reasoning, which is due to Helmholtz, although not rigorously correct, is experimentally so. It has generally been admitted as rigorously correct. Experimental results prove that the relative energy, W, which has been neglected is 0, and not equal to $- I\phi$, which is also shown by an exact calculation of W. In another example, by the analogous choice between the energies which should be considered, it is shown that the relative energy of two currents, I and I′, has the formula W = − M I I′. Further is deduced, from the theory of electromagnetic induction, the correct expression of the same energy, W = + M I I′.

The above, and other false reasonings, have been taken from well-known text-books.

M. SWYNGEDAUW—EFFECT OF THE CONDITION OF THE POLAR SURFACES OF AN EXCITER ON THE STATIC AND DYNAMIC EXPLOSIVE POTENTIALS.

(*Comptes Rendus, Vol.* 126, *No.* 20, *p.* 1264.)

When a succession of sparks are made to pass between the poles of an exciter which has been polished with emery, the surfaces between which the sparks pass become tarnished, owing to the oxidation of the poles. The author gives the results of investigation of the effect of this layer of oxide on the static and dynamic explosive potentials. The method of using two shunt exciters is easily applicable to these researches. The following are the experimental results obtained when the polar surfaces become tarnished, due to sparking action:—

1. The static explosive potential remains appreciably constant, or diminishes slightly.
2. The dynamic explosive potential, shows an increase which may become considerable. This sparking distance in the case of a polished exciter may be two or three times greater than that of a tarnished exciter.
3. The dynamic explosive potential may vary in large proportions from one spark to the next.

The author offers the following explanation for this phenomenon. It may be admitted that the thin layer of oxide formed by the sparks, is neither a perfect insulator nor a good conductor of electricity.

When the charge is very quickly made by the dynamic method, the electricity is distributed over each of the poles of the exciter, between the metal and the layer of oxide.

By reason of the very short duration of the dynamic charge, this coating plays the part of a solid dielectric.

In order that a discharge may take place between the poles of an exciter, the

electricity must not only pass through air, but also through the thin layer of oxide; but to pierce a solid dielectric a considerably greater P.D. is necessary than to pierce the same thickness of air. Therefore, the dynamic explosive potential of a tarnished exciter is greater than the explosive potential of a polished exciter. In the case of the static charge, the exciter is slowly charged; and the layer of oxide, which possesses certain conducting properties, plays the part of a conductor. The electricity is distributed over the free surface of the layer of oxide.

In order that the discharge may take place, it has only to pass through the solid dielectric, and so pierce a layer of air a very little thinner than if the exciter had been polished.

The static explosive potential of the tarnished exciter is a little less than that of the polished exciter. This last fact was published by M. Baille (*Ann. de Chim. et de Phys.*, 1882, 5th series, vol. xxv., p. 512).

MM. OUDIN and BARTHÉLEMY—A CROOKES TUBE FOR USE WITH ALTERNATING-CURRENT DYNAMOS.

(*Comptes Rendus*, *Vol.* 123, *No.* 26, *p.* 1269.)

The direct use of ordinary alternators and transformers for radioscopy and radiography, offers the disadvantage that their current, of approximately sinusoidal form, does not give a fixed polarity, as in the case of the Ruhmkorff coil; and that, consequently, in a Crookes tube each of the two electrodes becomes alternately cathode and anode, which results in no fixed focus, and a consequent want of sharpness of the image.

To overcome this difficulty, different shapes of tubes have been designed, the object in each case being to utilise the entire energy emanating from the alternating source of electricity. This was detrimental to the asymmetry of the electrodes, and caused the formation of parasitical foci, which spoilt the sharpness of the image. The authors have overcome these disadvantages by connecting the conducting wires to two concave aluminium electrodes placed at the extremities of the tube and facing one another, so that their foci coincide at a central point. At this point, is placed a strip of platinum, inclined at 45° to the axis, and symmetrically with regard to each of the concave mirrors. One of the mirrors acting as electrode directs the whole of the rays on to the strip, which reflects them on to one of the hemispheres of the tube; the same action takes place with the other electrode.

Under these conditions, only one alternation per second is utilised, but this is sufficient to ensure perfect steadiness in the light.

The same tube yielded very good results with high-frequency currents, such as are produced by the Tesla or D'Arsonval methods.

The platinum strip should have a greater surface than that of the mirrors, in order to arrest all the cathode rays emitted by either of the latter. Without this precaution, the rays which would pass by it would not fail to heat and melt the aluminium mirror placed in front, owing to the high intensities which are obtained with alternating currents.

F. GUILBERT—A NEW ALTERNATOR OF THE "SECTEUR DES "CHAMPS ELYSÉES."

(*L'Éclairage Électrique, Vol.* 9, *No.* 44, *p.* 133.)

The above company has been in operation since four years, and the number of lamps at present connected amount to 110,000. The plant hitherto consisted of three sets of steam alternators, including a reserve set of 400 kilowatts.

Two of these sets were constantly working, with only two stoppages of two hours per week.

In view of the strain on these machines, the company have recently installed a fly-wheel alternator. As the value of cos ϕ for the system amounts to 0·8, the true maximum power obtainable from each alternator is only 320 kilowatts; this output was not found to be sufficient, so the new 600-K.W. set was installed. This was constructed at the works of M. Joseph Farcot, of Saint-Ouen (Seine), concessionaire of the Hutin & Leblanc patents. This machine runs almost continuously. It supplies the whole day load of 480 kilowatts, and at night runs in parallel with another machine. The engine works condensing, and is similar in type to those previously installed. The cylinders are fitted with valves of the Corliss-Farcot type. It has a normal output of 650 indicated H.P., with a maximum consumption of 6·75 kgm. steam per indicated H.P., and will maintain an emergency load of 800 H.P. with a steam consumption of 7·75 kgm. of steam. The speed is 60 revolutions per minute. The alternator is of the Hutin & Leblanc type, fitted with damping coils. It is designed for an output of 600 kilowatts at 3,000 volts, on non-inductive loads, or 480 kilowatts with cos $\phi = 0·8$.

Its frequency is 40 $\sim$, and the number of poles 80. Each magnet pole is built up of iron plates 2 mm. thick, and carries, in addition to its ordinary winding, damping coils consisting of six copper bolts 530 sq. mm. section, and connected together by copper segments.

Each pole is fitted with two bolts, for the purpose of fixing it to the flanges on the rim of the fly-wheel. The field coils are arranged in four parallel circuits of 20 coils, the resistance of which is 1·9 ohms hot. The exciting current at full load on ordinary load is only 50 amperes, and when loaded with a cos ϕ of 0·8 the exciting current is increased to 80 amperes.

The diameter over the field magnets is 5·970 metres. The direct-coupled exciter is of 40-K.W. capacity, and capable of exciting several alternators. The armature has 80 coils, connected in two parallel circuits.

The iron armature segments are built up of 6-mm. plates. Each segment has six tunnels, equally spaced along the air gap to receive the winding. The winding consists of two coils of twelve turns each, connected in series, the first being wound in tunnels 1 and 6, and the second in tunnels 2 and 5; there is no winding in tunnels 3 and 4, for this coil would only add to the maximum E.M.F. without materially adding to the effective voltage. The surface of the coils, both within and without the iron, is insulated with micanite. There are two coils per armature segment. Each segment is held together by two insulated bolts, and fixed to the outer ring in the same manner that the field-magnet poles are secured to the fly-wheel.

These segments are insulated from the frame by means of ebonite [illegible] over the bolts, and from one another by means of ebonite plates 2 to 3 mm. thick.

The armature resistance is 0·43 ohm hot, and the section of the wire is 32 sq. mm. The external diameter of the armature is 6·870 metres, and its width 0·63 metre. The air gap is 8 mm.

The main characteristic of the Hutin & Leblanc alternator lies in the special armature winding, which gives a sine curve when the machine is loaded; and also in the use of damping circuits, which not only ensure stable parallel running, but also have the effect of halving the coefficient of self-induction of the armature. The action of these damping circuits can easily be studied by the aid of Leblanc's theorem. Characteristic curves are given of the alternator running on open circuit and on short-circuit. Another curve is given representing the apparent resistance of the armature circuit, with a lag of a quarter-period between the current and the induced E.M.F., and this tends towards a constant value of about 12 ohms under normal working conditions.

The author considers that the complete study of an alternator should consist of, apart from the static and short-circuit curves, a series of characteristics, either at constant excitation or at constant E.M.F., as a function of the outpout; and loaded on non-inductive resistances, or on inductive resistances with a constant lag. An illustration is given of the shape of the E.M.F. curve, obtained by a Blondel oscillograph, with the alternator loaded to 70 amperes. This curve is almost sinusoidal.

The following are the observed losses when running on a non-inductive resistance, and also on an inductive resistance, with $\cos \phi = 0{\cdot}8$:—

With non-inductive load.	Armature	Joule effect ...	17,000
		Hysteresis and Foucault currents	7,300
	Field	Winding	4,750
		Damping coils ...	6,750
			35,800

Efficiency at 600 kilowatts = 94·5 %.

With inductive load.	Armature	Joule effect ...	17,000
		Hysteresis and Foucault currents	7,300
	Field	Winding	12,000
		Damping coil ...	6,500
			42,800

Efficiency at 600 kilowatts = 91·8 %.

The combined efficiency of the set was in the latter case 83·5 per cent. The numerous trials which were made to run this alternator with the Hillairet machine previously installed were in every way successful.

The distribution of current in alternators of different design can be studied by M. Blondel's hypothesis.

i_s is the synchronising current;
i_u is the external current:
then the currents in the two alternators will be

$$i_1 = \frac{l_1}{l_1 + l_2} i_u + i_s$$

$$i_2 = \frac{l_2}{l_1 + l_2} i_u - i_s.$$

The mean inductance of the Hutin & Leblanc alternator is about 10 ohms, that of the Hillairet machine nearer 22 ohms; the internal resistances are about equal, and negligible. When these machines are coupled in parallel, and so excited as to give the same induced E.M.F., by altering the adjustment of the governors on one of the alternators, the ratio of the currents can be made equal to the ratio of the inductances, under which condition the ammeters in the alternator circuits will be absolutely steady. The minimum synchronising current is about 15 amperes, and this is partly due to the difference in the shape of the curves of the two machines.

As the engines are of the same output, it was tried to divide the load equally between them by adjustment of the governors; this worked with stability at light loads, but less so at heavy loads, the reason being due to the greater difference of phase between the induced electro-motive forces.

This is graphically represented by M. Blondel's method. It is shown that the equal division of load corresponds to a lag of about 17 degrees in the Hutin & Leblanc alternator with a load of about 750,000 apparent watts and cos = 0·8. With a load of 900.000 apparent watts and cos $\phi = 0{\cdot}8$ the equal division of load corresponds to a difference of phase of about 21 degrees between the two alternators.

It is shown that the stability, with an equal division of load between the two machines, decreases with an increase of load; in the two above cases particularly, the machines would not run properly in parallel owing to the magnitude of the synchronising currents and to the oscillations of the voltage.

No difficulty is experienced in coupling these machines in parallel, providing they are run properly to speed, and excited to the same voltage. The author concludes with the statement that for successful parallel running, the alternators should have approximately equal coefficients of self-induction.

C. E. GUYE—ELECTRICITY AT THE SWISS NATIONAL EXHIBITION: A GENERATOR FOR THE MANUFACTURE OF ALUMINIUM.

(L'Éclairage Électrique, Vol. 9, No. 48, p. 155.)

An illustration and description is given of a machine manufactured by the Œrlikon Company, and destined for the British Aluminium Company. Its output is 7,000 to 7,500 amperes at 64 to 58 volts. It runs at 150 to 130 revolutions per minute, and is designed for direct coupling to the spindle of a turbine. The weight of the machine is 34,000 kilogrammes. The field has 24 poles excited in parallel. The armature is drum, parallel-wound. The diameter of the armature is 2·4 metres, and the collector 1·7 metres. There are 120 brushes, which can be shifted by worm and wheel gear. The lead at full load is very small.

C. E. GUYE—THE NEW ELECTRIC BRAKE REGULATOR.

(*L'Éclairage Électrique, Vol.* 9, *No.* 43, *p.* 155.)

The author describes and illustrates M. J. Reiter & Co.'s brake regulator, the object of which is to quickly absorb the excess of power of an engine or motor when the load is thrown off, and when the ordinary governors do not act quickly enough.

The regulator consists of an electric brake, produced by a single or multipolar magnetic field, excited from some auxiliary source.

A solid mass of cast iron or steel is rotated in the magnetic field, and driven from the machine whose speed is to be regulated.

The brake action is caused by the Foucault currents and hysteresis produced in the outer rotating iron cylinder. The rotating part of the electric brake is coupled to an ordinary tachometer, which automatically acts on a rheostat placed in the field circuit. The arrangements are such that an increase of speed causes a diminution of resistance in the rheostat, and, consequently, an increase in the magnetism and in the brake action. The brake which is described has a single magnetising coil and eight poles.

The moving part consists of a cast-iron ring, rotating on a horizontal spindle fitted with automatic lubrication. Its speed is 400 revolutions per minute. If the speed for any reason exceeds the normal, the resistance of the rheostat diminishes automatically, and the brake can thus be made to absorb 25 H.P. without undue heating. The use of an outer rotating cylinder offers the advantage of a greater cooling effect, of having a greater linear velocity, and of obviating moving contacts for the field coils.

C. E. GUYE—ELECTRICITY AT THE SWISS NATIONAL EXHIBITION: AN ELECTRIC TRAVELLING CRANE.

(*L'Éclairage Électrique, Vol.* 9, *No.* 44, *p.* 217.)

The electric travelling crane exhibited by the Œrlikon Company had a maximum lifting power of 5 tons, and its total weight was 13 tons. It has three distinct motions. The motors driving the crane are series-wound for 250 volts. The lower motor, used for propelling the crane, is of 4 H.P., and runs at 870 revolutions, driving one of the spindles by means of worm and wheel gear. It receives its current by means of contacts laid along the track (Claret & Vuilleumier system). The current can, however, be also applied to the motors by means of a trolley placed above the crane, and independent of its motion of rotation. The second motor, producing this motion, is of 1·6 H.P., and runs at 1,400 revolutions, and is fitted with ball bearings. The motor used for working the load and lowering the arm of the crane is of 12 H.P. (at 700 revolutions). In the latter case the position of the load remains the same; thus allowing the crane to pass through the doors of a workshop.

The method employed for stopping the load at any desired position consists in placing an electro-magnet in the motor circuit, which, by its action, releases a spring brake, which acts on the spindle of the motor. All the above motions are easily obtained by means of three rheostats and a lever, placed on the crane itself.

C. E. GUYE—THE NEW ACCUMULATORS OF THE GERMANO-SWISS COMPANY OF FRIBOURG.

(*L'Éclairage Électrique, Vol.* 9, *No.* 44, *p.* 217.)

This company exhibited a battery of 140 cells, having a total capacity of 1,500 ampere-hours. The charging current was 150 amperes, corresponding to 2 amperes per kilogramme of electrode.

The maximum discharge current was 240 amperes. Illustrations are given showing the construction of the accumulator.

Its characteristics, lie mainly in its great capacity and its insensitiveness to great rates of discharge. Its efficiency is also high. These advantages are obtained by means of a special construction of the electrode, affording strength and at the same time reducing weight. The weight of active material per sq. cm. of plate is considerable, owing to the use of an outer case of perforated celluloid, together with a porous envelope preventing any fall of active material and yet allowing free access of the electrolyte.

The electrode consists of a grid or core of lead and antimony, to the faces and interior of which is fixed the active material, consisting of minium for the positive plates or litharge for the negative plates. This active material is held in position partly by the grid and partly by the above perforated sheaths, which are made in two halves and jointed when in position.

The plate is made up by applying to the interior of one half of the sheath, half the quantity of the oxide of lead required for the electrode. The grid is then applied to the layer of oxide ; the second portion of the oxide is then laid over the grid; the porous sheath is then placed in position, and finally the outer half of the celluloid sheath. The electrodes are then fixed together and immersed into a special bath, depending on whether the electrodes are positive or negative. After several minutes' soaking in the bath, the plate is submitted to a certain pressure, depending on its size. This pressure has the object of imparting the desired consistency to the oxide, which has become pasty through immersion, and at the same time allowing the superfluous liquid to run off. The electrodes are then dried under pressure and allowed free access to air for several days.

The positive plates are separated from the negative plates by means of celluloid rods fixed to the positive plates themselves.

The plates are formed in a bath of sulphuric acid, at a temperature of 28° to 24° Baumé. After 10 to 12 hours in this bath, the positive plates are made up into elements with the negative plates, and formed with them.

The first forming charge is at the rate of 3 amperes per kilogramme of plate for 20 hours, then at the rate of 2 amperes for 2 hours, then at the rate of 1 ampere, making altogether 24 hours. The first discharge should take place directly afterwards, at the rate of ½ ampere per kilogramme of plate, and is continued until the voltage has dropped to 1·8 volts. The capacity of this first charge is from 9 to 10 ampere-hours per kilogramme of plate.

The second charge is at the rate of 3 amperes per kilogramme of plate for 10 hours, after which the discharge is at the rate of 1 ampere per kilogramme. The capacity reaches about 14 ampere-hours. The third and following forming charges

take place at the same rate as the second. After five or six charges one obtains 20 ampere-hours per kilogramme (being the guaranteed capacity for stable batteries with a drop of 10 per cent.). With a successful formation, this capacity reaches 23 to 20 ampere-hours per kilogramme of plate as far as 1·8 volts. In the case of a charge of 1 ampere per kilogramme of electrode the mean charging voltage is about 2·3 volts, and the maximum charging voltage about 2·6 volts. The mean discharge voltage at 1 ampere per kilogramme of electrode is 1·97 to 1·98 volts, and the initial discharge voltage is 2·07 to 2·08 volts.

The efficiency is 92 per cent. in ampere-hours and 78 per cent. in watt-hours with a charge of 1 ampere per kilogramme of electrode and a discharge of 1 ampere per kilogramme of electrode.

With a charge and discharge rate of 2 amperes, the efficiencies are respectively 87 and 70 per cent.; and at 3 amperes the efficiencies are 77 and 62 per cent. respectively. The maximum charging current is 3 amperes per kilogramme, and the maximum discharge current 3·5 amperes per kilogramme.

The discharge rate can be pushed as far as 10 amperes per kilogramme of electrode, without fear of disintegration, in the case of cells specially constructed for traction purposes. These cells are fitted with a special lid to prevent any spilling of the acid, which consists of two celluloid plates kept apart by means of celluloid rods. The top plate has one, two, or four holes at the sides, and the lower plate has holes at the centre. By this method, any liquid thrown through the latter hole will not find its way out of the cell, so long as the tilting is of short duration.

E. VAN AUBEL—ON THE TRANSPARENCE OF BODIES TO THE "x" RAYS.

(*Journal de Physique, Vol. 5, November,* 1896, *p.* 511.)

With regard to the comparison of transparence of bodies to the x rays and to heat, the experiments of MM. Meslans, V. Novak, and O. Sulc show that the presence of fluorine, chlorine, and bromine, and especially iodine, in a molecule increases its opacity to the x rays. Certain facts quoted by the author prove that the presence of halogens and of sulphur in the molecule increase the opacity both to the x rays and to heat. The author further verified this conclusion by examining this transparence of tetrachlorides of carbon, of silicium, chloroform, and of sulphide of carbon to the radiations of a Colardeau tube.

The liquids were contained in vessels consisting of a perforated cork disc cemented to cardboard. These were filled with their respective liquids, and placed above a photographic plate wrapped up in black paper. One of these vessels contained water, for purposes of comparison.

MM. Bleunard and Labesse have found that silicium, which is transparent to the radiations of the Crookes tube, appears to communicate its transparence to amorphous silicon. The influence of oxygen on the calorific absorption is clearly shown by comparing, with M. Zsigmondy, the tetrachloride of silicium and of quartz (chloride and oxide of silicium). Bodies containing carbon, hydrogen, and oxygen allow the Röntgen rays to easily pass through them, whereas they absorb heat rays to a greater degree, as is shown by M. Ch. Friedel's researches.

With regard to the transparence of vapours to the x rays, Mr. Phillips has shown that the flame of a Bunsen burner is absolutely transparent to the x rays The opacity of powdered salts to these same rays increases with the weight of the metal which enters into the composition of the salt. This was verified by the author when working with chloride of thallium, a thin piece of this salt being placed on a photographic plate. Vapours from the fused salt imparting a green colour to the flame, produced no image on the photographic plate; chloride of sodium yielded the same result, as did also bromide of potassium, although this last salt, as shown in MM. Bleunard and Labesse's experiments, is very opaque to the rays. No effects were obtained with iodine vapours, although iodine itself is very opaque. The author is carrying out further research in this direction.

M. L. BENOIST—THE THREE-GOLD-LEAF ELECTROSCOPE.

(*Journal de Physique, Vol.* 5, *October*, 1896, *p.* 491.)

The gold-leaf electroscope, fitted with a metallic cage and with a practically perfect insulator such as dielectrine (Hurmuzescu electroscope), may be of great utility as an idiostatic apparatus, in the case of fairly high potentials, and when an accuracy of 1-100 is required.

By means of a simple modification introduced by the author, the sensitiveness is increased, and also its range. This modification consists in using three gold leaves instead of two. After having cut them out to the same dimensions, they are superposed and fixed together at one end by means of a strip of tinfoil, which is used for connecting them to the insulated rod of the electroscope. When the apparatus is charged, the centre leaf remains vertical, and is of use as an origin for measuring the angles, the deflection of the two outer leaves being equal. The deflection can easily be measured to $\frac{1}{4}$ degree by the aid of a transparent scale fixed to the instrument. The reading is taken by means of a telescope.

The sensitiveness is considerably increased by the addition of a third leaf, for each extreme leaf is repelled four times more strongly by the middle leaf than by the opposite leaf; and although the charge is divided between three leaves instead of two, yet a weaker charge will produce the same total deflection.

By simple calculation, it is found that for small angles the sensitiveness is increased in the ratio of 1 to 1·49; and this increases with the angle, it being 1·5 for an angle of 10°, and 1·66 for an angle of 60°. Further, in the case of a two-leaf electroscope the sensitiveness becomes *nil* in the neighbourhood of the limiting angle of 90°, for any increment in the charge does not cause any increase in the deflection; it can only cause a detachment of the leaves. With three leaves, however, the limiting angle is 120°. This electroscope can therefore be used for higher potentials.

M. H. BAGARD—THE HALL PHENOMENON IN LIQUIDS.

(*Journal de Physique, Vol.* 5, *November*, 1896, *p.* 499.)

Some months ago the author published some notes on the Hall phenomenon in liquids; he has since then made numerous experiments on this subject. In order to

eliminate the disturbances due to heating, he found it necessary to work with thicker layers of liquid, and in this way it was found possible to obtain a steady effect after the current had been on for an hour. These researches were carried out with layers of liquid 1 cm. thick, and the Hall effect, although much reduced, was, however, still marked. Experiments were made on recently boiled solutions of sulphate of copper, or on sulphate of copper of different strengths, kept at the temperature of the laboratory, the liquid being contained in a small box consisting of glass plates cemented together; the dimensions of the portion under experiment being 1 cm. thick, 40 mm. long, and 38 mm. wide. The electrodes consist of amalgamated zinc or copper, placed about 76 mm. apart. The whole vessel dips into a bath of distilled water, kept at atmospheric temperature, and is then placed between the poles of a Faraday electro-magnet the poles of which consist of two cylinders 7 cm. diameter and 24 mm. thick, their distance apart being 3 cm. Each of the liquids was subjected to three field strengths (viz., 385, 707, and 962 C.G.S. units), measured by the bismuth spiral method.

The current employed in these experiments was lower than 0·08 ampere, and was kept on for an hour before commencing to take observations. The electro-magnets were excited in alternate directions. At the end of the fourth minute the difference V a – V b becomes steady. The difference V c – V d is measured towards the end of the period, and V a – V b is measured at the end of the tenth minute, at the moment of reversing the current in the electro-magnet.

In his previous researches, when working with a layer of liquid 1·6 mm. thick, the author found that an increase of temperature accounted for the gradual increase of the Hall effect.

In the present experiment the Hall effect generally settles down to a definite value with a given current and magnetic field.

These experiments have led to the general result that, as was mentioned in the author's previous notes, the Hall effect is produced in the same sense as with bismuth; that is to say, the electric force is deflected in a sense which is opposite to that of the current in the electro-magnet.

The results of these experiments are included in several tables, and from which the following conclusions are arrived at:—

The Hall effect increases—

(1) When the percentage of salt in solution diminishes;

(2) When the current-density increases;

(3) When the strength of the field increases.

E. VAN AUBEL—ON M. H. BECQUEREL'S FORMULA RELATING TO MAGNETIC ROTARY POLARISATION.

(Journal de Physique, Vol. 5, November, 1896, p. 509.)

In a memoir published in the *Annales de Chimie et Physique* (5th series, vol. xii., p. 5, 1877) M. H. Becquerel arrived at the following results:—

1. The positive rotation of the plane of polarisation of a beam of light of a given wave-length, passing through unit thickness of a substance submitted to the magnetic action, is approximately proportional to

the function, $n^2 (n^2 - 1)$, of the corresponding index of refraction, and to a factor connected with the magnetism or with the diamagnetism of the bodies. This factor is the greater the more diamagnetic the bodies.

2. With substances belonging to the same chemical group, or with different compounds of the same chemical radicle, the quotient of the magnetic rotation, R, by the product, $n^2 (n^2 - 1)$, of the corresponding index of refraction is a value which is almost constant.

The author has worked with the object of ascertaining how the above constant varies for a given substance under the influence of temperature. The results of his calculations are embodied in tables. The researches of J. W. Rodger and W. Watson furnish data of the magnetic rotation of the plane of polarisation of light in sulphide of carbon and water with the sodium flame, and the experiments of Ketteler give the indices of refraction of these two liquids.

The results show that the influence of temperature on H. Becquerel's constant differs for the two liquids examined. The quantity $\frac{R}{n^2 (n^2 - 1)}$ increases with the temperature in a marked manner, especially in the case of sulphide of carbon; and with a given temperature, the values of this ratio differ from one liquid to another as the temperature increases.

The author made further experiments on different qualities of Jena glass manufactured by the firm of Schott, in order to verify the relation $\frac{R}{n^2 (n^2 - 1)} =$ constant, which H. Becquerel has studied for crown and different qualities of flint glass.

The results of these experiments are embodied in a table giving the indices of refraction for the sodium flame at a temperature of 181°, and also Verdet's constants ω D in absolute measure for the same light and temperature, and, lastly, the values $\frac{\omega' D}{n^2 (n^2 - 1)}$.

The results show that for different qualities of crown glass the above ratio varies from 471·7 to 772, and for flint glass from 862 to 965·5.

H. Becquerel had obtained $\frac{R}{n^2 (n^2 - 1)} = 0{\cdot}155$ for crown glass, and values varying between 0·207 and 0·234 for flint glass.

ED. DEFACQZ—ON THE REDUCTION OF WOLFRAM BY CARBON IN THE ELECTRIC FURNACE.

(*Comptes Rendus, Vol.* 123, *No.* 26, *p.* 1288.)

When reducing pure tungstic acid by means of sugar carbon, in the electric furnace, M. Moissan (*Comptes Rendus*, vol. cxxiii., p. 13) obtained pure tungsten, the properties and analysis of which have been published. The author has repeated these experiments on the tungsten ore, wolfram, the samples having been obtained from Zumwald (Bohemia). An analysis was made on an average sample. For the reduction of the wolfram, a mixture of wolfram and sugar carbon is prepared, containing about 14 per cent. of the ore; the whole is placed in M. Moissan's

electric furnace, and submitted for 12 minutes to the heating action of a 950- to 1,000-ampere arc at 50 to 60 volts. The resulting metallic mass has the following composition :—

	1.	2.
Tungsten	92·53	92·65
Silicium	0·49	0·51
Iron	2·37	2·15
Total carbon	5·21	4·96

From these analyses of the sample of wolfram, the author concludes that it can be reduced with facility, with the production of a fairly pure metal. Manganese and calcium have completely disappeared; silicium and iron have decreased in a large proportion.

The reactions are produced partly by the action of the high temperature of the electric furnace, and partly by the dross which is formed.

These researches show that the direct treatment of ores in the electric furnace is capable of producing metals of sufficient purity to become of direct practical utility.

M. E. COLARDEAU—ON A FORM OF CROOKES TUBE PRODUCING PHOTOGRAPHIC IMAGES OF GREAT SHARPNESS WITH SHORT EXPOSURES.

(*Comptes Rendus, Vol.* 5, *3rd Series, December*, 1896, *p.* 542.)

The condition for ensuring maximum sharpness with a Crookes tube, is that the source of emission should be as comparable to a point as possible. This condition was not realised in the first Crookes tubes. The cathode rays covered a large portion of the surface of the tube, and it was the whole of this portion which emitted the x rays. For this reason the image lacked sharpness.

This has been obviated to a large degree by the use of concave cathodes, which render the cathode beam convergent. By cutting this beam at its narrowest place by the surface which is to emit the x rays, the latter becomes active only over a small area, and the images gain in sharpness. This concentration of the cathode beam is accompanied, however, by a considerable rise of temperature, which is sufficient to melt the glass bulb against which the rays are concentrated.

M. Perrin has shown that all solid bodies placed within the tube become a centre of emission of the x rays, when struck by the cathode flux. It is on this principle that the so-called focus tubes are constructed, and which yield with short exposures, much better results than did the old tubes. The shape and dimensions of tube usually employed present the following disadvantages:—

1. The thickness of glass must be sufficiently strong to withstand external pressure, but glass is not very transparent to the x rays.
2. The space within the tube is of large dimensions. The discharge fills the whole of this space. It would be advantageous to localise the energy of the discharge as much as possible within the cone described between the cathode and the focus.

3. The length of path of these rays within the tube is of importance. The author made experiments with tubes in which the cathode could be placed at different distances from the anti-cathode region.

   When this distance is very small (1 cm., for instance), the tube heats and rapidly alters, but it is photographically very active; the action, however, is considerably weakened when the distance is made 15 to 20 cm.

4. The author studied the shape of the cathode flux in a cylindric tube with a concave cathode, by intercepting the flux at various places with substances such as chalk or Iceland spar, which become luminous when in the path of the rays. The experiments showed that the point where the flux is most strictured is often much beyond the centre of curvature, and that its position varies with the degree of vacuum.

   Consequently, the focusing strip which is placed at the centre of curvature of the cathode is intercepted by the flux, not at a single point, but over a fairly large area. The x rays are emitted by nearly the whole area of this strip, causing a want of sharpness when the distance between the sensitive plate and the object to be photographed amounts to an appreciable value.

The author's tube was constructed with the object of overcoming these various objections. It is of a cylindric form, and its diameter is not more than 6 to 7 mm.

A cathode 4 mm. in diameter is used, placed at just sufficient distance from the glass to prevent heating. The cathode is concave, and its radius of curvature is about ½ cm. At a distance of 7 to 8 mm. is placed the focus, inclined to 45°, as usual, to the axis of the tube. In order that the x rays may pass out of the tube with as little resistance as possible, this part is blown to form a small bulb 1-10th mm. in thickness in front of the focus, and on the active side of the slip. The action of the discharge in such a tube would rapidly alter the degree of vacuum; it is therefore necessary to either have it connected to the mercury pump, or to connect it to a vessel of larger capacity. The good results obtained with such tubes show that the centre of emission of the x rays is evidently very small. By employing an induction coil with a 5- to 6-cm. spark with the above tube, good silhouettes of the hand are to be obtained with one minute's exposure; and when using a 25- to 30-cm. spark and one discharge, photographs of a child's hand were obtained. The duration of this exposure was measured, and found to be 1-1,000th of a second.

Stereoscopic photographs were obtained by taking two negatives of the same object, with different positions of the tube.

M. C. DUPERRAY—ON THE OPTICAL PROPERTIES OF A CYLINDER OF GLASS RAPIDLY ROTATING IN A MAGNETIC FIELD.

(Journal de Physique, 3rd Series, Vol. 5, December, 1896, p. 540.)

M. Villari published (*Pogg. Ann.*, vol. cxlix., p. 324) the results of an experiment made by him in 1873, and from which he concluded that it requires an

appreciable time (1-800th of a second) for flint to acquire the rotary magnetic power.

M. Villari caused a flint cylinder to rapidly rotate between the poles of an electro-magnet, the axis of the cylinder being perpendicular to that of the poles. If the cylinder be stationary, on exciting the electro-magnet a certain rotation of the plane of polarisation is observed.

According to M. Villari, this rotation would decrease with an increase of speed, and would disappear with a speed of 200 revolutions per minute. The author repeated these experiments, and found that the rotary power remained independent of the speed of rotation, this being contradictory to M. Villari's results.

1. The author studied the effect of rotation alone, without a magnetic field on the optical property of the cylinder. A beam which is polarised in a rectilinear direction gives rise, on leaving the rotating cylinder, to an elliptically polarised beam, and which cannot be annulled by the Nicol prism. This is, however, not the case if the plane of polarisation of the incident beam is parallel or perpendicular to the axis; in this case the vibrations remain rectilinear. Disturbances were experienced, due to the centrifugal force developed in the rotating cylinder, and to vibrations set up due to the motion of rotation.

2. The electro-magnet was excited with the cylinder at rest, the light being polarised in a plane perpendicular or parallel to the axis of rotation. The rotation of the plane of polarisation, on leaving the cylinder, was measured by means of a Laurent saccharimeter. A fairly weak field was employed, in order that this rotation should be only a few degrees (about 5).

By this means, the light passing through the cylinder always remained polarised, very nearly parallel or at right angles to the axis of rotation, and the effect of accidental double refraction becomes negligible. The cylinder was then made to rotate at speeds of 200 or more revolutions per second, and a new measurement taken of the rotation of the plane of polarisation: it was found to be exactly the same as when the cylinder was stationary. This experiment is therefore in no way contradictory to the results of previous experiments. Perhaps the accidental double refraction had influenced M. Villari's experimental results.

L. LOMBARDI—ABSOLUTE MEASUREMENT OF THE CAPACITY OF CONDENSERS BY MEANS OF ALTERNATE CURRENTS.

(*L'Elettricista*, 5, *pp.* 1–8, 25–32, 1896; *Beiblätter*, *Vol.* 20, *No.* 6, *p.* 546.)

Capacity measurements made on condensers of different types and with different dielectrics, by means of alternating currents the E.M.F. curve of which was exactly determined, showed that with medium frequencies the apparent value of the capacity was different from and smaller than that with continuous pressures and considerable times of charging. The capacity is somewhat dependent on the frequency, but its variation, with good condensers, can, for practical purposes, be made inappreciable.

—HAGA—A DAMPER FOR A QUADRANT ELECTROMETER NEEDLE.

(*Handelingen van het vijfde Natuur en Geneesk. Congres, Amsterdam*, 1895, *pp.* 130–132; *Beiblätter, Vol.* 20, *No.* 6, *p.* 548.)

The damping of the electrometer needle is effected by attaching a copper cylinder to the needle system, this cylinder moving in a permanent magnetic field. The damping can be regulated exactly, and an aperiodic motion easily obtained. With a quartz fibre of 55 mikron, a degree of accuracy of 1-1,000 was obtained in the measurement of 1 volt.

A. COEHN—ON THE ELECTROLYTIC SOLUTION AND SEPARATION OF CARBON.

F. VOGEL—NOTES ON THE ELECTROLYTIC SOLUTION OF CARBON.

(*Zeitsch. f. Elektrochem.*, 2, *pp.* 541–542, 581–582, 1896; *Beiblätter, Vol.* 20, *No.* 6, *pp.* 552, 553.)

Dr. Coehn found, when electrolysing dilute H_2SO_4 at elevated temperatures, using carbon electrodes, that the H_2SO_4 was coloured dark red, and contained carbon ions which could be deposited on a platinum electrode like a metal.

When lead peroxide in the form of a charged accumulator plate is placed opposite to the plate, an element is obtained in which carbon forms the soluble electrode. This element will yield a strong and constant current, and gives 1·03 volts when the external resistance is 100 ohms, the action lasting until the accumulator plate is discharged.

These results are criticised by Herr Vogel, who came to the following conclusions, viz.:—1. Heretofore no one has succeeded in bringing carbon into solution electrolytically, and it is very improbable that this should be effected. 2. Heretofore carbon has not been separated from a solution of carbon compounds as a cation, but only as a component part of a cation; Faraday's law has been proved to be true when carbon alone is either cation or anion. 3. Elements whose soluble electrode consisted of carbon have already been successfully made by Bartoli and Papasogli (see *Nuovo Cim.* (3), 8, p. 278, 1880, and other references), and were improved by Coehn.

O. MURANI—ON THE INFLUENCE OF VIBRATION ON THE ELECTRIC RESISTANCE OF METALLIC WIRES.

(*Beiblätter, Vol.* 20, *No.* 6, *p.* 561.)

The author measured the resistance of vertical stretched copper, iron, steel, platinum, German silver, and manganin wires, which were set in transverse vibration by a tuning-fork connected to their lower end by means of a silk thread, whilst they could be stretched to different tensions from their upper end. They were connected to a Wheatstone's bridge by two thin copper wires soldered

thereto; the tension of the wire was so adjusted that the places where the copper spirals were attached were at the nodes, so as not to impede the vibrations. When temperature variations and like disturbing influences were eliminated, the author found that transverse vibrations exert very little influence on the resistance; the variations observed scarcely amounted to $1{\cdot}5 \times 10^{-8}$ per unit of the original value.

R. IHLE—ON THE FORMATION OF AMMONIA IN THE ELECTROLYSIS OF NITRIC ACID.

(*Zeitsch. f. Physik. Chem.*, 19, *pp.* 572–577, 1896; *Beiblätter, Vol.* 20, *No.* 8, *p.* 708.)

In the electrolysis of nitric acid the H ions migrate to the cathode. They either give up their charges thereto and pass into the gaseous condition (polarisation), or they combine with the hydroxyl ions separated from the nitric acid to form undissociated water, whilst the positive charges set free on the nitrogen simultaneously pass over to the electrode (no polarisation). On the further generation of free H ions, the nitric acid is caused to give up one hydroxyl after another, until finally uncombined nitrogen remains, which either escapes as such, or, if sufficient hydrogen be present, combines therewith to form ammonia. The reducing action at the cathode is dependent on (1) the current-density, and (2) on the oxidising power of the nitric acid, or the rate at which it can give up (H O) ions. This depends both on the concentration as well as on the presence of nitrous acid, which in a great measure promotes the oxidising power of nitric acid. In order to determine whether the one or the other action proceeds the more rapidly—the separation of H by the current, or the splitting off of hydroxyl—it is necessary to observe the generation of hydrogen, and the formation of NH_3 at the cathode. In order to test these relations, the author carried out a series of experiments, from which it appeared that the oxidising power increases with the concentration, whilst the reducing power of the hydrogen obtained at the cathode increases with the current-density. Consequently, by varying the current-density, one is enabled to obtain a stronger or weaker reducing action as desired.

K. BIRKELAND—ON CATHODE RAYS UNDER THE ACTION OF STRONG MAGNETIC FORCES.

(*Beiblätter, Vol.* 20, *No.* 9, *p* 802.)

The author brought different Crookes tubes with aluminium crosses and platinum plates in front of the pole of a powerful electro-magnet, and found that the cathode rays especially extend themselves along the magnetic lines of force. The degree of exhaustion was estimated by the sparking distance between balls 1 cm. in diameter; this amounted in various experiments to from 5 to 50 mm.

When a divergent pencil of magnetic lines of force was directed normally against an aluminium cross serving as the cathode, a pencil of cathode rays was shown, by its phosphorescent image on the glass wall of the tube, both in the direction of the magnet and in the opposite direction; these images were rotated relatively to one another through an angle of about 10°. When acting axially on a concave cathode, the lines of force reduced the divergence; with a flat cathode disc the pencil can be brought to a sharp point. When a tube with a Maltese cross was brought axially up to a magnet pole, the dark cross on the opposite wall decreased continuously in area, and was simultaneously rotated in a clockwise direction, looking along the lines of force.

F. W. DWELSHAUVERS-DERY—ON THE REFLECTION OF THE "x" RAYS.

(*Beiblätter*, *Vol.* 20, *No.* 9, *p.* 809.)

The author, from various experiments, comes to the conclusion that—

1. The diffuse reflection mentioned by various physicists is caused, at least partly, by the generation of rays in the substance experimented upon which are different from the x rays; there is consequently no true reflection.

2. Geometrical reflection does not exist.

3. The x rays produce phenomena of a different kind in metals; they produce rays of greater wave-length, together with electrical and thermal effects.

STRECKER and KARRASS—ON AN IMPROVED METHOD OF SOLDERING THE ELECTRODES OF ACCUMULATORS.

(*Beiblätter*, *Vol.* 20, *No.* 9, *p.* 813.)

The lead strips which are used as connections are immersed in strong potash, and then washed in water. The lugs of the electrodes and the lead strips are then carefully scraped clean at the joints immediately before soldering. Lead, heated considerably above the melting point, is then poured into a pair of tongs which surrounds the joint below and at the sides, and serves as a mould.

E. ORLICH—ON THE POLARISATION CAPACITY OF MERCURY ELECTRODES.

(*Beiblatter*, *Vol.* 20, *No.* 10, *p.* 896.)

The result of these researches may be stated briefly as follows:—

1. The determination of capacity by alternate currents is a convenient and reliable method, which gives results which agree perfectly with figures obtained by other methods.

2. The effect of depolarisation cannot be eliminated; an alteration in the phase of the E.M.F. takes place which complicates the measurement and makes the capacity appear too great.

3. Of the solutions investigated, chlorides have the greatest capacity; then follow nitrates, and then sulphates.

4. By this method, measurements of capacity can be made on electrodes which are already polarised. With sulphates and nitrates the capacity first increases with the polarisation. At about 0·5 volt a reversal takes place, when the capacity rapidly decreases.

5. The measurements on chlorides were disturbed by the formation of a layer of calomel; the polarised cathode surface behaves similarly to that with the nitrates and sulphates.

— VOGEL—RECENT ADVANCES IN THE MANUFACTURE OF ACCUMULATORS.

(*Beiblätter*, *Vol.* 20, *No.* 10, *p.* 897.)

The author discusses a number of new patents in which a greater strength is imparted to the active material by the addition thereto of organic substances, such as glycerine, vegetable alkaloids, chrome gelatine, &c. In the discussion, Messrs. Pollak, Sieg, Liebenon, and Correns agreed that the capacity of accumulators is increased by the presence of organic substances, but at the cost of durability. The organic lead salts are gradually destroyed by charging, and the previously hard material becomes loose and disintegrates. In practice only those accumulators are of value in which chemically pure lead and chemically pure salts are employed.

G. BRION—ON THE TRANSITION OF CARBON FROM THE NON-CONDUCTING TO THE CONDUCTING CONDITION.

(*Wiedemann's Annalen*, *Vol.* 59, *No.* 12, *p.* 715.)

Whilst graphitic carbons conduct, solid carbon allotropes and compounds, such as the diamond, pure wood-charcoal, and coal, are non-conductors of electricity. From the manufacture of lighting gas and incandescent lamps, it is known that all carbonaceous substances will become conducting if they are subjected to a very high temperature, air being excluded. The author investigated at what temperature, and in what manner, this transition of carbon from the non-conducting to the conducting modification takes place.

In the experiments, the results of which are given in the following table, the material was left in a porcelain boat in the furnace for 15–20 minutes at the temperatures stated. The fibres were quickly removed, their temperature measured, and then replaced in the furnace to be subjected to a higher temperature.

Temperature.	Resistance in Siemens Units at the Temperature of the Room.		
Centigrade.	Broad Bamboo Fibres.	Thick Round Cellulose Fibres.	Thin Round Cellulose Fibres.
695°	No observable conduction.		
735°	10,400 9,800	8,500 6,900 8,400	over 10,000
745°	6,200	7,100 5,800	
765°	...	830	
780°	970	620 790 835	3,400 3,600 3,300
800°	345 310	600 450 480	
820°	147 153	195 198 189	730 770 745
920°	71 68	89 95 88	350 380 370
980°	45 42	60 58 61	
1,060°	37 33	56 48 53	198 190 195

The author found that, after cooling, the resistance of the carbonised fibres gradually increased; the resistance, in the case of material carbonised at a low temperature (735° C.), increased, after 100 hours, to from three to four times the value which it had immediately after carbonisation; when strongly heated during carbonisation (to 820° C.), the resistance, after 40 hours, only increased to twice the value to which it had immediately after carbonisation. In each case, a limiting value was approximated to after a certain time, the increase being least and the limiting value most rapidly obtained in the case of bamboo fibres.

W. LÖB—THE USE OF POROUS CARBON CYLINDERS IN ELECTROLYTIC RESEARCHES.

(*Elektrotechnische Zeitschrift, Vol.* 17, *No.* 47, *p.* 725.)

The author, in some researches which he made on the synthesis of organic compounds, found it advantageous to replace a clay porous cylinder by a cylinder of compressed carbon, in order to reduce the internal resistance of the cell.

Whilst the non-conducting clay cylinder, excepting the increase in the internal resistance of the cell, did not produce any essential alteration in the electrical phenomena, the carbon cylinder, whose conductivity approximated to that of the metals, acted as a kind of intermediate conductor when employed in place of the clay cylinder. Electrolysis takes place at both sides of the carbon cylinder, and, when the anode dips into the external electrolyte, the outside of the carbon cylinder is the cathode, and the inside the anode. Moreover, it was found that electrolysis always took place in the interior of a carbon cylinder used as an electrode.

The results of the experiments were as follows:—

1. A carbon cylinder, employed in place of a clay cylinder, simultaneously possesses the properties of a permeable partition and a metallic intermediate conductor; that is to say, free passage of the ions takes place through its pores, whilst at its solid parts electrolysis, and the formation of an anode and a cathode, take place.
2. When a carbon cylinder is employed as the cathode (or anode), the whole surface, including both the in- and outside, forms the cathode (or anode).
3. If the cathode (or anode) be brought into metallic contact with the carbon cylinder, so that they are both at the same potential, the whole system acts as cathode (or anode); the surface only being increased.

UMBREIT and MATHES—CUPRON CELL.

(Elektrotechnische Zeitschrift, Vol. 17, No. 36, p. 572.)

The cupron cell is an improved Lalande cell (copper oxide-alkali-zinc). In the containing vessel are two zinc plates, and between them a plate of porous copper oxide, the exciting liquid consisting of a potash or soda solution of 19° to 21° Bé. The chemical action which takes place may be indicated by the following formula:—

$$Cu\,O + Na_2\,O\,.\,Aq. + Zn = Cu + Na_2\,O\,.\,Aq.\,Zn\,(O\,H)_2.$$

The initial E.M.F. is 1·0 to 1·1 volts for the first few minutes, the normal E.M.F. being 0·85 volt; the higher E.M.F. is due to the presence of free oxygen condensed in the pores of the copper oxide plate. To regenerate the cell when run down it is only necessary to wash the plates, and leave them in a warm, dry place for from 20 to 24 hours, and to replace the exhausted solution. The internal resistance of the cell is very low, viz., 0·06 ω for a cell with 1 plate 120 × 100 mm. (?), this cell being capable of giving 40–50 ampere-hours without recharging.

L. SCHRÖDER—ELECTRIC TRAMWAY SYSTEMS WITH STATIONARY ACCUMULATORS.

(Elektrotechnische Zeitschrift, Vol. 17, No. 53, p. 805.)

The author describes first the results obtained on the Zürich-Hirslanden line, in which stationary accumulators have been employed for two and a half years. During

the last nine months these accumulators have been used directly in parallel with the dynamo supplying the line with current, without the interposition of any automatic regulating devices. The results have been very satisfactory both as regards equalisation of the load on the engine and in the reduction of the coal bill. In one series of observations, extending over 24 minutes, whilst the current supplied to the line varied between 20 and 210 amperes, the current in the dynamo circuit only varied between 72 and 102 amperes, the P.D. on the line meanwhile varying between 535 and 560 volts. The engine is of 90 H.P., and the coal consumption 1·1 kilogrammes per tram kilometre. The accumulators after two and a half years' work show no signs of deterioration. Further particulars of this line are given in the *Electrotechnische Zeitschrift* for 1894, No. 26.

Particulars are also given of the Meckenbeuren-Tettnang electric railway, in which an accumulator battery is also employed directly in parallel with the dynamo, which is a compound-wound machine capable of working at 700 volts. The accumulators are thoroughly charged up once every day. In this case the P.D. only varied between 630 and 680 volts, and, when charging up, to 700 volts. When the accumulators were not in use the P.D. varied between 300 and 800 volts.

The electric tramway at Remscheid is also described. In this case the accumulators are in series with the armature of an auxiliary dynamo the field magnet of which is excited by two differentially wound coils, one of these coils being in the main circuit of the dynamo supplying the line with current, the other being a shunt across the terminals of the accumulator battery. When the current supplied to the line is equal to the capacity of the main dynamo, the accumulator should neither give nor receive current, and the E.M.F. of the auxiliary dynamo should be zero; when the current required by the line is less than the capacity of the main dynamo, the auxiliary dynamo causes the accumulators to be charged; when greater, the accumulators send current into the line. Thus the main dynamo always works at full load. This system is patented by Messrs. Siemens & Halske. In the Remscheid installation, whilst the line current varied between 100 and 460 amperes, the current in the main dynamo only varied between 210 and 255 amperes, the variation in the voltage being about 10 volts.

CLASSIFIED LIST OF ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM

Appearing in some of the principal Technical Journals during the Month of DECEMBER, 1896.

S. denotes a series of articles. I. denotes fully illustrated.

ELECTRIC LIGHT AND POWER.

ANON.—The Electric Light and Power Installation for the Chief Works and Railway Stations at Gleiwitz, in Silesia.—*E. T. Z.*, part 49, December, 1896, p. 742 (I.).

Dr. O. GUSINDE—The Solution of the Glow Lamp Question.—*Ibid.*, part 52. p. 786.

H. RIGGERT—Results of Glow Lamp Tests.—*Ibid.*, part 53, p. 797 (I.).

ANON.—Graphic Comparison of Two Electricity Works.—*Ibid.*, p. 801 (I.).

O. SCHOTT—On Electric Capillary Light.—*Wied. Ann.*, No. 12, vol. 59, p. 768.

C. E. GUYE—Electricity at the Swiss National Exhibition: The New Thury Regulator for Series Motors; The Schneider Accumulator Switch.—*Ecl. El.*, vol. 9, No. 49, p. 438 (S. I.).

E. BOISTEL—The Arc Light.—*Ibid.*, p. 446 (S. I.).

C. E. GUYE—Electricity at the Swiss National Exhibition: The Sulzer Centrifugal Pump; The Alioth Soldering Machine; Electrical Heating and Cooking Apparatus.—*Ibid.*, No. 50, p. 481 (S. I.).

G. RICHARD—Mechanical Applications of Electricity: Elevators (Otis, Herdman, Warner); Fiegehen Overhead Traveller; Walker Electro-magnetic Coupling; Holmes Portable Electric Drill.—*Ibid.*, p. 484 (S. I.).

J. BLONDIN—The Swiss Electric Installations from Geneva to Zurich: The Electric Tramways and the Central Station of Lausanne; The Jura-Simplon Hydraulic Station, and Accumulator Charging Station at Fribourg.—*Ibid.*, p. 490.

The Val de Travers Installations; The Coombe-Garrot Station; The Clées Station.—*Ibid.*, No. 51, p. 548.

Central Station of the Municipal Installations of Berne; The Hydraulic Station of Wynau.—*Ibid.*, No. 52, p. 590.

M. A. BOCHET—The Calculation of Electrical Conductors.—*Bull. Soc. Int.*, vol. 13, No. 129, p. 217 (I.).

M. J. LAFFARGUE—Description of the Generating Station of the "Secteur de la "Rive Gauche."—*Bull. Soc. Int.*, vol. 13, No. 129, p. 223 (I.).

A. Blondel and X. Gosselin—Remarks on the Calculation of Electrical Conductors.—*Ibid.*, No. 130, pp. 244 and 249 (I.).

T C. Martin and A. Hillairet—Utilisation of the Niagara Falls.—*Ibid.*, No. 131, p. 302 (I.).

DYNAMO AND MOTOR DESIGN.

G. Roessler—The Effect of different Tension Curves on Asynchronous Alternating-Current Motors.—*E. T. Z.*, part 49, December, 1896, p. 746 (S. I.).

Koloman v. Kaudó—On the Armature Reaction of Unipolar Alternating-Current Machines.—*Ibid.*, p. 753 (I.).

H Eisler and Dr. Max Reithoffer—The Effect of Unsymmetrical Self-Induction on Alternating-Current Work.—*E. T. Z.*, part 50, December, 1896, p. 762 (S. I.).

Professor E. Arnold—On the Calculation and Criticism of Dynamo Machines for One or more Phases, and for Continuous Currents.—*Ibid.*, part 51, p. 774 (S. I.).

Wilhelm Kübler—On the Predetermination of the Light-Load Current of Drehstrom Motors.—*Ibid.*, part 52, p. 786 (I.).

F. Loppé—Determination of the Electro-motive Force developed in a Part of a Gramme Ring rotating in a Uniform Magnetic Field.—*Bull. Soc. Int.*, vol. 13, No. 130, p. 271 (I.).

TRACTION.

A. Bochet—Precautions to be taken against Electrolysis in the Installation of Tramways.—*Bull. Soc. Int.*, vol. 13, No. 129, June, 1896, p. 215 (I.).

G. Pellissier—The Westinghouse Electro-magnetie Tramway.—*Ibid.*, No. 130, p. 252 (I.).

C. Tainturier—The Electric Tramway from La Place de la République to Romainville.—*Ibid.*, p. 276 (I.).

A. Hillairet—On Mechanical Traction in Paris.—*Ibid.*, No. 133, p. 423.

MAGNETISM.

H. Du Bois—On the Absence of Disturbances in Magneto-metric Operations.—*E. T. Z.*, part 49, December, 1896, p. 800 (I.).

F. Braun—On an Experiment in Connection with a Magnetic Field.—*Wied. Ann.*, No. 12, vol. 59, p. 693.

A. B. Bucherer—On the Effect of Magnetism on the Electro-motive Force.—*Wied. Ann.*, No. 12, vol. 59, p. 735.

W. Leick—Additional Notes to my Work, "On the Magnetic Property of "Galvanic Deposits."—*Ibid.*, p. 750.

C. Duperray—On the Electric Properties of a Glass Cylinder rapidly rotating in a Magnetic Field.—*Jour. de Phys.*, vol. 5, December, 1896, p. 540.

INSTRUMENTS AND MEASUREMENTS.

F. BRAUN—Experiments on an Electrical Surface Conductivity.—*Wied. Ann.*, No. 12, vol 59, p. 673

F. BRAUN—On the Continuous Transmission of an Electric Property in the Layers of Solid and Liquid Bodies.—*Ibid.*, p. 682.

F. BRAUN—On the Conductivity of Electric Air.

G. BRION—On the Resistance of Carbon from the Non-Conducting to the Conducting Condition.—*Ibid.*, p. 715.

O. WIEDEBURG—On the Potential Difference between Metals and Electrolytes.—*Ibid.*, p. 742.

J. WILSING and J. SCHEINER—On Research relating to Electro-dynamic Sun Rays; On the Change produced in the Contact Resistance of Two Conductors by the Electric Rays.—*Ibid.*, p. 782.

MM. OUDIN and BARTHÉLEMY—On a Crookes Tube for Alternating-Current Dynamos.—*Ibid.*, No. 26, p. 1269.

M. H. BAGARD—On the Hall Phenomenon in Liquids —*Ibid.*, No. 26, p. 1270.

ANON.—Siemens and Obach Carbon Contacts.—*Ecl. El.*, vol. 9, No. 49, p. 452 (I.).

ANON.—The Replogle Turbine Governor.—*Ibid.*, p. 452 (I.).

H. ARMAGNAT—Electro-motive Force Standards.—*Ibid.*, No 50, p. 495 (I.).

ANON.—The Wood and Mayes Electric Recorder.—*Ibid.*, p. 501 (I.).

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A. HOLLARD—The Analysis of Commercial Copper by means of Electrolysis.—*C. R.*, vol. 123, No. 23, p. 1005.

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C. A. MEBUIS—On Polarisation Phenomena in Vacuum Tubes.—*Wied. Ann.*, No. 12, vol. 59, p. 685.

M. SWYNGEDAUW—Effect of the Polar Surfaces of an Exciter on the Static and Dynamic Explosive Potentials.—*C. R.*, vol. 123, No. 26, p. 1264.

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H. PELLAT—Electrostatics, not based on Couloumb's Laws: Electric Forces acting on a Dielectric (2nd part).—*Jour. de Phys.*, vol. 5, December, 1896, p. 525 (I.).

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CH. BOUCHARD—New Note on the Application of the Radioscope to the Diagnosis of Diseases of the Thorax.—*C. R.*, vol. 123, No. 26, p. 1234.

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G. PELLISSIER—Illumination by Acetylene; Liquid Acetylene; Compressed Acetylene.—*Ecl. El.*, vol. 9, No. 49, p. 442 (S. I.).

M. PERRIN—The Process of Discharge of Electrified Bodies by the Röntgen Rays.—*Bull. Soc. Int.*, vol. 13, No. 132, p. 399 (I.).

JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

Vol. XXVI. 1897. No. 127.

The Two Hundred and Ninety-sixth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, February 11th, 1897—Sir Henry Mance, C.I.E., President, in the Chair.

The minutes of the Ordinary General Meeting held on January 28th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the Class of Associates to that of Members—

Arthur E. Porte.
Alfred Slatter.

From the Class of Students to that of Associates—

Arthur Anstruther-Thomson.
W. J. Belsey.
John H. Bunting.
Herbert Francis Hunt.
William M. Nelson.
Noël B. Rosher.

Mr. R. W. Weekes and Mr. F. W. Wyles were appointed scrutineers for the ballot for new members.

The SECRETARY announced that donations to the Library had been received from Mr. H. Scholey, and from Mr. G. Adams and Mr. J. Slater Lewis, Associates; and the thanks of the Institution were unanimously accorded to the donors.

The SECRETARY read the following letter from Major Marindin:—

February 10*th*, 1897.

DEAR SIR,—Yours of the 6th inst. I had hoped to be able to attend the discussion to-morrow, but have to go off to Manchester for an inquiry.

I hope that Lieut.-Colonel Addison will be able to attend, and, as he is far better acquainted than I am with electrical details, his views on Mr. Hollins's interesting but very technical paper will be of more value than mine.

So far as the general principle of lock and block working is concerned, it is one which the inspecting officers of the Board of Trade have long advocated; and it would be difficult to exaggerate its value in providing a safeguard against a signalman's mistake, such as has, I understand, led to a very serious collision near Stockport two days ago.

We have, moreover, always considered that a very essential part of the invention is that, by means of a treadle, or in some other way, the actual passing of the train should be made one of the factors of safety; and although no doubt the mere electrical control of a starting signal at one signal cabin by the signalman in advance has its advantages, and still more if the block instruments are locked, yet we would very much prefer to see the system introduced in its entirety, and extended to cross-over roads and other connections.

Believe me,

Yours truly,

F. A. MARINDIN.

Mr. Langdon.

Mr. LANGDON: I very heartily congratulate Mr. Hollins on the excellent paper he has brought under our notice. Associated, as we have been, in years gone by, it is particularly pleasant to me to meet him here to-night in the *rôle* of author, especially on a subject which at the present time commands attention. The paper itself, although perhaps not of very great interest to the members of the Institution generally, is one which is of the greatest possible interest to railway companies, and, indirectly, to the railway travelling public; and I presume we may say that at the present time nearly everybody is more or less a railway traveller. The letter from Colonel Marindin which the Secretary has just read emphasises the importance of the question which we are to-night here to consider.

Mr. Langdon.

Mr. Hollins will, I am sure, pardon me if I remind him that this is not the first occasion on which papers dealing with the question of block signalling have been brought under the notice of members of this Institution. In the session 1872–73 three important papers were read in succession by Mr. W. H. Preece, Major Mallock (of the East India Service), and Mr. Alexander Siemens. Mr. Hollins has stated that in 1869 Mr. Tyer took out a patent for interlocking the electrical with the mechanical signals. I am not aware that Mr. Tyer ever put into practice, whatever may have been his intentions, or that he has in any way associated his name with, the practical interlocking of these two classes of signals. I think I am entitled to claim for Mr. Preece and myself the first practical application of interlocking. In 1870 Mr. Preece obtained a patent, with which my name was associated, for interlocking the "home" signal governing the section into which a train might be about to pass, so that the signalman could not take off the home signal—at that time there were no "starting" signals—unless the block instrument showed that the line was clear. The first signal so fitted was one at Southampton, and this was immediately followed by its application to signals for the up and down roads at Wimbledon. Later the interlocking was applied to the control of the switch which governed the electric block signal at the distant end. That was on the Kew branch of the London and South Western Railway. In 1870, as you are all aware, the transfer of telegraphs to the State took place, and both Mr. Preece and myself were then called away in attendance upon other interests. In 1878, however, I again became associated with railways, and shortly afterwards, in contemplation of a desire on the part of the Board of Trade to urge railway companies to adopt interlocking, I applied my attention to the subject, and was successful in producing the system which has since been in use on the Midland Railway for interlocking the single-needle block with the mechanical signals. It occurred to me when considering the question that it was of material importance that the design should be of such a character as to admit of its application to the existing form of block instrument in such a manner that it should

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not necessitate a change in the form of instrument or in the mode of working it; and that, I think, is a point which will commend itself to your consideration. It is very undesirable, in my opinion, that, whatever may be the form of block instrument in use, it should be replaced by something perfectly new. We cannot, in my opinion, employ apparatus too simple in character or mode of working. All that is necessary is that on a train passing into a section it should automatically place the starting signal, or the signal which controls the section, at danger, and that it should not be possible for the signalman to remove that danger signal until the train has passed out of the section. Members will notice that I have taken advantage of the permission accorded by the President on the occasion of the last meeting, to place upon the screen drawings illustrative of the manner in which the interlocking arrangements have been carried out in connection with the single-needle block, and I have further placed upon the table two of the instruments in use.

Fig. 29 is a front elevation of the locking instrument. The needle is operated by the movement of the handle, A, and moves in consonance therewith. When vertical it indicates "Line "blocked;" intended towards the right, "Line clear;" and towards the left, "Train on line."

To maintain the indication in either of the two latter positions, the handle has to be fixed in a corresponding position; thus, if the "Line clear" signal has to be rendered, it is necessary to fix the handle in position B. The mere movement of the needle in either direction does not constitute a "Line clear" or "Train on "line" signal; the indication must, for the time being, be permanent.

To fix the handle in either position, one of the pedals, *d*, *d′*, has to be pressed down. When the handle is held over in the "Line clear" direction, and the pedal *d′* is pressed down, the handle not only becomes locked, but additional battery power is brought into use to such an extent as to lift the lock on the starting signal at the entrance to the section, so as to admit of the signalman there pulling off the signal to allow the train to enter the section. The battery power required for operating the

block signal itself is very small—from two to four No. 2 Leclanché being ample—but this power is useless for lifting the lock on the Mr. Langdon.

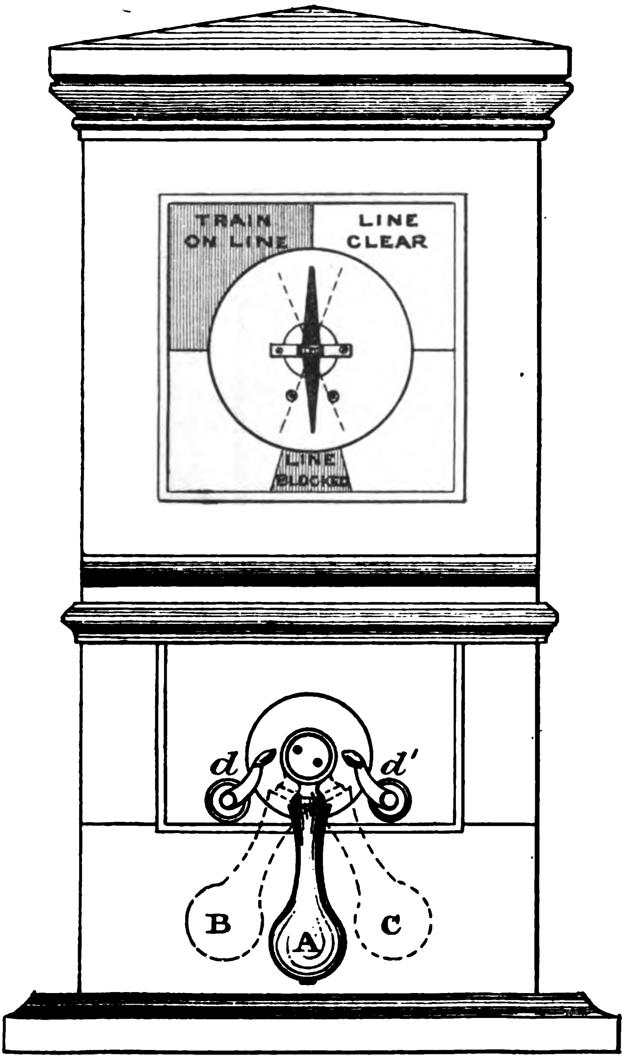

Fig. 29.

starting signal; hence the introduction of the additional battery power. The locking instrument in connection with the "starting" signal consists of a sliding bar attached to the signal lever, which,

Mr. Langdon.

when in the "danger" position, admits of a small locking piece entering a slot, and thus, when present, preventing the movement of the lever. This sliding bar is, further, provided with a contact

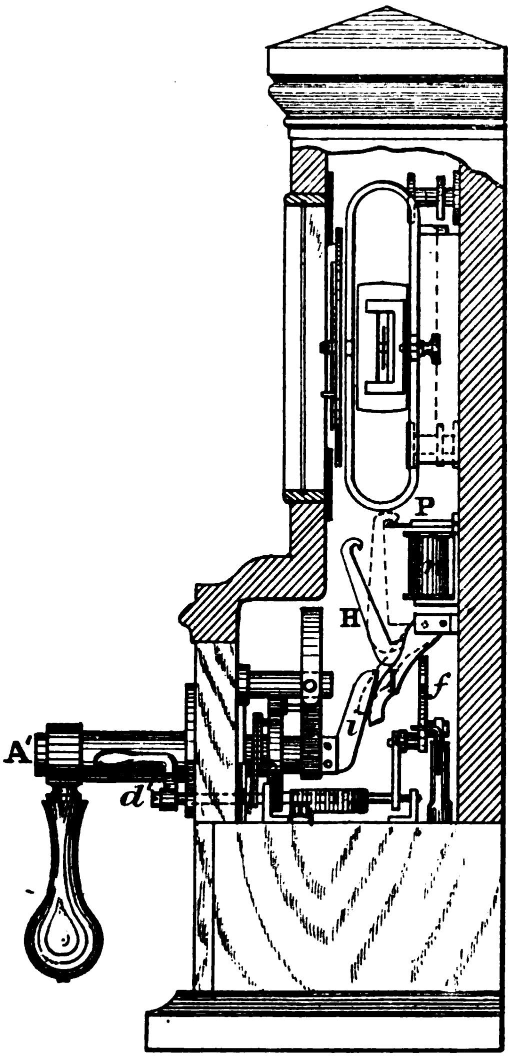

Fig. 30.

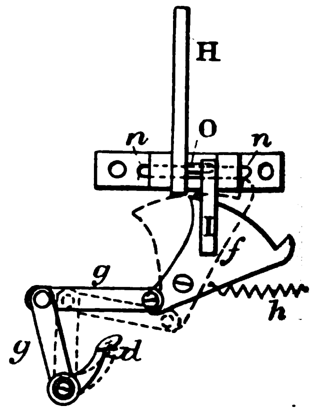

Fig. 31.

arrangement which renders it necessary on the part of the signalman to restore his lever to the "danger" position after taking it off before he can give the "Line clear" signal for a train in the rear.

On the departure of a train being announced at that end of the section to which the train is advancing, the signalman releases the "Line clear" signal, replacing it by the "Train on line" signal, which is held in position by pressing down pedal *d*. The pressure of *d* locks the signal to the "Train on line" indication until it is released by the train passing out of the section. The action of *d* will be better understood by reference to Figs. 30 and 31. On *d* being pressed, the crank, *g*, moves the circular inclined piece into the dotted position; *f* now lifts the locking-piece I, which, in its turn, by means of a pin moving in a limited slot *o*, cut near the pivot of H, lifts it into the dotted position, causing it to engage with a catch-piece on the armature P of the small electro-magnet *m*. On the shaft of the handle A is fixed a rigid locking piece *i*. On H being raised so as to engage with P, the two locking pieces I and *i* engage, and the handle is locked. Mr. Langdon.

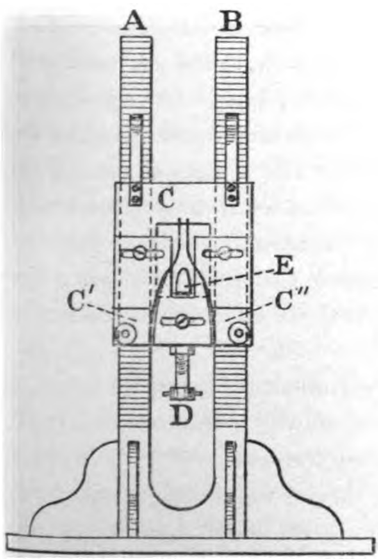

FIG. 32.

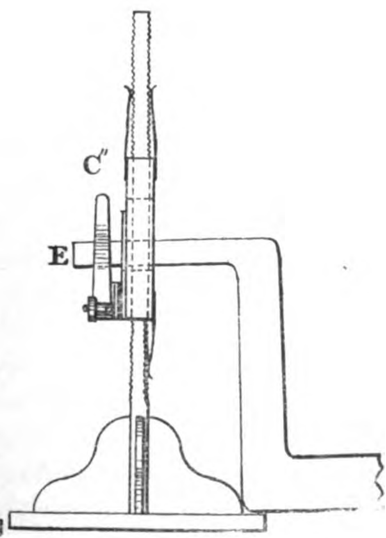

FIG. 33.

At a point which determines the end of the section is placed the clearing treadle or contact-maker—either the Siemens hydrostatic contact-maker, or that originally invented and for some years employed by me. The former is shown in the paper;

Mr. Langdon.

the action of the latter will perhaps be better understood by reference to Figs. 32 and 33. E is a lever actuated by the depression of the rail. A and B are two vertical legs the surfaces of which are roughened. C is a metal frame, with slots so arranged that it may, under pressure, be moved up or down the legs A, B. The end of the lever E passes through an aperture provided in the centre of the metal frame C. In its normal state the end of E rests upon an adjusting stud, D, carrying the frame C down to whatever position is requisite to accommodate it to the position of the rail. On a train passing over the rail, the opposite end of the lever is depressed and the end E is raised, forming contact with the two insulated springs, C′, C″, thereby completing the circuit with the small electro-magnet *m*. The armature P is then drawn down, and the catch-piece H falls away, but, moving in a slot within I, it falls without carrying the latter with it. I remains engaged with *i* by friction—the springs operating the handle A maintaining *i* firmly pressed against I. The train has now passed over the treadle, and the lock on the block instrument has, so far as the action of the treadle, been discharged. The "Train on line" signal is, however, still maintained, waiting its removal by the act of the signalman. The signalman, on slightly urging the handle in the opposite direction, removes the tension on I, which, in its turn, falls to its normal position, when the signalman is at liberty to announce the section clear. The instruments are now in their normal condition, ready to receive another train.

It will be observed how very similar this treadle is to that described by Mr. Hollins, the use of which has been attended with so much success, and it may be asked why it was not equally successful with me. The success which has attended that in use by the author of the paper is no doubt due to the lever being attached to the rail by means of a portion of it being passed through the web of the rail, so that its movement was in perfect accord therewith. I was not so fortunate; the then engineer of the Midland Railway objecting to the web of the rail being bored, my lever had to be keyed on to the rail.

It is very interesting to notice the extraordinary action of the

hydrostatic contact-maker of Messrs. Siemens. There is one on the floor, and the two claws which connected it with the rail will, I think, be found to be 20 inches apart. Now it seems impossible that you can obtain a deflection of the rail in so short a space, and it would therefore appear that the result which is obtained —and it is a very accurate result too—must be the result of a wave action passing along the rail as the weight of the vehicle comes upon it. It cannot be that within this short space an 85-lb. rail can be deflected sufficiently to produce the necessary action on the mercury. But railway engineers are now preparing to use 100-lb. rails, and, whatever the action may now be with the 85-lb., the deflection will certainly be less with the 100-lb. rail. Mr. Langdon.

Mr. Hollins has referred to some five or six systems of interlocking, but I think we may take it that there are but three principal, or established, systems. There is that of Mr. Sykes, that of Mr. Spagnoletti, and my own. Mr. Sykes has been a very active agent in the introduction of his system, and a very excellent system it is. There was a firm also who gave considerable attention to the question, about 1872 or 1873, I think, viz., Messrs. Saxby & Farmer. I do not know how it is that they have been dropped out, but they certainly did do something, and I have regarded it as a very good system. In considering the principles of interlocking, as I have remarked, simplicity is a very important factor. It is very desirable that the interlocking should not be such as to cause the slightest inconvenience in the manipulation of the signals. All that is desired is that the train passing into the section should place the signal at danger, and that it should not be possible to remove that danger signal until the train has passed out of the section; and that the signalman should always have the power to place his signals, either electric or mechanical, at danger.

Mr. Hollins has produced, at the end of his paper, a list of the sections of lines of the companies who have made use of interlocking, and it is very interesting to see to what extent we have thus gone. I have roughly summed up this mileage, and it appears that there is at the present time something like 800 miles of line protected by it. That certainly is something. But

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when we come to consider that the railway mileage, according to the Board of Trade returns for 1895—those of 1896 are not yet issued—is, taken as single line, 31,278*—we realise that it is but a very small portion.

There can be no question that the accident which has recently occurred, and which Colonel Marindin has referred to in the letter which the Secretary read, will direct serious attention to the question we are discussing here. I have stated on another occasion in this room that the systems which are in operation at the present time are not all that can be desired. We are now obtaining a clearance signal by the first vehicle which passes out of the section. It is not very probable that passenger trains will become divided, but goods trains do occasionally break asunder; and there is practically as much danger attending a portion of a goods train left in a section as there would be in a portion of a passenger train. There may be some difficulty in effecting a satisfactory means for signalling only the last vehicle of a train, but I am clearly of opinion that efforts should be made to do so. If it is understood that it is desirable, I have no doubt that inventors will apply their attention to it; and eventually something successful will be obtained.

Before sitting down, I cannot refrain from observing that, although an accident has recently occurred, the manner in which the block signalling in this country is worked reflects upon the railway companies of the kingdom, upon their officers, and all who are concerned with the management and control of signals, and especially upon the men who have the handling of those signals, having regard to the fact that they have before them two distinct systems to deal with—the electric block signals entirely dis-associated from the mechanical signals—the greatest credit for the manner in which they carry out their duties. When we call to

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	Double Line.	Single Line.
* England and Wales	9,325	4,472
Scotland	1,314	1,768
Ireland	613	2,534
	11,252	8,774

mind the extent of mileage and the enormous traffic which passes over our lines, and the few accidents which occur, I am sure this expression will commend itself to everybody here. Mr. Langdon.

The PRESIDENT: We are fortunate in having with us to-night Colonel Addison, of the Board of Trade, and I will now ask him to kindly favour us with a few remarks.

Lieut.-Col. G. W. ADDISON, R.E.: I should like, in the first place, to call attention to that portion of the second paragraph of Mr. Hollins's paper in which he refers to the "constant extension of the "application of electricity for expediting and more safely controlling "the traffic of our railways." I believe it to be a fact that wherever electricity has been intelligently applied to railway working the result has invariably been—although proposed, in the first instance, with a view mainly, if not entirely, to safety requirements—not only to give increased safety, but also to give the means of safely expediting traffic. I might perhaps say, though it may be hardly necessary, that the systems mentioned in the paper—or most of them, at any rate—have passed what may be called their experimental stage; they cannot be regarded as mere fads of elecuricians, or yet of timid inspecting officers of the Board of Trade. I think there is really very little difference of opinion amongst railway men as to the essential features of a perfect lock and block system. As Colonel Marindin has mentioned the chief requirements in his letter, and Mr. Langdon has also referred to them, I shall not delay you by going over that portion of the subject again. I would just point out this: Some people seem to think, when we ask for *automatic* means to enable a train to place the signal at danger behind it as it goes forward into the section, that we are really asking for automatic signalling. That, of course, is not the case at all. The signalman has still, in the lock and block system, to operate his own signals; he is just as responsible as ever for keeping a proper look-out, and he should not take off his signals until he sees that everything is clear and right for the train to proceed. The automatic portion of the system simply consists in causing the train to watch over the lock of the block instruments, and to put back to danger signals which the signalman ought to put back, but which he may Lieut.-Col. Addison.

eut.-Col. Addison.

forget to operate. Perhaps I may usefully employ the time at my disposal to-night if I deal with the chief objections to these systems which are raised in various quarters. Some of the objections really apply to any system of *block* working. It is said that by providing these safeguards you make the signalman into a mere machine, and destroy his intelligence. I do not think it is necessary, in the present day, to argue at all on that basis. But there are not a few who say that unless they get the lock and block in its most complete form—that is, providing not only, as Mr. Hollins points out in his paper, for through running, but also for all the complications caused by shunting operations—that unless they get it complete (with all its attendant expense), the system is not worth having at all. Rome was not built in a day; and if we are to wait for absolute perfection, I am afraid we shall make little or no progress. I think we must be content to take a part in the first instance, if we cannot get the whole. Then there are three objections which cannot be lightly dismissed. The first, Colonel Marindin has touched upon, viz., that different companies use different instruments, and have block regulations which, although they are identical in principle, are not so in some of their details. To adopt the instruments of Mr. Sykes's system, or of Mr. Spagnoletti's, means to a great many companies that they must change the system to which the whole of their staff have been accustomed and trained for many years. That certainly is not a light matter. Then, again, a great many companies, as you will see from Mr. Hollins's paper, use the single-wire block to a greater or less extent; and that would entail the erection of very considerable additional lengths of wire before Sykes's or Spagnoletti's lock and block could be used. However, I, for one, cannot see that there can be any real difficulty in adapting lock and block arrangements to any ordinary system of block working, and I think that is the view that Mr. Langdon holds, although he did not go quite so far in his remarks just now. But he has certainly made an attempt to show that it can be done, because he has fitted lock and block arrangements to the instruments used on his own line. I am sorry to see, from the figures given in Mr. Hollins's paper, that Mr. Langdon has not yet had an opportunity

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of trying lock and block on at all a large scale, but I hope he soon will have. I have also heard—a little bird has whispered it to me—that, if railway companies attach any great importance to having a single-wire lock and block, Mr. Sykes is almost, if not quite, ready to oblige them in that respect. Then we come to the question of the contact-maker, or treadle. That is a vital part of any lock and block system, unquestionably, and many people are very much afraid of treadles. They seem to think that treadles are unreliable, and difficult and troublesome to maintain. I should be sorry for it to go forth that I think any treadle I have seen up to the present time is free from defects, but we have some facts to go upon, and I should not be surprised to hear that those who have used Sykes's treadle most would be the least inclined to condemn it. Then Mr. Hollins has given us figures, on page 13 of his paper, as regards the treadle in use on the Great Eastern Railway, which are rather remarkable, and he is certainly to be congratulated upon the results he has obtained with that treadle. Perhaps he will not mind my asking him to tell us, when he replies, whether the figures are the actual results obtained under normal conditions of working, or whether he had a special staff looking after the treadles, nursing them, so to speak, during the period included in these returns. If there are other gentlemen here who can give us some similar figures with regard to Sykes's treadles, Siemens's treadles, or any other patterns, I am sure the information would be most useful and instructive. Before leaving the subject of treadles, I may say that a treadle was brought to my notice not very long ago, which I think is in use on the Lanarkshire and Dumbarton lines of the Caledonian Railway; it may be described as a Sykes treadle, but with rubbing contacts instead of mercury contacts; and it seemed to promise very well. The last of the serious objections to the lock and block systems, in their present shape, refers to the arrangements for cancelling trains and resetting the apparatus. Mr. Hollins has given us very fully and clearly the reasons why it is necessary to provide some means for releasing block instruments when after being locked they are not set free in the ordinary way. I gather

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from what he has written that he does not consider the present arrangements satisfactory; and I entirely agree that to place a key, which will release the electric lock, in the hands of one signalman, is almost to do away with all the benefits, and all the safety, we expect to get from the lock and block system. Mr. Hollins suggests a way of controlling the use of that key to some extent, and he proposes to do it with a very small amount of additional apparatus, and therefore without much additional complication or expense. I do not know whether he has any working installation in actual use embodying his proposals, but they certainly seem to deserve a thorough trial. With reference to what Mr. Langdon said just now as to the limited extent to which the lock and block system has been adopted by railway companies, it seems to me that the fact is really the strongest testimony signalmen could desire to have to the extremely careful and intelligent way in which they, as a rule, carry out their very responsible duties. Before sitting down I should like to thank Mr. Hollins very much for the trouble he has taken in collecting this valuable information. To many it was inaccessible before, and I can only hope that his paper will go some way towards converting those who are still unbelievers in the efficacy of a lock and block system.

Mr. Spagnoletti.

Mr. C. E. Spagnoletti: In a discussion of this kind it is always a very agreeable position for a man to occupy when he can stand up and be complimentary, congratulatory, and agree with everything that has been said; but I am sorry this evening I shall not be able quite to occupy that enviable and happy position. To-night I shall have to pose as a nasty, disagreeable man, because I want to find, in some respects, a little fault with my friend Mr. Hollins's paper; but I wish it to be distinctly understood that I acquit Mr. Hollins of any intention or desire to say or do anything which is not highly right and proper; but I think in this paper he has made a great omission. Papers read and discussed at these meetings are of great value to this Institution; they are of threefold value. In the first place, they are for general reference; secondly, for guidance and instruction to younger members; and I hope, in the third place, to enlighten

Mr. Spagnoletti.

posterity. Therefore it is most necessary that such papers should contain very full and accurate accounts of any matters or systems brought before us. Mr. Hollins's paper purports to be a description of a system he is responsible for recommending to his company, and which they have adopted. But in this paper I think he has made one serious omission, to which I wish to take exception. He has given a most excellent and full description of the particulars and details of the system which he has employed on his company's line; nay, he has gone further than that, because he has given a further description of it and its attributes upon other lines. Now this has given very great prominence to this system; and, as he has in the fore part of his paper named two or three other systems, and the description he has given of them is very inadequate and meagre—and I may say in the case of my own, which he has done me the honour to mention, is not quite correct either—it puts, I think, those systems which he has not fully described to the utmost disadvantage in the comparison which this paper would lay before any reader. I do not know very much of the other systems, because I have not seen them; and I have not heard that they are much used, if at all, at present, except experimentally. On page 65 of his paper Mr. Hollins states: "No system of electric interlocking can hope for "very wide application that does not specially commend itself, "not only for ordinary through working (which is comparatively "easy), but as equally safe and applicable for siding, junction, "and cross-over working." I wish to say that, although no description of such working appears in the account he was good enough to give of my system, I do all that, and, in fact, I go further; because not only do I arrange for shunting and cross-over trains, but I protect the trains while they are standing on the wrong line. You may shunt a train backwards and forwards from the up line to the down, and *vice versâ*, and the whole of the time the train is being shunted both lines are protected, and as soon as the train leaves one line, that line is open, but while it stands upon the other it is blocked. The shunting may be done as often as one likes, without any difficulty or trouble to the signalman. I had the honour of submitting this system to one

Mr. Spagnoletti. of the inspectors of the Board of Trade, and he was kind enough to say that it was a very good system. He even went so far as to state that he thought that in that respect I had gone further than any other system brought under his notice. With regard to the working of the junctions, that is another point I have given great consideration to. A junction with five or six converging lines can easily be worked with one instrument; and by having one instrument only you cannot possibly have an error, because when you have released the lock for a train to come from one line you cannot work that instrument again until the train arrives to reset it; therefore it makes it absolutely safe. The instruments should be as free from mechanical complications as possible, as each piece of mechanism brings its own liabilities of failure. The worst of having too many mechanical parts, as some of these instruments exhibited have, and automatic working, is that you cannot put intelligence into these systems, and a flash of lightning may set them in action. You have to have signalmen to do the work of their position, and men well trained for that work —men whom Mr. Langdon has given a very high character to to-night for the excellent way in which they carry their duties out, which by a long experience I can fully endorse. I think myself it is a great mistake to go in for too great refinements, and to have much automatic working, when you must have these men to perform their duties at stations; and if you leave them to do their work, and do not relieve them of their responsibility, they become very much better workmen than if you take the work out of their hands and allow them only to look on occasionally and see whether any system is working properly or not. I recollect once speaking to the late Sir Daniel Gooch upon automatic working. He was considered a very good authority in mechanics and railway matters generally, and he made this observation: "You gentlemen, with your automatic working, do "most excellent work. You make capital instruments, and they "work admirably well for a considerable time; and the longer "they work perfectly, the more you lull your signalmen into an "idea of safety, and they then rely too much on their good "working, and they grow less vigilant, and do not do their work

"so carefully as they ought to do." I replied: "Do not take the "work from the signalmen, but check your signalmen;" and that is what I have endeavoured to do. The instrument I use, and which I believe I was the first to introduce, using colours, and assimilating the indications with the lamps by night and signals by day, I see now is very generally adopted by everybody, as is my induced coil in these single-needle instruments. I had a great deal of trouble to get the treadles introduced. In 1873 I brought out a treadle to be acted upon by the deflection of the rail. I had a great deal of trouble with these treadles, and I should think I tried 50 different sorts. At last we got one which worked very well indeed, and we now have little or no trouble with them. I see now that other lines are using treadles also; and I suppose, considering the opposition that was raised against them, I ought to feel a very proud man, as it is said that imitation is the most sincere form of flattery. Therefore I take it that they have found out now that treadles are the crux of the whole thing, and have determined to use them. I was very glad indeed to hear what Colonel Addison had to say about the treadles, and also to hear Major Marindin's letter; because if you have a treadle it checks both signalmen at each end of the section; therefore it requires an error to be made by the man at each end, and by the treadle, before any of the signalmen can work the instrument again to admit another train in the section. The treadle itself is the thing which controls both men. We have in use nearly 300 of these treadles and on the Metropolitan Railway, where there is an exceptionally heavy traffic, the trains run over these treadles incessantly; and working in tunnels, for maintaining the apparatus, is not so convenient as in daylight. Yet the number of defects that we get, in the 260 treadles upon that line, is about 1·3 per cent., and the number of failures in the action of these in the aggregate comes to about one failure in a million operations. I do not, therefore, think that that is very bad working. I quite agree with Mr. Hollins that the more simple the apparatus is the better. What I simply aimed at was that you push in a plunger at one end to take the lock off at

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Mr. Spagnoletti. the station in the rear. That lock being taken off, the instrument which has been plunged at the station in advance is locked. Directly the man moves a lever to lower the signal to let the train leave or pass the rear station, that immediately gives an indication, "Train on line coming," on the telegraph instrument at the station in advance; so the man can see the instant the signal has been lowered to allow the train to leave the station in the rear. Then the man at the station in the rear puts his signal back to danger. If he does not put his signal back to danger, he cannot accept another train from the station in the rear, and therefore he is bound to put it back before he can take the lock off for a train to follow. When the train arrives at a station in advance, the signalman there gets the lock taken off by the station in advance of him, and as the train passes out of his station and over the treadle about a train's length in advance of that station, then his instrument for taking off the lock of the signal in the rear is unlocked by the train; therefore the train only releases his instrument, and puts it in a condition to enable him to work it again for a following train. We do not reset any instruments by putting levers back. It does not matter to us whether the lever is put back or not. If it is not put back, you cannot get a train from the rear; and if it is put back, you are working in proper order. We have no mechanical appliances between the lock at the other end and the plunger at *this* end. When you plunge, an electro-magnet is affected at the other end. You simply, by magnetic attraction, pull the lock off what Mr. Hollins calls the "trigger"—we call it the "catch rod." Mr. Hollins thinks it is better to put it on the lever; but I do not agree with that, because the strain a man can put upon this catch rod with the grip of his hand is not very great, and therefore he cannot do any damage to, or strain the machinery. But if you lift *this* "catch rod" up, and put the whole weight of a man's body upon the lever and pull very hard, you may either damage the machinery, or bend it, or slip the lock out of the slot. I do not think that is as good a system as putting it on the "catch rod," where you cannot use undue force to pull the lock off. Then Mr. Hollins thinks it would be better to put the indicating

instrument which shows the lock "on" or "off" in the line circuit, for many reasons; and I shall be glad if he will kindly, when he replies, give us a few of those reasons, in order that we may know why it is better. I cannot quite agree with him that it is so. To me nothing seems more simple than, when you plunge and take the lock off, the lock itself closes a circuit, and that gives you the indication that the lock is off; and therefore you cannot have a wrong signal, and the man cannot be misled. But if you put it in the line circuit, as the indicating instrument is very much more delicate to work than the lock itself, in case of any failure of the battery power from any cause or any leakage on the line, you may be able to send sufficient current to actuate the indicator, which is a light working instrument, but not sufficient to take the lock off; and in that case the man might be confused, because he would not have an indication that the lock was off, and when he tried it he would find it was *not* off. Therefore I do not think you can have anything more simple than to send from the lock itself the indication of its position. Now with these instruments you can place them in any position you like. The locks can be either in the signal box or on the line, and the instrument for taking those locks off may be put in the signal cabin, or in the stationmaster's office if necessary, or anywhere else you like. But if you have a mechanical system such as Mr. Hollins has shown, you must put those instruments directly over the levers, because the electrical portion is not in the lock, which is mechanically released, but is in the instrument on the shelf above, and the action is as follows:—A current neutralises the magnet holding the catch; this lets the armature drop; this removes the detent; this lets the rod fall on to one end of a lever, which cocks up the other end of lever and takes lock off. I think there are four or five operations which must be gone through before the lock comes off. But in my system we simply plunge and draw the lock out at once by the force of the electro-magnet. In working single lines we have so arranged this system that when a signalman plunges to take the lock off a signal at the other end of the section, he not only breaks the instrument down at his end, but

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also breaks the instrument down at the other end; and therefore it is quite impossible for a man at the other end to send a train in the contrary direction, as it is for the man to allow another train to follow in the same direction till the train is clear of the section. The system works remarkably well. We have it in all kinds of working—on open lines, in tunnels, underground, overground; and it works with surprisingly few defects—in fact, so much so, you may say practically the failures are next to *nil*. We get somewhere about a total of a dozen failures in 12 months, working some millions of trains and millions of miles, which means very severe working indeed.

This system can be easily applied to any existing "block" instruments; all that is required is an extra contact to send a current to take off the lock. It is arranged to give leave for shunting at intermediate sidings, and this can be done safely and with no delay in waiting for the man to take or return with the key as at present; also for working drawbridges, and other special or awkward places. It is impossible to lay down a hard-and-fast rule for uniformly working a large line—the variations of circumstance and requirements are such that it can't be done—and special regulations have to be laid down for special places. Reverting for a moment to the working of single lines, I think it is a great pity that railway officials are so impressed with the idea that the Board of Trade will insist on a "staff" being used instead of signals being electrically controlled. The staff is a thing that ought to be relegated to the past. It was all very well before the electrical appliances we have now were in use, but it is now a hampering, expensive, and troublesome system, causing great delays to fast traffic, as at every station the train has to stop or slacken sufficiently to exchange the staff on a train leaving one section for another; whereas by the electrical locking no delay from slackening is required. Further, with the staff you give your drivers two authorities to start their trains on—the staff and the signal. Surely the signal, the recognised engineman's only guide, ought to be enough if you can control the signals at either end from both ends of the section, which we now can do. The Board of Trade, I am told, by correspondence with them on this subject,

are willing to try the electrical locking without the staff if any railway company will ask their permission to try it, but they will only give their consent on an application from a railway company desiring to try it; but I find the officials of the railways have it so strongly fixed in their minds that the Board of Trade insist on having a staff on single lines, that they are not disposed to apply for any change in the system, although they are cognisant of the inconvenience the staff working entails. Mr. Spagnoletti.

With regard to the "insulated rails" and "fouling bars" Mr. Hollins describes, I should be glad to learn from anyone who has used them how they work in cases where the line is flooded, as lately in many parts of the country we have had this state of affairs.

We have in several places advance sections: that is, a signalman at a station works two sections—one to the station in the rear, and one to an intermediate signal nearly at his station; so that when a train is in a station discharging its passengers, a second train may come up to the commencement of the second section. This saves time, and an intermediate signal box with men and consequent expenses. This system works very well.

In the adoption of light railways they could be very safely and economically worked with simply a one-wire electric locking system, and a telephone put on the same wire for communicating between the men. The new phonopore telephone, which is now so largely used on railways for working on telegraph or block wires, would excellently meet all requirements.

Mr. W. H. WINTER: I will not detain you long with what I have to say in connection with this matter. Acting on the suggestion contained in Mr. Hollins's paper, and also upon that thrown out by the President on the last occasion, I thought it desirable to introduce to your notice the block instrument patented by my brother, and which is very largely used on both single and double lines in India, and also in New Zealand, the Argentine Republic, Victoria, and South Australia. It will not be necessary to go into the electrical details of this arrangement; suffice it to say that the chief principle is that station "A," after asking if line is clear from station "B," can do nothing to alter Mr. Winter.

Mr. Winter. the signals until permission has been obtained from station "B." If you will allow me, I will just go through the process of signalling from "A" to "B," and *vice versâ*. Both "Up" and "Down" dials at *each* station show "Cleared line" as the normal condition. I will first give the usual signal asking, "Is section clear?" "B" replies in the affirmative, if he can do so, putting his switch lever to "On," and "A" acknowledges, the dial showing "On line" at each station for trains passing from "A" to "B." I now have permission to lower my signal, which I could not do until I had that permission. The train now enters the section, and in doing so it passes over the treadle placed at the commencement of the section and raises the signal to danger. The train is now passing to "B." On its arrival at "B," "B" signals to "A" notice of its arrival, first putting his switch lever to "Off." This "A" acknowledges, and the dial again shows "Cleared line." The whole thing is now complete. It is impossible for me to again lower my signal, because I have no permission from "B" to allow a train to enter the section, nor can any train lower the signal.

Mr. D. Sinclair: May I interrupt to ask what may happen if "B" neglects to send a return signal when the train arrives at "B"?

Mr. Winter: Nothing will happen, because no other train can enter the section from "A." I will now repeat the operation in the opposite direction. I first ask, "Is line clear?" On receipt of the signal, "Line is clear," I can, when I have acknowledged it, lower my semaphore, because I have permission to do so; I could not do so before. The train now enters the section and puts the signal to danger, and so on, as before. One advantage of this system is that any person coming into a signal cabin at any time can always see the actual condition of the section. The instruments show whether there is a train on line or not, and, if there is a train on line, in which direction it is going; and they also show, of course, if the line is clear. This system was designed for a single line of rail, but without any increase of apparatus, by simply taking off a cross connection, it may be made applicable to a double line. My brother has since taken out a patent for using the line clear

tickets, which system is also similarly electrically controlled. The signalman cannot open his drawer to take out a ticket without permission from the distant station, nor, after he has taken out a ticket, can he put the drawer back in preparation for taking out another ticket until he has the arrival signal from the distant station; and then, again, he cannot take out a fresh ticket until he asks, "Is line clear?" and gets the necessary signal. Mr. Winter.

Mr. T. IRELAND: Having been accorded the privilege of joining in this discussion, I should like to make a few remarks upon the apparatus on the Great Northern Railway mentioned by Mr. Hollins. I must first say that the thanks of everyone interested in electrical appliances for railway signals are due to Mr. Hollins for his interesting and comprehensive paper. Taking the apparatus on the Great Northern Railway in the order in which it is mentioned in the paper, we first have the Blakey-O'Donnell electrical locking. Mr. Hollins mentions that it is on trial; it may, however, be said to be permanently in use, and has been seen, and, I believe, approved of, by an officer of the Board of Trade. In designing this apparatus the object kept in view was the adaptation of electrical locking to the single-needle block without any cumbersome mechanism, and without necessitating the slightest alteration to the printed regulations of the company respecting block working. To Mr. Hollins it seems mostly Sykes's apparatus, Sykes's treadle and signal-reversers, are used; and, I think, wisely, because it would not be merely difficult, but almost impossible, to obtain or devise more perfect apparatus for the purpose. It will be admitted that the arrangement goes at least as far as any other application of electrical locking to the single-needle block—for straight running, at all events—and that it is distinctly novel inasmuch as "Train "entering section" is automatically signalled by the train itself; so that the possible omission of that signal by the signalman is guarded against. I think it will be conceded that the apparatus will effectually prevent the signalman admitting two trains into one section, and that the arrangement is extremely simple. The signal-reverser is described by Mr. Hollins as normally discharged, but as a matter of fact it is normally in a condition to become Mr. Ireland.

Mr. Ireland. discharged should the signalman move the lever without "Line "clear" being signalled. I may say that it works very satisfactorily; the maintenance gives little trouble, and I think it might be extended with advantage. With regard to the electrical interlocking in the King's Cross tunnel, which Mr. Hollins describes as interesting, I am happy to say that it is also useful, and gives such safety and facility in working, under somewhat difficult conditions, as could hardly have been obtained in any other way. The arrangement at St. Paul's referred to by Mr. Hollins, on the London, Chatham, and Dover Railway, is, I believe, minus very much more than the signal-reverser. I believe the installation at King's Cross is unique; and when it is understood that, in addition to preventing a train entering a tunnel until the previous train has passed out, it provides for shunting, and gives an efficient electrical locking apparatus, which enables us to dispense with two mechanical slots, two mechanical wires through the tunnel, and two frame levers in the Belle Isle box, I think it will be considered very useful indeed. I may say that this arrangement does not require any additional wire; we used two wires for the electrical repeaters only, previously, whereas we now make one wire do for releasing the slots and for the repeaters also. It may be interesting to hear of the means we adopt to insulate the rails. We dispense with the ordinary fish-plates at the joints at each end of the insulated section, and substitute a piece of vulcanised fibre of about the same size and shape as the ordinary fish-plates. The ends of the rails rest on separate joint chairs, fixed on the same sleeper, which is made larger for the purpose. We find it is sufficient to deal with one rail only in this way. I have here a rough model showing how we do it. It is very rough, having been put together hurriedly this afternoon only. I may say that the length of the section insulated is about 500 yards. The insulation resistance of the track circuit is 0·8 to 2 ohms, according to the atmospheric conditions; the resistance of the track circuit relay coil is 1·05 ohms; the conductor resistance of track is 0·14 to 0·4 ohm (very variable); the voltage of the battery open circuit, 0·8 (we use five Daniell's gravity cells in parallel). The voltage of the battery when connected to the track is 0·4 ohm, and when the train is on the

track 0·1 ohm. The current normally passing through the track is 0·3 ampere; when the train is in section it is 0·6; and the normal current passing through the relay is 0·1. These figures may be interesting. I shall be glad to furnish them to anyone who would like to have them. Mr. Ireland.

Mr. J. N. SHOOLBRED: Although an old railway man, and taking a very great interest in the subject, I must now be looked upon as an amateur in this matter. It is a subject which for many years I have followed closely. This paper, to my mind, is by far the most important electrical communication that has been made in the series which is gradually leading up to that final stage, the completion of the "lock and block" arrangements that Mr. Hollins has referred to. It will be some time yet, probably, before the completion is reached. Many, however, of the mechanical details mentioned in the paper, especially in the last portion of it, remind one of the details of some of the earlier systems of mechanical interlocking of points and signals, and some of the marvellously interesting devices for selecting, and for giving priority to the various combinations; such as occur in the arrangements of Saxby & Farmer, of Skinner, of Anderson, and of others. Of these, the "Anderson"—the originator of which is now known as Sir William Anderson, the Director-General of Ordnance—presents singularly interesting mechanical details, especially in the matter of selection. I cannot but think, that a careful consideration, of some of the arrangements in the several older systems just referred to, would benefit the authors of some of the arrangements described in the paper; and help them towards the solution of the lock and block system generally. We shall hardly, I think, get to that solution until the use of large electric currents for the various railway operations in and around large stations, including goods yards, lifts, and electrically transmitted power, shall enable the small primary battery currents to be dispensed with. Then it will be possible to use more readily the large masses of iron and other materials which seem requisite to be embraced in this complete arrangement of "lock and block." Personally, I cannot but thank the author of this paper for the immense amount of trouble, which he must have Mr. Shoolbred.

Mr. Shoolbred. taken, to explain so lucidly a very large number of the most elaborate and delicate apparatus touched upon in the paper; and especially as to the details of some of the mechanical arrangements thereof.

General Webber. General WEBBER: I do not wish to take up the time of the meeting for more than a few minutes, but I should like to include myself among the list of amateurs with Mr. Shoolbred, although I believe our amateuring was really practical work. In my own case, 23 years ago I had charge, under the Post Office, of the working of the electrical signalling on two railways—not very celebrated railways: one I daresay you have never heard of, namely, the Colne Valley Railway, worked on the staff system; and the other, better known, the London and Tilbury Railway, worked on the double-needle signal system. I may add that the Great Eastern was also worked at that time, under the late Mr. Sach, with the double-needle instruments. Two line conductors were required, which occupied more space than the one which is now solely in use for the same purpose on that line, as described by the author of the paper. I daresay Mr. Hollins was probably instrumental in replacing the old system by the new, although he does not mention it in his paper. I daresay he will tell us that his paper was quite long enough—and we think interesting enough—without giving us details of the history which has led up to the adoption of the perfect system which we now have in this country. Just consider that at this moment there are, if not thousands, at any rate hundreds, of trains rushing over the country as we sit here, all of them controlled and governed by signals of one kind or another such as we have heard described. I often think of it when I am in an express train; but I am glad to say I do not often recall the fact that my whole safety is dependent on two men and a treadle, or two treadles and a man—I forget which. Whichever it is, it is rather an awful thing to consider that the whole of one's safety is dependent upon mechanical and such uncertain human conditions, where men in charge of signals may not be quite wideawake, and may have been overworked. With regard to the treadles, we have heard from Mr. Spagnoletti that he has tried 50, and that out of those 50 he could only find

one to work entirely to his satisfaction. Fancy what all the other 49 treadles must be doing, and all the accidents we have been liable to while Mr. Spagnoletti was making those experiments! General Webber.

I daresay you have often heard in the past that the telegraph was called the handmaid of the railway; and when you come to remember what railway working was then, when the telegraph commenced its handmaid work, and what it is now, you will see that it has been a most useful handmaid. It is an interesting fact that they have grown up together, and that the handmaid has always been equal to the work required of her, and that the experience gained in one has added to the experience gained in the other. Now that which came home most to my mind in this discussion to-night was that the history of the needle instrument connects it not only with telegraphy—and I am not an old enough telegraph man to say whether the needle instrument was used first for telegraphic messages (though I daresay Mr. Hollins could tell us), or first for railway work—but also with railway signalling. I am in doubt whether the idea of the single needle first sprang out of the necessities of railway work or of telegraph work; but it is a very curious thing when you come to remember that the needle instrument is almost the first instrument which was used for message purposes in Europe, and that that instrument, almost in its integrity, is now being used to govern in the way described by Mr. Hollins the working of the block system, and that the same signals, with the same minute voltage milliamperage, is now being used as in the fifties. I daresay, in fact, their battery power was crammed on far more in those days than it is now. The absence from the paper of information on this subject as to what is going on, and has been, in America and the Continent is a little remarkable. I daresay we can pick it out from among the technical papers, and it would be very interesting if compiled. On the Continent for many years inventors turned their attention to all sorts of minute and exceptional uses to which to put electric currents for the purpose of signalling moving trains and governing railway working. Amongst others, I remember—I think Mr. Spagnoletti will recollect it also—that about the year 1867 we had to examine in

General Webber. France an ingenious system of installation of what has been described this evening as track circuiting, by which signalling was attempted to be kept up on some French railway lines by which signalling was possible between stations and a moving train. On the Continent it has almost disappeared. Whether it is much used in America or not, and how far subject to frosts and conditions of exceptional weather, I do not know; and I hope someone connected with railways will give us a paper on that subject, for it is one full of interest. As regards what Mr. Shoolbred said on the subject of using larger currents, I should like to remind him of Sir William Siemens's strong magneto pull signaller, which in its make and break was used on some of the early railway systems, and introduced a reversing current for moving signals and for bell calls in two different directions. It was very useful and practical, and was adopted by some railway companies. There is no doubt that the actual energy exerted by those signals at the point where they acted was very powerful. I do not wish to take up the time of the meeting any longer, because I daresay there are others present who wish to speak; but the point which to my mind is most interesting is the history of that needle instrument and its uses, beginning with the double needle, and then being reduced to a single-needle instrument, as it is now used in so many of the signal boxes on our railways.

Mr. Winter. Mr. W. H. WINTER: May I point out to the meeting that this system of my brother's is essentially a single-wire system? The whole of the operations are conducted on a single wire, whether for a single or a double line of railway; whereas in the case of most systems, as used on the majority of railways in the United Kingdom (see Appendix II. of Mr. Hollins's paper), three wires are employed, with, in some cases, a fourth wire for the treadle or rail-contact circuit.

Briefly, I would summarise the advantages claimed for this system as follows:—

1. One wire required instead of three—important as affecting both first cost and maintenance.
2. Indicators show at a glance the exact condition of the section or sections. The signals cannot be misunder-

stood, and nothing is dependent on memory or entries in signal book. They show— Mr. Winter.

(*a*) Whether a train is on the section or not, and the last signal given, and by which signalman.

(*b*) If a train is on the section, in which direction it is travelling.

(*c*) Which station gave the "Line clear" signal. This is of great importance where a train is waiting at either end.

3. No alteration can be made in the signals displayed by either instrument without the combined action of the signalmen at each end of the section.
4. The signals are unaffected by false currents, whether due to lightning or contact, nor can the semaphore be lowered thereby.
5. No signal can be completed and exhibited on the instrument until it has been not only made, but *duly acknowledged*.
6. The starting semaphore cannot be lowered until "Line "clear" has been received and *acknowledged*. It is controlled electrically by the instrument itself.
7. The train raises the semaphore to danger as it enters the section.
8. The system, although originally designed for a single line of railway, is equally applicable to a double line, by simply removing a wire connecting together two terminals outside the instrument, and no additional apparatus is required and no outlay involved in the change; this because a separate dial is used for trains travelling in opposite directions in the case of a single line, one dial being lettered "Train coming "from," and the other "Train going to," with the name of the station common to both dials.

Mr. Spagnoletti.

Mr. C. E. SPAGNOLETTI: If I am not out of order, may I give a few more figures? On the Metropolitan Railway, 46 million passengers were carried in the six months ending December, 1896, in about 300,000 or 400,000 trains. On the City and South London

Mr. Spagnoletti. Railway—the Electric Railway—in 1896 six millions and a half of passengers were carried; and since the opening of the line in 1890, 37 millions have been carried; and the number of trains run on the City and South London line in the last six months was 73,489. They have got 12 treadles on that little line, and had only eight failures in the six months with such a heavy train service. The fixing of the treadles requires special care. When properly fixed we have no trouble with them, but few men seem to acquire the tact of putting them down properly. With my treadle no cutting or drilling of the rail is required.

Mr. McMahon. Mr. P. V. McMahon: Mr. Spagnoletti made some allusion to the working of his system on the City and South London Railway. Having had about six years' experience of that system, perhaps I might be allowed to make a few remarks. At the Stockwell terminus signal box the number of movements made by one man in a single day is over 8,500, and 4,500 of these are electrical. The failures of electrical locking during the last six months amount to eight, and this includes all the instruments on which, as I said, 4,500 movements are made per day. A member made some remark about having the treadle operated by the last coach. Nearly all the treadles, especially the outer home treadles, are worked by the last coach. These treadles are simply strips of brass about 3 or 4 feet long, secured to the side of the tunnel, and insulated therefrom. A brush is attached to the bogie of the last coach, and makes circuit through the rails. These treadles have been in operation about three years, and the failures are very few indeed. I can also bear out Mr. Spagnoletti's statement with regard to his treadles that their failures are few and far between. The principal trouble we have had is with the contact springs breaking. No doubt some of these failures were due to the contact arm being set too close to the rail, but on the whole they work very satisfactorily.

Mr. Weekes. Mr. R. W. Weekes: I think that electrical engineers should not forget that all these signals we have heard of to-night only assist the signalmen to keep the traffic in order; but that we have heard of nothing which prevents the engine-driver from overrunning or disobeying the signals. When the time comes that

the busiest lines will be worked electrically, there can easily be provided devices by which the engine-driver cannot over-run his signal and proceed on to a section of line which is not clear. It is a very easy matter when the trains are electrically driven to divide the conductor up into sections, and to place the switches by which these sections are energised in the signal box. The signalman can then cut the current off the sections not cleared, and the engine-driver will be absolutely under control. It will also be impossible for an engine-driver, mistaking the signals, to start out of a busy terminus and cause an accident, as frequently happens. In this way the electrically driven train will afford increased safety to the public, and will give the engine-driver the same security against error that the present methods afford the signalman. Mr. Weekes.

The PRESIDENT: There is a communication here from Mr. Goodenough, the electrical engineer of the Great Western, which I think you will like to hear read.

Mr. H. T. GOODENOUGH [*communicated*]: I very much regret being unable, through illness, to be present at the discussion on Mr. Hollins's interesting paper. Mr. Goodenough

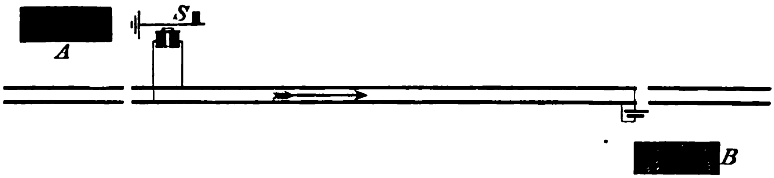
I do not think, as regards electric locking, that we have yet reached finality, nor that this will be so until the various systems include the principle that the starting signal shall remain locked until the section in advance is actually clear of all obstruction. This means, of course, using the rails as a conductor for transmitting the current that releases the lock, and the abandonment of treadles which are actuated by the first vehicle of any train.

I think that a permanent current is much preferable for the releasing of a lock, and, further, that the lock should be taken off by a line current rather than by means of a relay.

Using the rails as a conductor, I should say that it is possible to have a system much simpler than any locking system now in use, combined with a signal-replacer equally as simple. Take, for instance, a section of line, "A"–"B," controlled by the starting signal, "S," and let there be two levers on the signal—the one connected with the signal arm, and the other worked from the signal box "A." The weight on the former, semi-circular in shape, might contain an electro-magnet in circuit through the

Mr. Goodenough

rails with the battery at "B;" and the weight on the latter, also semi-circular in shape, and fitting down closely on the weight on



the other lever, might be the armature. If there was no obstruction on the line, the working of the lever from the signal box "A" would cause the lever connected with the signal arm to be moved also, and the signal lowered to the "All right" position; but as soon as a train had entered the section the electro-magnet would be short-circuited, and the arm would go to danger, and could not again be lowered until the train had passed out of the section. A battery and suitable contact at "B," and an electro-magnet at "A," would be all that was required, with the addition of suitable indicating instruments for the signal boxes.

With regard to treadles, I certainly prefer mercury contacts as being the less liable to fail on the wrong side. I have, however, found a little difficulty with these, through their being over-sensitive in the case of crossing warning bells, where on single lines a break treadle is fixed just in advance of the make treadle; the mercury in the latter remaining in motion rather too long, and making contact after the break treadle has been passed. The treadle arrangement of Mr. Hollins seems to be a very ingenious one, and, I should think, a perfect safeguard against failure on the wrong side with treadles of the lever type. The treadles we use on the Great Western system proper are mostly "Buck's."

Having about 1,100 miles of single line to deal with, I am naturally very much interested in the application of the electric locking to the electric tablet and electric train staff system. The arrangements described by Mr. Hollins, however, seem to be wanting in one important respect, namely, the principle of electric locking that the train shall arrive at the other end of the section before the electric lock can again be released. I take it that the

sole object of applying the electric locking to the electric train staff or tablet system is with the idea of providing a safeguard in the event of a driver leaving a staff station without the staff. Such cases have occurred, but neither of the arrangements mentioned by Mr. Hollins provides any protection in a case of this kind. The conditions of a combination between the two systems which I have had laid down to work are, first, that it shall only be possible to lower the starting signal at that end where the staff is withdrawn; and, secondly, that, a staff having been once taken out, and the starting signal lowered, it shall not be possible to again obtain a staff or lower a starting signal until the train has arrived at the other end, passed over a treadle there, and the staff has been replaced. If the locking and staff or tablet system are to be combined, I do not think that the essential principle of the locking system should be sacrificed; indeed, without it, I do not see much use in locking the starting signal at all. Mr. Goodenough

Mr. W. LEONARD [*communicated*]: I think all will agree that Mr. Hollins has applied Sykes's system very thoroughly on the Great Eastern Railway. It is satisfactory to observe that the five Southern companies have instituted a uniform system. Sykes's system has given entire satisfaction on the South Eastern Railway, and is now being extended throughout the Metropolitan District. The readiness with which it can be adapted to all the intricate conditions of working is a great recommendation. Mr. Leonard

My experience of treadles extends over a period of 10 years. Spagnoletti's spring contact treadles were first tried, and were discarded in 1890 for Saxby & Farmer's mercurial treadles, which have worked well. In 1893 a Siemens treadle (old type) was fixed on the East London Railway; but after a time the red lead which was employed to secure the diaphragm perished, and pieces dropped into the mercury, stopping the channel. This was replaced by an asbestos washer, since which it has practically required no attention. Two of a later form have recently been fitted—*one* on the main line, where the trains run at great speed. I am greatly in favour of this type, as they are readily fixed, and require but little attention; but whether they will be entirely

Mr. Leonard. satisfactory under all conditions has not been finally determined. The Sykes treadle has done good service on Cannon Street Bridge, and other places; but is not so easily fixed as the Siemens.

I am of the opinion that mercurial treadles will prove less costly to maintain in the long run, and will compare favourably with the spring contact type in efficiency, although Mr. Hollins's experience of the latter appears to be highly favourable to its adoption.

The PRESIDENT: As it is already past the usual hour for adjournment, I will ask Mr. Hollins to kindly postpone his reply until the next meeting, when he will have more time to answer all the questions that have been put.

I have much pleasure in announcing that the scrutineers report the following candidates to have been duly elected:—

Associates:

J. H. C. Brooking.
Walter Wm. Crawford.
William Cross.
Arthur Dennis.
John William Hame.
J. R. Hewitson.
J. Fraser Lister.
Ernest Dinsdale Phillips.
Norman Staniland.
Kenneth Watson.
Burkewood Welbourn.

Students:

Archibald Thorpe Crosher.
Otto Max Constantin Heyl.
Elias Marcuson.
William Middleton.
Herbert William Ottaway.
E. A. Parkinson.
Ferdinand Edward Reiss.

Adjourned to February 25th.

The Two Hundred and Ninety-seventh Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, February 25th, 1897—Sir HENRY MANCE, C.I.E., President, in the Chair.

The minutes of the Ordinary General Meeting held on February 11th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members

Henry Clavell Crews.	John Gell.
John Denham.	Philip Monson.
L. Epstein.	John Rance.

Robert Ord Ritchie.

From the class of Students to that of Associates—

Frederic John Bakewell.	David Wright Niven.
Ernest E. Benham.	Frederic Simpkin.
Henry Frederick Hesse.	Charles Davis Taite.

John Walton Ward.

Mr. George R. Webb and Mr. K. Edgcumbe were appointed scrutineers of the ballot for new members.

The PRESIDENT: I will now call upon Mr. Hollins to read his reply to the criticisms on his paper.

Mr. HOLLINS, in reply, said: Mr. President,—I should like, in the first place, to heartily thank you and the various speakers and members generally for the kind expressions of appreciation accorded to the paper I have had the honour to present to you. Mr. Hollins.

Mr. Hollins. I would also like to add with regard to it that if I have failed to do full justice to any arrangement or system it has been my privilege to describe, I trust it may be put down rather to an oversight on my part than to any demerit of the apparatus under consideration.

Since the proof of my paper was issued, I have received a communication from Mr. Dunn, of the Caledonian Railway, pointing out that the description given of that company's electric interlocking is incomplete. It appears that the treadle which puts the controlling signal to danger behind the train as it enters the section does not release the plunger, to enable another train to be accepted from the rear, but that the "Train entering section" signal must be given to the box in advance, and acknowledged, before the lever which is back-locked can be put back to re-engage with the signal, and this lever being put fully back and relocked admits of another train being accepted.

I much regret having misunderstood the verbal description given me by the manufacturer. I need hardly say I had no intention to underrate the safety of the system, or to minimise the ingenuity of its designer.

Mr. Wyles pointed out that the arrangement of Mr. Tyer (Fig. 6) for interlocking the signals with the tablet instrument did not compel the signalman to put his signal to danger. It was never intended to deal with that point at all, but only to show how it was proposed to unlock the signal by the act of obtaining a tablet. There are several ways by which this may be done. One is to automatically disconnect the battery whilst the signal is in the "Off" position; and another is to mechanically lock the commutator of one or both instruments in whatever position it, or they, may stand, so long as the signal is in the "Off" position.

With regard to the question of lightning. Mr. Wyles told us that he had never heard of it releasing Sykes's apparatus. Possibly not. Sykes's apparatus, I think, stands normally released on the London and South Western. It is only locked whilst the train is in section. Does this suggest the reason to Mr. Wyles? I believe the failure is a very rare occurrence; but

it was from Mr. De Malan, of the Hull and Barnsley Railway, that I heard of an occasional fault taking place, although, as he says, the signalman always knows what has happened, and raises the rod. The lightning, therefore, in these cases *must* have been "positive," or it would not have released the instrument. Mr. Wyles may be sure that I have not lost sight of the possible effect of the inductive discharge giving a current of opposite polarity, but I had some evidence, as he will now see, to warrant at least an experimental trial of a reversal of the coils and batteries. Mr. Hollins.

Mr. Wyles suggested that I had put forward the question of the back lock as peculiar to the Great Eastern Railway. I never made any statement to warrant such an inference.

Mr. Wyles also questioned how I got at the figures given as to the working of the rail contacts. This information was obtained from a statement very kindly got out for me by our Superintendent of the Line (Mr. Nettleship), and therefore its accuracy is beyond question. This return shows that we have 78 boxes fitted with Sykes's apparatus; adding up the number of trains passing each box during the 24 hours, we find we have an average (Sunday and week-day) of 31,556 trains signalled by Sykes's apparatus during that time. We have 1,000 trains passing some of the signal boxes every day. On each occasion one at least (and in some cases three or four) of the rail contacts have had to electrically actuate or reset the locking apparatus. Multiply this 31,500 by the number of days, and you have the figures given by me.

Before replying in detail to Mr. Langdon's remarks, I should like to thank him very much for his congratulations and kindly references to myself. From no one are they more acceptable than from my old and much-respected chief.

Mr. Langdon must pardon me pointing out that I did not claim that my paper was the first on block signalling. The papers referred to by Mr. Langdon (now some 24 years old) had but small reference to, and practically gave no details of, electric interlocking.

As regards the similarity of the rail contact of Mr. Langdon and my own, I have very little doubt that, if Mr. Langdon's

Mr. Hollins. arrangement had been a friction clutch, instead of, as it seems to me, in the nature of a ratchet, and the tail-piece of the lever had been not only passing through the web of the rail, but keyed into it (by a steel key-piece), he would have had a similar result. The base, or bedding, on which to fix the rail contact is also a very important factor, and will have a considerable bearing upon the result.

I was asked by Colonel Addison, with regard to my rail contact, if the results were obtained under the normal conditions of working. That is so—under the normal conditions permanently in force. I was afraid that in giving the period covered as six months ending September 30th, and my reading the paper at the end of January, would cause some doubt to arise whether a favourable period had not been selected, or a special result tried for in view of this paper. This was not so. The paper was written and sent in by me in October. Hence I mentioned the results of working during the latest period of six months at the time of writing. Since then we have had almost continuous wet weather, and yet I am pleased to say I have had even better results, for I had only two failures in the four months from September 30th to January 31st. There was not a single failure either in October or January, so that these 400 rail contacts have been actuated in the aggregate for nearly four million trains with only two failures. It should be understood that all these failures are on the safe side, leaving the apparatus locked. I have not had a single instance during the last 12 months of failure on the wrong side.

Whoever is responsible for the maintenance of any complete system of electric interlocking, on very busy lines, where timekeeping of the trains is held to be, next to safety, of the first importance, will soon realise that it is a very evident, and a very heavy responsibility. It must be remembered that I am speaking now of sections over which there are from 500 to 1,000 trains per day passing. I determined from the first, therefore, to so apportion the duties as to fix individual responsibility. The lineman in charge, with his assistant, whilst being really responsible for everything, is more directly responsible for the locking,

the apparatus, and the wires; a batteryman is responsible for the batteries; and each section having from 65 to 100 rail contacts has a junior responsible for these. Each one has imperative orders that he must examine, clean, and, if necessary, oil the parts as often as possible, the interval never to exceed 10 days or a fortnight. Steps are taken to ensure that this is done. Such are the arrangements in force (not more for one portion than another) in order to avoid delays to trains. But a better or more loyal and energetic staff (and I am pleased to tell Colonel Addison that some of the best are from the Royal Engineers) no man could have. A special word of commendation is due to my untiring and energetic inspector, Mr. Sach, the son of my predecessor. Mr. Hollins.

With regard to the remarks of Colonel Addison as to the opinion which seems to be entertained in some quarters, that treadles or rail contacts are unreliable, and difficult to maintain, I should like to say a few words. I do not care what treadle or rail contact may be invented now, or at any future time, I am absolutely certain that if people think they are going to put them down and then leave them unexamined and uncleaned for an indefinite period, in the expectation that they will not fail, they are foredoomed to disappointment; in fact, they deserve to fail. I have tried all kinds of contacts, and I have had two attached to the rails, the same as the Siemens and the Sykes contacts, for over two years, and they have been carefully watched. I am sure I have not sought to make any invidious comparisons, for I am quite certain that the Sykes and the Siemens treadles are most reliable. But whilst attachment to the rail is excellent in principle—and you must keep any contact firmly in approximately the same position in relation to the rail—still, it will not without care and attention avoid failure. The very necessity of the principle brings in another factor necessitating attention. You must have bolts and nuts, or some similar device, to attach the *support* to the rail. Every railway man knows how nuts work loose on the bolts securing the fish-plates, and necessitate constant attention by the platelayers to keep them tight up. You may put on your locking nuts and your cotters. The

Mr. Hollins. shake, and the rattle, and the vibration, with 50- to 80-ton engines thundering over them, will upset those little devices; there is nothing for it but to attend to them and keep the nuts tightened up. If this is not done it is not giving the contact a chance, as it will inevitably fail, however perfect it may otherwise be.

And here let me say I lay no claim to originality as regards the friction clutch. That was patented by David Rosseau, of New York City, over 20 years ago. Whatever little merit may attach to mine is in the method of applying it.

As regards Colonel Addison's remarks with reference to the control of the cancelling key or switch: It has not yet been tried in actual practice on the line, but to show its practicability it was fitted to the model installation which was in the room when the paper was read. I hope, however, to soon get an opportunity of giving it a trial in actual practice.

I now come to the remarks of my friend and colleague Mr. Spagnoletti. I really thought when he commenced that he was going to demolish every point in my paper. He told us he was going to be a nasty, disagreeable man, and really I think to some extent he succeeded—at any rate, in so far that he pressed so many invidious comparisons upon our notice (as between his own system of electric interlocking and that for which I am responsible) that, unless I agree to stultify myself, as it were, by silent acquiescence, I must undertake the disagreeable duty of controverting some of his statements. The very comparisons made invite, I will not say retort, but defence.

In the first place, he complained because in my paper I have had the temerity to give, as he puts it, undue prominence to one system—that is, because I fully described that which I know all about, and left, so far as details are concerned, my friend to do that full justice to his own which he could do so much better than me. I wish he had done that, and left those comparisons out. But first as to this complaint of my giving only a meagre description of his apparatus. Has he forgotten that I asked him to let me have the instruments to take drawings and descriptions, or to favour me himself with a detailed description? I had to

take notes and write the description from memory of the verbal Mr. Hollins. explanation given me. How can he complain now, when he deprecated my giving any full description, and advised me to deal with the subject briefly? Surely he did not expect that in taking his kindly advice about his own system I must necessarily do the same with others.

I noticed Mr. Spagnoletti seemed to make a very broad claim to the rail contact for electric locking, and to take it as a compliment to himself that others now use them. Every railway man acknowledges how much Mr. Spagnoletti has done in block signalling and electric interlocking, but surely he knows that electric rail contacts were devised and used for signalling purposes many years prior to 1873. As I mentioned in my paper, Mr. Edward Tyer had one in use in January, 1852, for train signalling; but in November, 1852 (the same year), a Mr. J. S. Crowley proposed to use a treadle, or rail contact, for electrically putting the signal to danger behind a train, and for the levers to be also actuated by it, or released so that an attendant could actuate them. In fact, there are many instances prior to 1873.

And now, with regard to the traffic on the Metropolitan Railway (not the District), Mr. Spagnoletti said: "They have got "somewhere about six or eight million trains in a half-year running "over that line." I should like to know if Mr. Spagnoletti still maintains those figures, because I have been in search of those eight million trains ever since our last meeting.

And then as to the tunnels. Mr. Spagnoletti speaks of the *dis*advantages of having the treadles, or rail contacts, in tunnels: should not *dis* be crossed out, and the word read as "advantages"? No doubt it is inconvenient to get about after the rail contacts in the dark tunnels, but then there is the advantage in other respects, and that is, the absence of the effects of the heavy rains, and the frost, and the snow, on the ballast, which is such a fruitful cause, on open lines, of the road giving and the rails settling down. I have only four rail contacts in tunnels, and these do not, on an average, fail once in 12 months.

Mr. Spagnoletti, in referring to his lock being on the trigger,

Mr Hollins. or catch rod, of the lever, represented me as saying in regard to this that I considered the lock should preferably be on the lever. This is really too bad. I simply stated where the lock was fitted. I never expressed an opinion about it at all, and I refrain now. With regard to his statement as to what may take place with the lock on the lever, if a man puts his whole strength or weight on it—well, I may be sensible of the weight of Mr. Spagnoletti's arguments, but I challenge him or anyone else, with all their strength and weight, in any fair manner, to either "damage "the machinery, or bend it, or slip it out of the slot."

Mr. Spagnoletti states that with Sykes's system the locking instrument must be placed over the lever. But this is not so. As a matter of fact, sometimes the instrument (say at Central Junction, Stratford) is placed over one lever, it is true, but is perhaps locking several other levers 20 or 30 feet away, as it is not necessary with Sykes's system to have a separate locking instrument with electro-magnet for each lever, as is the case with Mr. Spagnoletti's.

Again, Mr. Spagnoletti represents me as saying that I thought the lock indicator, showing lock "Off" or lock "On," should be in the line. I said nothing of the kind. I said the *block* (not the *lock*) indication, "Line clear" and "Line blocked," should be given by a coil in the line circuit. I hope he will pardon me for the presumption, but personally I think his *lock* indication might be better given mechanically and directly than electrically. His lock is at the back of each lever, and what more proper place for the indication?

And now as to my reason for the block indication being given by a coil in the line. In the first place, what does our combined apparatus purport to be? Why, apparatus for *electrically interlocking the block with the mechanical signals* —not abolishing the one because we are locking the other. I have always strongly urged, and I do so most strenuously now, that the signalmen should not work merely and solely by the lock indications. They should work strictly to the *block* indications, and leave the lock what it is primarily intended to be—a check upon the use of the lever of the corresponding

outdoor signal until the block instrument, by giving the state of Mr. Hollins.
the section, indicates that the section is clear and the lever may be used. The lock indications are merely to show if the lever *can* be used. In any system I think that this principle of working is of the highest importance.

Another reason is (if Mr. Spagnoletti will pardon the suggestion) that, having once given the "Line clear" indication by electrically unlocking the lever, the man in advance is deprived of the means of again blocking the line in case of emergency. Again, the signalman in the rear has really no indication of the state of the section in advance. It is the position of his own *lock* alone. However, I refrain from giving further reasons; and let me here add that I should be sorry if these invited criticisms (I was challenged to give them) should cause anyone who may agree with me to take them as serious objections to Mr. Spagnoletti's system of electric interlocking. It is an important point, in my opinion, but it is simply corrected by placing in the line one of his admirable coils (for which all railway men have to thank Mr. Spagnoletti) with the induced undemagnetisable needle.

With regard to Mr. Goodenough's communication, I think he misunderstands what I said in my paper. I daresay I have not been as clear as I might have been. He states, as regards interlocking of the tablet and train staff instruments on single lines with the starting signals, that the arrangement seems wanting, as there is nothing to ensure that the train shall arrive at the other end before the signal lock can be again released. Everyone of the conditions Mr. Goodenough lays down as essential is complied with in the arrangements I describe—indeed, *without* one part, namely, the treadle. That is not necessary, because the tablet or staff must arrive at the other end before the signals at either end can again be lowered or another tablet or staff obtained. You cannot get a signal without first getting a tablet or staff; and as you can only get one of *them*, you can only get one signal. The signal must be put to danger or you will never get another tablet or staff, and once to danger it is locked.

I think the suggestion of General Webber and Mr Shoolbred

Mr. Hollins. that there might be something to be learnt by looking back and examining some of the arrangements and devices proposed, and in use to some extent, many years ago for signalling trains, well worth consideration. Long before the time of signal boxes, lever frames, and block telegraphs had been seriously thought of, all kinds and descriptions of automatic signalling arrangements were proposed, and very ingenious many of them were.

Mr. Weekes caused some amusement by remarking that we had heard of nothing yet to stop the engine-drivers from over-running the signals. Why, even something more remarkable than this was proposed by Manuel De Castro in 1853. He proposed, by means of electricity, that, should two engines of different trains get within a certain distance on the same line of rails, an alarm should be sounded on both engines, steam shut off on the second engine, and the breaks put on.

In fact, having by me all the abridgments of electrical patents from the earliest periods, I have been extracting from them in my leisure hours a few notes, in chronological order, of those old electrical inventions relating to railways. They are very interesting; they induce a feeling of considerable respect for the work of those who have trod the path before us, and a corresponding humility of spirit in estimating the value of our own.

Some day I may ask my railway friends to accept a copy of these notes.

It has been said that it is very undesirable to alter existing systems of block, necessitating the signalmen being taught an entirely new system of working; and also that it is absurd to suppose that railway companies will scrap all their present block instruments.

This looks a very formidable argument, but in my opinion too much is made of it. It seems to me that to tack on to existing block instruments the necessary additional parts—such as a special electric lock for each lever, switches, relays, &c., for each block instrument—will cost just as much, if not a little more, than to provide the entirely new combined electric lock and block instrument designed for the purpose. If anyone will go deeply into the

question, as I have been compelled to do, they will find it is as I say. Mr. Hollins.

Then as to the altered system of working. We have entirely changed our system of working from Tyer's one-wire to Sykes's three-wire on our suburban lines in some 78 boxes. We have not found the least difficulty in this, and I am not aware of a single instance of mistake in working, notwithstanding our heavy traffic, through changing from one system to the other. Of course each section is changed over, box by box, carefully ensuring that all is working easily and smoothly at one box before another is brought into use. We never had the slightest hitch.

There is the further consideration of simplicity in working, and that is particularly important on busy lines. I know that on the Great Eastern Railway, when it was under consideration to adopt the single-needle block, it was found absolutely impossible to adapt it, with its code of signals as in use on other railways, to the requirements of our heavy suburban traffic.

We could not have got our traffic through.

In signalling trains there are a certain definite number of bell, plunger, and (in the single-needle block) needle, movements required to be transmitted by the signalmen for each train. I give a few of the code requirements:—

Tyer's double line block	19	movements.
North Eastern single-needle block	30	„
Great Northern single-needle block	32	„
Midland single-needle block ...	44	„
Sykes's lock and block (G. E. form)	11	„

It will thus be seen how important the question is. So simply and so rapidly can the Sykes system be worked, that we have boxes manned by one man where there are six lines of rails, with two or three sidings connections, and about 1,000 trains per day passing. In one case it will be seen that at these boxes there is a saving of 33,000 movements per day. I say distinctly that with the single-needle block, for instance (with any of the codes named), this working would be absolutely impossible.

The PRESIDENT: Gentlemen,—We have already passed a vote of thanks to Mr. Hollins; it only remains for us to express the The President.

The President. pleasure with which we have listened to his reply. The paper was one to which it was scarcely possible to do justice without studying it line by line. Having done this, we can more properly appreciate the immense amount of thought which has been bestowed by various workers on a branch of electrical engineering of which we hear but little, but to which we are greatly indebted. To those who pass a considerable portion of their lives on the railway it is comforting to know, and a matter for congratulation, that the ingenious appliances designed for the public safety act with such unfailing precision.

I now call upon Mr. Ernest Wilson to read his paper on "The "Relative Size, Weight, and Price of Dynamo-electric Machines."

THE RELATIVE SIZE, WEIGHT, AND PRICE OF DYNAMO-ELECTRIC MACHINES.

By ERNEST WILSON, Member.

PART I.—SIZE OF MACHINE.

Mr. Wilson. In the design of dynamo-electric machines the rate at which work is done on or by a machine at a given speed of rotation is amongst other things, an important consideration. The output' of a generator of electricity is stated, in the case of steady currents, when the potential difference in volts at the terminals of the machine, the current in amperes supplied by the machine to the external circuit, and the revolutions per minute of the machine, are given. This also holds with alternate currents on non-inductive loads. The quantity

$$\frac{\text{Volts} \times \text{amperes}}{\text{Revolutions per minute}},$$

which is here called the "mass factor," bespeaks, with certain limitations, the size or mass of the machine. The limitations, to be treated fully, are somewhat complex; for instance, the potential difference at the terminals of a generator, for a given speed of rotation, can be varied by varying the conductors on the armature, or, again, by keeping the number of conductors constant and varying the field. In the former, the percentage increase or decrease of insulation material has to be considered when dealing with wide limits, and the consequent variation of current-density

in the conductors if the same output is required, involving the important question of compliance with given temperature specifications; in the latter, the weight of iron in the field magnets can be varied, but since the bed-plate, bearing brackets, armature, &c., form a large proportion of the total weight of the machine, the consequent variation in this total weight is smaller. Then, again, the ultimate temperature of the armature on load is a function of the speed:* armature reaction and efficiency variation with speed have to be considered, and so on. There is, however, no doubt but that this mass factor can be of considerable use to the designer of dynamo-electric machines; for instance, when dealing with a given type of machine, such as the two-pole with drum armature, so largely used at the present time for central station work, a salient feature of such a machine is the diameter of the armature. Another feature is the breadth of the magnets parallel with the armature shaft. Let a curve be drawn in which the ordinates are the mass factors, and the abscissæ the diameters of the armatures, according to any good practice. The curve in Fig. 1 (Plate 1) illustrates this relation. For each diameter a series of values of $\frac{V \times A}{R}$ are given, each corresponding to a series of breadths of magnet limb for a given thickness, or a series of thicknesses of magnet limb for a given breadth, or breadth and thickness may each be varied simultaneously. Such a curve for a given type is of use to the designer, and is the outcome of actual experience in the best proportions for a given output. An important feature of this curve is that the diameter at each successive increase covers a greater range of variation in the mass factor; that is to say, the diameter tends towards a maximum. It is, of course, clear that the practice of one firm may produce a different curve to the practice of another in the design of the same type of machine, and each firm may turn out equally good machines. The quantities involved may be varied through fairly wide limits. The salient features of other types of machines, whether for steady or alternate currents, might be treated in the same manner. Mr. Wilson.

* See *The Electrician*, October 11th, 1895.

Mr. Wilson.

Part II.—Weight and Price.

The object of this part of the paper is to investigate the way in which the weight and price of dynamo-electric machines, as constructed by different firms, vary with the variation of the mass factor. The figures in Tables I. and II. refer to English manufacturers, and were recently (October, 1896) sent to the author (except in one instance—No. 3, Table I.): they therefore represent latest practice. The data contained in Table I., from which the curves in Figs. 2 and 3 (Plate 1) have been plotted, refer to steady-current dynamos, complete, for belt driving. In most cases the mass factor has been deduced from machines giving about 100 volts, and the headings in Table I. give the curve number in Figs. 2 and 3, and type of machine to which the figures refer. With regard to the weight of the dynamo, it must be remembered that the power it supplies at a given number of revolutions per minute can be varied considerably without affecting the net weight. Due notice must be taken of efficiency, and the compliance of the machine with given temperature specifications. Broadly speaking, the curves in Fig. 2 show that the weight increases initially somewhat rapidly as the mass factor increases, and then develops into a fairly straight line. With regard to price, the initial increase is more marked. Now that the curves are drawn one naturally compares them with one another, and it is interesting to note how, both in regard to weight and price, the different firms in the manufacture of two-pole machines have come to work, with one or two exceptions, nearly on the same lines. In the comparison of the curves with one another one should take due notice of efficiency, and the compliance of the machine with given temperature specifications. The weight and price of the multipolar machines, No. 12, are lower than any quoted for a mass factor of 110. It is well known that multipolar dynamos are lighter and cheaper than the ordinary two-pole machine for large sizes, but there is not sufficient data at the author's diposal to fix *definitely* the point where this becomes important.

In the case of alternate-current dynamos the mass factor is equally useful for comparison of weight and price. Since in

a given machine the frequency and volts at the terminals on Mr. Wilson.
open circuit are each proportional to the revolutions per minute, it follows that the mass factor is independent of frequency if we assume equal armature reactions. We must not forget that the product of volts and amperes is only permissible when the current is in phase with the potential, as on non-inductive loads such as glow lamps. In the transmission of power to motors, for instance, the current may or may not be in phase with the potential. In the latter case the current must be increased for the transmission of the same power. Then, again, there is the loss in the pole-pieces,* which may or may not exist in a given machine. For a given speed of rotation, if we vary the frequency, we must vary the number of poles; but this requires an inverse variation in the field to give the same volts for the same armature. This might have an effect upon both weight and price, but of course the choice of frequency is dependent upon other external considerations. The curves in Figs. 4 and 5 (Plate 1) have been plotted from the data contained in Table II., and refer to single-phase machines without exciters, complete for belt driving. An inspection of the curves shows again that the initial rise is greater in the case of price than in weight. The difference between the costs by the different makers is more marked than in the case of steady-current machines.

Taking the whole of the curves collectedly with regard to weight, the steady-current machines and alternators are about equal up to a mass factor of 100 or 120; beyond this, the alternators would seem to be somewhat heavier. In regard to price, the alternators would seem to be dearer all through.

It is almost unnecessary to add that the mass factor can be used in connection with motors as well as generators.

I wish to thank Messrs. Woodfield, Tomkins, and Strutt, demonstrators in the Siemens Laboratory, King's College, London, for the help they have given me in the preparation of this paper. I have also to thank Mr. C. J. Evans for checking the figures and curves.

* See *Phil. Trans. R. S.*, vol. clxxxvii. (1896), A, pp. 229–252.

Mr. Wilson

Table II.

1. Up to 5,000 Volts, any Frequency.

K.W.	Revolutions per Minute (approximate).	$\frac{V A}{R}$	Nett Weight (approximate).	Price, f.o.r.
			Cwts.	£
5	1,500	8·3	12	75
15	1,000	15	25	150
30	750	40	40	240
50	550	91	90	380
75	500	150	130	500
100	420	238	180	650

2. 1,150 to 2,300 Volts, Frequency 125.

K.W.	Revolutions per Minute.	$\frac{V A}{R}$	Nett Weight.	Price, f.o.b. London.
			Cwts.	£
30	1,500	20	22	125
60	1,500	40	35	173
90	1,250	72	70	293
120	1,070	112	84	350
300	682	440	198	973

3. 1,000 to 1,050 and 2,000 to 2,100 Volts, Frequency 100.

K.W.	Revolutions per Minute.	$\frac{V A}{R}$	Nett Weight.*	Price, f.o.r.
			Cwts.	£
12·5	1,200	10·4	28	200
25	857	29·2	37	285
30	857	35	38	310
37·5	666	56·3	56	340
45	666	67·6	57	420
50	600	83·3	90	450
60	600	100	92	540
75	500	150	141	660
80	500	160	144	705
100	428	234	196	880
250	300	833	378	1,870

4. 2,100 Volts, Frequency 83·5.

K.W.	Revolutions per Minute.	$\frac{V A}{R}$	Nett Weight.	Price, f.o.r.
			Cwts.	£
15	835	18	28	185
25	716	35	44	230
40	626	64	52	295
65	556	117	88	390
100	500	200	118	550
150	417	360	185	750
200	358	539	235	980

5. 1,000 to 1,050 and 2,000 to 2,500 Volts, Frequency 75.

K.W.	Revolutions per Minute.	$\frac{V A}{R}$	Price in London.
			£
12·5	1,125	11·1	180
20	750	26·7	250
30	643	46·7	350
50	560	89·3	410
80	500	160	630
114	450	258	825
120	375	320	1,080

* These weights have been deduced by the author from gross weights by deducting 20 per cent. from the first three, 10 per cent. from the next seven, and 6 per cent. from the last.

Mr. W. B. Esson: Before the discussion commences, I should like to ask Mr. Wilson a question with regard to the diagrams. I find that the curves sent with the proofs differ considerably from the enlarged curves shown on the wall, and I do not know exactly where I am in the matter. In Fig. 2 in the proof sheet there is a very decided kink in curve 3 before it arrives at the figure 250; whereas it is not so shown on the large diagram. In Fig. 3 of the proof sheet you will find that curve 5 is above the other curves all the way; whereas in Fig. 3 on the wall curve 5 dips underneath curve 3 at a point somewhere before 200. If Mr. Wilson will tell us whether the mistake is in the proof sheets or on the wall diagrams, we will be better able to see where we are. Mr. Esson.

Mr. E. Wilson: Will you please adhere to the diagrams in the paper? Each of the members has got the diagrams, and you had better adhere to them. Mr. Wilson.

Professor Silvanus Thompson: We are indebted to Mr. Wilson for a useful contribution to this aspect of dynamo machinery. If he has not been able to give us all the information we would desire, that probably is not his fault. One knows that it is always a little difficult to get precise information on some points from the manufacturers. The first thing that strikes me on looking at this paper is the nature of this quantity which he defines as the "mass factor"—the output of the machine divided by the number of revolutions per minute, or, in other words, the watts per revolution. May I be permitted to raise the query whether that is really the best basis of comparison? It will be seen that this is a basis in which the quantity called "mass "factor" comes out large for slow-speed machines quite irrespective of the size of the revolving part. In brief, in adopting this basis, you have a machine of very substantial construction, suitable for a high peripheral speed, classified along with another machine to run at the same nominal speed, having as its revolving part something of lesser diameter, of more slender construction, and which possibly could not be run safely at any higher speed than it is run at. The consequence is that you are comparing together as of the same class, so far as speed of revolution is Professor Thompson.

Professor Thompson.

concerned, two machines, one of which has a very much larger peripheral speed than the other. I think the right basis of comparison would rather have been the number of feet per minute moved through by the revolving part of the machine, whether armature or field magnet. The electro-motive force which a wire generates in a given field does not directly depend upon its angular velocity, but depends on its linear velocity. The right basis of comparison, it seems to me, would be to take the output divided by the linear velocity of the conductors or the field magnets, as the case may be. Now, if one adapts to this order of ideas such information as is given, one gets at a somewhat interesting result. Take the first diagram. You have the so-called "mass factor" plotted vertically as ordinates against horizontal abscissæ representing the diameters of the armatures; the latter being missing quantity, in fact, which will enable one to connect the two things I desire to compare together. But suppose we multiply both the quantities plotted, the vertical and the horizontal, by the revolutions per minute, we shall have as ordinates simply the outputs, as abscissæ quantities that need only to be multiplied by 3·1416 to give the linear speed of the periphery. The same diagram, therefore, will, by a mere change of scale, give us linear speed horizontally against the output of the machine vertically. We then have the information given, in the form of an irregular curve, that for this particular set of machines to which the diagram relates the output is not proportional to the linear velocity, but increases much more nearly as its square; and, apparently, experience has dictated to the makers of this particular line of machines that there is one particular linear velocity which it does not do to overpass. It is pretty well known that for a given type of machine it does not do to run that machine beyond a certain linear peripheral velocity. Those machines that are substantially built, and have nothing but masses of solid iron running round, clearly may be run with a higher linear velocity than others. So that for the purpose of comparison it would be very useful indeed to have a number of similar diagrams so plotted. We could then compare the curves for a set of machines of one type with those of a set of machines on another type.

Every engineer no doubt desires to make the best use of the materials—the iron and the copper—at his disposal. The question of the specific utilisation of material in a dynamo machine has not claimed much attention of late, though some 10 years ago it was much discussed. Do you, as a matter of fact, when you build large dynamos, make a better use per pound of your iron and of your copper? Certainly you do, if you can run the machines with a higher peripheral velocity. It comes back to a matter of design whether you can, by good designing, procure machines which can be run with a higher peripheral velocity. If so, then you will be able to make better use per pound weight of your materials—iron and copper. Professor Thompson.

There is one direction in which progress is no doubt tending. I cannot see any trace of allusion to it in these figures which Mr. Wilson has put before us, for the reason that he has carefully concealed the names of the manufacturers who have furnished these data; and the data have not enabled me to identify the particular manufacturer about whom I want to inquire. We had at this Institution not so very long ago two papers, on certain matters of dynamo construction, by Mr. Sayers. The whole point of Mr. Sayers's communications was that you might make a dynamo of less weight and cost by so designing the parts that you should be able to get rid of the demagnetising effect of the armature, or compensate for it, and therefore be able to work with less copper on your field magnets, therefore with smaller field magnets, and therefore with a machine of less total weight. In spite of the larger amount of workmanship required in the winding of a Sayers armature, the saving in material in the field magnets more than compensates for the cost; and thus machines so designed can be produced at a lower price than machines in which there is no such compensation. If it were possible to find by any extension of Mr. Sayers's ideas a reasonable way in which the armature of a dynamo could be made to magnetise its own field magnets without any field winding at all, then I am perfectly sure we should be able to make dynamos with very much less material, and certainly with greater specific utilisation of material. We could get a greater output for the same weight of iron and

Professor Thompson.

copper. Now it seems important in considerations of this kind that we should avoid anything like a misleading basis. Far be it from me to say that this basis of the "mass factor" is misleading. I only want to point out that it is not the only basis, nor perhaps the right basis in all cases, for a comparison. It happens that some 14 years ago I listened in this room to an exceedingly important and able paper on the design of motors, and the authors of that paper laid it down as a general proposition that motors ought to be designed on different lines from dynamos. We were told that motors ought to have comparatively small and unimportant field magnets, and with comparatively large and powerful armatures; the whole object of this being—and this was the misleading premise underlying the whole thing—to reduce the weight of the motor to a minimum. Now, if you grant that the right way to design a motor under all circumstances is that you must sacrifice everything else to get the gross weight down to the minimum, you will certainly arrive at the same conclusions. But is that the right and only way to design motors? Is it an essential of motor design that the weight must be kept down, to the sacrifice of everything else? Clearly it is not; and all practice for stationary motors has gone the other way. If there is any difference at all nowadays between the design of motors and of dynamos, it is that the field magnets of the motors are made rather heavy and more powerful relatively than is the case for the dynamos. The arguments stated 14 years ago really retarded the progress of motor construction in the industry for a time, as it was not generally understood that the arguments put forward rested on that narrow basis—the idea that the motor must be designed so as to have minimum weight.

Very little has been said by Mr. Wilson about the question of efficiency. He recognises, of course, as we all do, that efficiency governs the design, and that probably the differences observed in some of these curves are accounted for by the circumstance that some firms work under Admiralty rules as to temperature and so forth, while other firms do not work under such narrow specifications as to heating. Consequently, you have not the same attempt to save in the waste of energy as would be the case if it

were intended to comply with more strict regulations. No doubt that will account for some of the discrepancies between the curves of one manufacturer and another. There are no curves given for arc lighting machines. It would be rather interesting to see how they would come out. In the case of arc lighting machines where the current is invariable the efficiency enters into the problem in a somewhat different way, the armature heating being equal at all loads. Lastly, one very minor point of criticism. Why does the author suggest that there is anything unsteady about alternate currents? Why should continuous currents be dubbed as "steady" ones, and alternate ones, by implication, be supposed to be unsteady? Professor Thompson.

Mr. H. W. RAVENSHAW: I will divide the curves into two classes—namely, those which are taken from the prime costs, and those which are averaged from the prices of other firms. In many cases I have heard it said, when a tender was being prepared, "What do you think So-and-So's price will be?" and I am afraid that a great deal of money has been lost owing to the cutting down of prices to the level of some particularly low price list. Mr. Ravenshaw.

Some of the curves are very regular indeed, and I cannot help thinking that the shape has something to do with the particular French curves in the possession of the estimating clerk. Other curves wave in a curious manner, and are perhaps taken from the prime costs of machines which are made in sets; all the machines in one set having the same diameter of armature, bearings, commutator, &c., the length of armature varying with the output. The longest machine in the set is usually a good deal cheaper than the shortest in the next set, so that the prime-cost curve will not be regular.

The cross field in a Gramme armature causes very heavy eddy-currents in the shaft and spiders, and in machines of 30 kilowatts and over the watts lost due to this cause are often greater than the $C^2 R$ losses. As these losses vary as the square of the speed, it is not possible to run large Gramme armatures so fast as those with drum windings. The fact of some makers using cast steel, and others cast iron, for the magnets, would have some effect on the curves comparing "mass factor" and "weight."

Mr. Ravenshaw. I think the curves would have been more valuable if the net prices had been taken. Some makers give a good deal more discount than others, so that the curves do not correctly represent the relative prices of the dynamos.

Mr. Scott. Mr. E. K. SCOTT: Figs. 6, 7, and 8 (Plate 2) show the curves of some machines I designed for the Brush Company about 4½ years ago, and, although I did not use the "mass factor" (which, by the way, I am sure dynamo designers will find very useful), I find that they follow Mr. Wilson's curve No. 5 very closely. Of course I carry them much further, because it was our object at the time to get on to the market with the larger types of machines. I wish we could have had the discounts knocked off the prices in Mr. Wilson's curves, because some makers take off 20 per cent., and 2½ per cent. besides the 2½ extra for cash, whilst others only take off 15 per cent., and some less than that. This would alter the price curves considerably, and would, I think, tend to bring them more together. As a matter of fact, it appears to be the rule of the trade to quote the full discounts to every buyer, excepting, perhaps, in the case of shipping orders. With regard to the Sayers machine, I was called in to advise, along with Mr. Ravenshaw, with regard to a very large size of Sayers wound machine some time ago, and I have since heard that the brushes need as much attention as those of an ordinary machine. For my own information, I went into the subject from the multipolar point of view, and I really found that if the machine had been built ordinary multipolar it would have been an advantage. I went into it very carefully indeed, comparing with some particulars which were from actual cost and actual weights, and I came to the conclusion there was very little in the Sayers winding. If the forward lead of brushes is a disadvantage, there are other simpler ways of minimising it.

Mr. Geipel. Mr. W. GEIPEL: I have only one small suggestion to make, if it is not hypercritical, and that is, that instead of the use of the words "mass factor" we might continue the use of the words "watts "per revolution," which we have used in our designing department at the Brush Company for some time past. I think also that a more useful curve would be obtained by comparing the

price of the machine with the watts, giving the price of the machine per watt; that is to say, the ordinates would be given in pounds and the abscissæ in watts. You would then get the price per watt for the different sizes of machines, which would, I think, be a more useful curve than the curve given by the author. Mr. Geipel.

Mr. S. MAVOR: I do not know that I can add anything of value to the discussion. Mr. Sayers's winding has been referred to by Professor Thompson, and this winding has an important bearing on the subject of the paper. My firm has had large experience in the use of this method of winding armatures, and it has been entirely successful. Professor Thompson's doubt as to the additional cost of winding may be set at rest. In all machines with Sayers's winding, except small ones, the additional cost of the armature, which is really very trifling, is more than recouped by the saving in weight of the magnets; and this applies to all machines above 6 or 8 kilowatts. In machines smaller than that the advantage is not considerable. I have plotted on Fig. 2 a number of Sayers wound machines, and find the weights for corresponding mass factors are considerably lower than any of the curves shown; but I do not submit curves of our machines, as, owing to the absence of data relative to efficiency, temperature, and the weights of the bed-plates, the curves given with the paper have little value for comparison. Mr. Mavor.

I may add that Mr. Wilson's "mass factor" corresponds with the "machine constant," which term is used in my firm's catalogue to indicate the size of machines. For dynamos the machine constant $= \frac{\text{watts}}{\text{revs.}}$, and for motors the machine constant $= \frac{\text{H.P.} \times 1{,}000}{\text{revs.}}$.

Mr. W. B. ESSON: Mr. Wilson in his paper has not laid himself open to much criticism. I do not think there is a single point that one could controvert in the whole paper, as it deals only with hard facts. The paper is short, it is interesting, and it is true; the latter attribute being, of course, from the critic's point of view, somewhat of a disappointment. The interest of the paper centres rather, I think, in the fact that it introduces a Mr. Esson.

Mr. Esson. new mode of thinking about dynamos, than that it throws any new light on the subject of designing. We have been accustomed for years past, when comparing the weight-output ratio of dynamos, to reduce the speeds of the machines compared, either to one common speed basis of so many revolutions per minute, or to a basis of equal circumferential armature velocities. Mr. Wilson introduces quite a new way of looking at the thing, and in the short time I have had to read the paper I do not think that I have grasped all its possibilities. As regards the methods of comparison I speak of, both are attended with considerable disadvantages, and I take it that it is with the object of getting rid of some of those disadvantages that Mr. Wilson introduces to our notice this new ratio which he calls the "mass factor."

In Fig. 1 the author gives the relation of the mass factor to the diameter of the armature. The output of a dynamo, as I pointed out some time ago, is approximately proportional to the product of the diameter of the armature squared, the length of the armature, and the number of revolutions. From this it follows that the "mass factor" is proportional to the diameter of the armature squared into the length of the armature. It seems to me that that curve of Mr. Wilson's connecting diameter and mass factor is too steep. I do not know whether Mr. Wilson agrees with me with respect to this relation, but, if so, it appears that the curvature upwards is too rapid.

I was very much struck with that beautiful, regular line, curve 5. At first I thought that it must represent my own dynamos, it is so regular, gives plenty of weight, and the price is not cut too finely. I was very soon undeceived on that point, however, for, on asking my estimating department to run through Mr. Wilson's tables and spot the makers of dynamos for which he had given the figures, I was told that ours could not be found in the list; nor could the figures be made to tally with those of any other makers whose lists we had. I may tell you that, as a matter of curiosity, we keep by us the price lists of all makers of repute throughout the world, so where Mr. Wilson got his figures from is at present a mystery—to me, at least. However, he has got them from somewhere, and the curves which

give them graphic interpretation show very considerable differences in weight and price for the same "mass factor." For instance, in Fig. 2 I find that in the case of a 50-kilowatt machine, running at 500 revolutions—corresponding to a mass factor of 100—one maker gives a weight of about 4 tons, while another gives over 5 tons. The price variation is larger still, one being put down at £320, and the other at £450. Mr. Wilson says that all the makers are tending in the same direction; but I should say, according to his figures, some are tending in the way of building up a profitable business, while others are tending in the direction of getting official recognition from the Official Receiver. Mr. Esson.

But dynamos having not only the same mass factor, but giving the same amperes and volts at the same speed, vary very much, according to the skill or ideas of the designer. There is a central station not very far from here where one can get objective evidence of the different ideas held by designers with reference to proportion. The station has two pairs of direct-driven dynamos, by different makers. I will not give the names, but will distinguish the machines by the letters E.H. and G., and of course you would never guess whose makes they are. These machines were, I presume, made to the same specification, and they give each 500 amperes and 225 volts at 350 revolutions per minute. Here are the leading dimensions:—

	E.H.	G.
Diameter of armature	17½ ins.	22 ins.
Length of armature (effective, measured on magnet)	24½ "	30 "
Width of magnet	16 "	11 "
Armature conductors	172	160
Commutator sections...	86	80

According to these figures there is nearly 20 per cent. more iron in the field of the machine having the smaller armature than in the other. The total magnetic flux is not very different in the two machines, as the number of conductors on the armature is not very different; but in the E.H. machine the lines are crammed through a very much smaller area of air gap, so that in the space between the field and armature the density is 40

Mr. Esson. per cent. greater in this machine than in the other. It is needless to say that with such a high density the effect of armature reaction is negligible, and that the machine is sparkless. At the same time it is to be observed that the current-density in the bars of the small armature is far in excess of that in those of the large, and, as a consequence, running under full load, the rise in temperature in the E.H. is 50° Fahr. more than in the G. machine. This question of temperature Mr. Wilson has not said much about, but it really influences the matter very much. He has given us some data, but not enough; and we want to know the efficiency of the various machines which are indicated in the curves, also their temperature rise, so that we can arrive at a fair conclusion.

I do not think I have anything further to say, except to thank Mr. Wilson for his paper. After all, the basis of comparison, whatever it may be, is inevitably reduced to £ s. d. terms; and Mr. Wilson, in suggesting a new output-weight-price comparison, has, I think, hit upon something which will be of considerable use to dynamo designers.

Mr. Wilson. Mr. ERNEST WILSON, in reply, said: Mr. President and gentlemen,—I thank you very much for the kind way in which you have received this short paper—you can hardly, in fact, call it a paper—this short note on dynamo working. It is rather difficult to answer all that has been said, replying as I do immediately after the discussion, but I will do my best. I must apologise for the discrepancy between the curves on the wall and those in the paper, pointed out by Mr. Esson. The reason I stated we had better adhere to the curves in the proof was this: Those curves were plotted in the first instance, and were sent to the printers. I had then only the table of figures left, and Mr. Strutt, one of our demonstrators, plotted the points of the curves on the wall, and drew them through to the best of his ability. You can very well understand that two people drawing curves like these, through the same points, would not draw them exactly the same, although no doubt the general run of the curves would be practically similar in each case. That is the reason there is a little disagreement.

Now, with regard to Professor Thompson, this is a very nice way of looking at the matter; but, as he mentioned, it was impossible for me to give more information than I had, and I could not put the subject forward in that light. Of course those who have the figures can work it out from this point of view for themselves. I simply had the data given in the lists. There were no diameters of armatures, or anything of that kind. I just took the simple volts, amperes, and revolutions per minute. There is one thing in regard to this mass factor, and that is what the engineer really wants. My point is simply this: You give me an output in watts at a certain number of revolutions per minute—that is, a certain mass factor; I can choose what type of machine I like, but at that speed I have got to produce that output with certain economy, and it is just a question as to which is the best type of machine to choose for the purpose. What the engineer is really interested in is the weight, dimensions, and the price of the machine for a given mass factor and a given economy, leaving the type of machine altogether out of the question. That also leaves out of the question the peripheral speeds of armatures; these being matters for the designer. Professor Thompson mentioned arc lighting machines. I have dealt simply with about 100-volt direct-current machines and single-phase alternators. I have not mentioned anything specially in the way of arc lighting, because I had no data by me. I have only dealt with matters in which I had a good number of curves, for the reason that it is only in numbers you can get any basis or any comparison that is worth having. I have some interesting data with regard to multiphase working, but very little of it, and it was hardly worth while incorporating it in the paper. Then Professor Thompson asked why I used the word "steady" in preference to "continuous." When all is said and done, an alternating current is continuous—I might put it in that way—so long as it keeps flowing. For my own part I much prefer the word "steady" to "continuous," but "uni-directional" is better than either. Mr. Wilson.

Mr. Ravenshaw mentioned the net price. As I said, I could not possibly bring in trade discounts. It would simply mean

Mr. Wilson. dealing with many matters which I could not properly handle, and therefore I let it alone. I simply took the prices as they were given. You all know perfectly well that the price of the machine depends on the customer. The same machine would be sold at different prices. All I could do was to take the prices as given in the lists. Mr. Scott has worked out his own figures, and they give the same character of result as in the paper. I am pleased he thinks this way of looking at the matter may be useful. I am also pleased to hear from Mr. Mavor that the Sayers machine, when armature reaction is obviated in the way mentioned by Professor Thompson, gives such good results. Of course it is an exceedingly important question in many ways.

Then with regard to Mr. Esson: He points out that it is really a disadvantage to employ the linear method of comparison which Professor Thompson mentioned.

Professor THOMPSON: He gave no reasons.

Mr. ESSON: I will put the reasons in the *Journal.*

Mr. WILSON: Mr. Esson thought Fig. 1 was too steep. The curve in Fig. 1 is not drawn to scale. I have not put in the diameters of armatures in inches, and the mass factor in any actual figures. It is simply to indicate the sort of thing you get. Naturally, different people might bring out equally good machines, and work with curves which were in themselves somewhat different; but *that* is the general run of things you will find if you plot your diameters. I daresay Mr. Esson's own data would show the same shape of curve, though it may not, perhaps, have the same proportion. With regard to his inquiry as to where the figures came from, I can assure him they all came from England, and I received them through the post in the ordinary manner. I am very much surprised he has not been able to identify practically all the curves. Temperature and efficiency, as you know, are not given on the price lists, and it is of importance to know these figures. At the same time, I believe that all those figures which are given in the price lists sent out by the different firms really represent a good machine. If you want a machine which is to fulfil special conditions—perhaps

better conditions than the Admiralty, or even as good—with regard to the rise of temperature, you may have to pay a little more for it; but I believe the data I have given in each case represent a good machine. Of course I cannot possibly give the rise of temperature on a six hours' run at full load and the efficiency of these machines. With regard to other matters, the data given in the paper may be to some extent tentative; for instance, the introduction of forged steel in the making of dynamos would no doubt bring down their weight somewhat, and of course there is the improvement in design. One thing I should like to have heard something about was the question of the four-pole machine. There is no doubt that it is lighter and cheaper in the larger sizes, but none of the speakers mentioned anything on the subject. Then I think there can be no doubt that the alternators over there are, on the whole, dearer than these direct-current machines for the same "mass factor." There is no getting away from that, and you must remember that they are dearer and heavier without the exciters. That is an important thing. If you add on the exciters, it will not only make them heavier, but add to their expense. Whether it is a matter of frequency or not, is a question I cannot very well go into. Then there was the question of multiphase working. That is a point of exceeding interest, and a point which has not been touched on in the discussion. I was hoping some members would give information with regard to multiphase working. I believe I am right in saying with regard to three-phase that the machines do come out not only lighter, but also cheaper. I have some data here which show that you do get certainly lighter and cheaper machines when you use three-phase currents instead of direct currents. Whether that is generally so I would not like to say. I have not given this information in the paper, because I have little of it, and I thought it best not to bring anything forward unless I had sufficient to form a good basis of comparison. The dimensions of this "mass factor" are those of work per time divided by a velocity—that is, they are the dimensions of force—that is, under constant acceleration *mass* is the variable. That is all I have to say in this immediate reply to the discussion. Mr. Wilson.

The President.

The PRESIDENT: Gentlemen,—We have had a very practical paper, which no doubt will commend itself to all of you. Mr. Wilson in his reply has endeavoured to rectify any omissions he may have made in the original paper. I am inclined to think that, if some of our manufacturers had chosen to come forward, we should have elicited the fact that they had some such formula in use at their own factories; indeed, I know of one where they designate it, not the "mass factor," but the "output factor." Curves taken from catalogues are likely to be sometimes misleading, as there is a tendency amongst manufacturers to follow one another in their figures. I suppose they think that imitation is the sincerest form of flattery. Mr. Wilson's formula will no doubt be useful in comparing machines of the same type. He has explained why he has not gone into the question of efficiency; he has simply made the best use he could of the figures available to the public. In conclusion, I may point out that we have rather overlooked the point of efficient commutation: this has a great deal to do with the value of a dynamo; and we should also take into consideration its non-sparking qualities.

I am sure you will, in accordance with our usual custom, pass by acclamation a vote of thanks to Mr. Wilson for his paper.

The motion was carried with acclamation.

The PRESIDENT: I have to announce that the scrutineers report the following candidates to have been duly elected:—

Members:

C. P. Hammond.	Hermann Charles Warburg.

Associates:

George Archibald March.	Robert Buckland Miller.

Students:

Wilfred Birks Cleeves.	Robert Grigg.
Frederick William Fawdry.	John M. A. Margetts.
J. H. Grieve.	George Masinier.

William Denton Perrott.

The meeting then adjourned.

ABSTRACTS.

J. BLONDIN—THE SWISS ELECTRICAL INSTALLATIONS FROM GENEVA TO ZURICH: CENTRAL STATION OF THE BERNE MUNICIPAL INSTALLATIONS.

(*L'Éclairage Électrique, Vol.* 9, *No.* 52, *p.* 590.)

This installation works on the three-wire continuous-current system, the supply being at 100 volts.

The water power of the Aar is utilised; for which purpose a dam has been constructed. The general fall is 4 metres. Five Jonval turbines of 150 H.P. each are installed, running at 40 revolutions per minute.

The dynamos consist of eight-pole Œrlikon machines, running at 360 revolutions per minute, and driven from the turbines by means of spur gearing with wooden teeth. Their output is 700 amperes at 120 to 140 volts. Two of these generators are continuously working; the third is kept as a spare. The installation includes two boosters, direct coupled to electro-motors. Their output is 340 amperes at 10 to 50 volts when running at 900 revolutions per minute. These boosters are used for charging four batteries of Tudor accumulators, consisting of 72 cells per battery, 18 of which are used for regulation. The total capacity of the battery is 1,400 ampere-hours. The overhead feeders run along the banks of the Aar, and are connected at two points to the underground system of distribution.

J. BLONDIN—THE SWISS ELECTRICAL INSTALLATIONS FROM GENEVA TO ZURICH: THE WYNAU CENTRAL STATION.

(*L'Éclairage Électrique, Vol.* 9, *No.* 52, *p.* 591.)

This central station was carried out by the firm of Siemens & Halske. The motive power is obtained from the river Aar. A dam has been constructed for this purpose, giving a fall of about 4 metres, and an available power of about 5,000 H.P.

Seven Rieter turbines are installed, there being five of 750 H.P. and two of 120 H.P.

To the shaft of each of the large turbines is coupled a three-phase alternator, giving 450 volts at a speed of 150 revolutions per minute. The two smaller turbines drive two continuous-current dynamos used for exciting the alternators, and for other purposes. The alternating current is conveyed by concentric cables to 12 200-kilowatt transformers, which raise the pressure to 8,000 volts. The energy is then transmitted to a distance of 25 kilometres by means of overhead conductors, there being six lines of conductors. Netting is placed under the conductors where they cross roadways. Each conductor on leaving the station is fitted with a Siemens & Halske lightning protector. In the villages the pressure

is reduced to 500 volts for working motors. For incandescence lighting the pressure is further reduced to 120 volts. The energy is sold through the municipalities. There is a mill at Roggwyl, some miles from Wynau, which alone takes 280 H.P. on a secondary circuit of 120 volts, part of this load consisting of an 80-H.P. motor.

M. G. LIPPMANN—A METHOD OF COMPARING BY MEANS OF THE ELECTRIC SPARK THE DURATION OF OSCILLATION OF TWO PENDULUMS REGULATED APPROXIMATELY TO THE SAME PERIOD.

(*Comptes Rendus, Vol.* 124, *No.* 3, *p.* 125.)

The method adopted by the author is more convenient than the method of coincidence, and allows of a comparison between two pendulums, without disturbing them or without loading them.

The ratio of the present oscillation of two pendulums, A and B, is measured by illuminating them by the discharge spark from a Leyden jar. With this light the pendulums appear to be perfectly stationary, and their position at this moment can be observed or photographed.

If a and b be their respective phase at one moment, and $a' - b'$ at another moment, and n be a whole number, then the required ratio is

$$\frac{n + a - a'}{n + b - b'}.$$

It is proposed that the pendulum A should be arranged to break an electric circuit in which would be placed a relay operating on the primary circuit of an induction coil connected to a Leyden jar; the spark thus produced would be used to illuminate the position of the second pendulum, B. Its position on a scale would be noted by means of a telescope. This method is extremely sensitive.

L. BENOIST—THE LAW OF TRANSPARENCE OF GASES TO THE "x" RAYS.

(*Comptes Rendus, Vol.* 124, *No.* 3, *p.* 146.)

The opacity of bodies to the x rays increases with their density, but there does not exist any direct proportionality in the case of solids. The author thought that gases might offer simpler results, and studied their power of absorption to the x rays by employing the electrometric method first suggested by M. Hurmuzescu and himself.

The gases are enclosed in a metallic cylinder 74 centimetres long, fitted with aluminium ends. This cylinder is placed between the electrometer and the Crookes tube, and the gases within it raised alternately to pressures of 1 and 2 atmospheres.

The following results show that a proportionality exists:—

	Absorption, a.	Normal Specific Mass, m.	Quotient, $\frac{m}{a}$.
Sulphurous acid	0·263	2·861 gr.	10·87
Methyl chloride	0·223	2·254 gr.	10·11
Air	0·111	1·293 gr.	11·60
		Mean	10·86

This is the relation which M. Lénard had obtained in the case of cathode rays, before Röntgen's discovery. By calling the specific power of absorption of a substance that which corresponds to a strip the thickness of which represents unit mass per square centimetre of surface, it will then be seen that this specific power is a constant for a given initial pressure and temperature, and for the same kind of rays.

The author proposes to utilise the variations of the constant to differentiate these rays.

It would, moreover, be advisable to compare the specific power of absorption of different bodies to that of gases; thus, perhaps, obtaining a better relation between solids and liquids.

The specific power of absorption of gas, resulting from the preceding determination, is 0·14. In the case of aluminium this value is 0·09; this somewhat low value being perhaps explained by luminescence phenomena. Silver gives the value 0·84. The difference appears to increase with the density in the case of solid bodies. The author has further observed that the absorption diminishes rapidly with an increase of temperature under constant pressure, this being predicted from the law of densities.

The author proposes to investigate whether the specific power of absorption is independent of temperature, as would be expected, at least under constant volume.

M. RADIGUET—THE FLUORESCENCE OF VITRIFIED MATERIALS UNDER THE ACTION OF THE RÖNTGEN RAYS.

(*Comptes Rendus, Vol.* 124, *No.* 4, *p.* 179.)

The author has discovered that the following substances become luminous under the action of the x rays, arranged in decreasing order:—Baked enamels, crown flint glass, ordinary glass (and particularly crystal glass), Saint-Gobain mirror, porcelain, enamelled faience, enamel powder before baking, and even cut diamond. It is known, moreover, that most of these substances are more or less fluorescent when exposed to the action of violet and ultra-violet rays. The author suggests that these substances should be used for making fluorescent screens, as they possess the advantage that they can be optically worked with such success; the images are sharp, although not so brilliant as those obtained by the ordinary method. The author suggests that the fluorescence of glass may explain the fact that people who are suffering from cataract can see the x rays.

M. A. PEROT and CH. FABRY—AN ABSOLUTE ELECTROMETER FOR MEASURING SMALL DIFFERENCES OF POTENTIAL.

(*Comptes Rendus, Vol.* 124, *No.* 4, *p.* 179.)

The forces acting in an electrometer can be increased either by increasing the surface of the attracting parts, by diminishing their distance apart, or by employing a high auxiliary potential. In this electrometer the discs consist of silvered glass. Their distance apart (about 1-10th mm.) can be specially measured, and their parallelism is adjusted by the interference method.

The possibility of making an exact measurement of the distance simplifies matters, and makes the instrument virtually idiostatic.

The lower attracting surface is fixed, and consists of a glass cylinder about 1 cm. high, the upper end of which presents a plane surface.

The diameter of this cylinder is 5·9504 cm. at 20°, and was measured by an optical method. This cylinder is supported by a convenient system of adjusting screws.

The upper attracted surface consists of a circular glass plate 22 mm. thick and 7 cm. diameter, acting as an indefinite surface. The two attracted surfaces are weakly silvered. If a beam of monochromatic light is directed normally to these surfaces, a system of fringes will be observed, and by the aid of which the discs can be adjusted parallel to one another. Their distance apart can at any time be determined by the method previously described by the author, which consists in a comparison with a thin prismatic layer of air. The upper disc is supported by three springs, so that the attraction of the two discs is represented by a decrease in their distance apart The movements are aperiodic and very slow, on account of the viscosity of the thin layer of air separating them. The attraction is calculated as though the density were normal over the whole surface; to the surface of the disc must, however, be added the surface of a strip of which the length is $\frac{2e}{u}$, e being the distance between the discs.

The electric attraction is measured, as in Lord Kelvin's electrometer, by substituting this force for the weight of a known mass. A pneumatic device is employed for manipulating these weights without shaking the apparatus.

Measurements were made of the electrostatic value of the electro-motive force of the Clark call. This amounts to $2{\cdot}9989 \times 10^{10}$ volts, assuming the electro-magnetic value to be $1{\cdot}4535 \times 10^{8}$, as obtained by M. Lind. The weights employed have, however, to be finally standardised.

A. MOUTIER—THE BUDA-PESTH CENTRAL STATION.

(*L'Éclairage Électrique, Vol.* 10, *No.* 3, *p.* 103.)

The electric lighting of Buda-Pesth is carried out on the Ganz system. The single-phase alternating current is transmitted at 3,000 volts, and is transformed down to 100 or 50 volts for distribution purposes.

The central station occupies an area of 10,800 square metres. Ten boilers are installed, having a heating surface of 2,500 square metres.

The machine room is 13·4 metres wide and 10·02 metres high, and at present contains seven sets, the first of which has a normal output of 300 H.P., and the six others of 600 H.P. Each set consists of a horizontal compound engine driving an alternator and its corresponding exciter. The alternators have rotating magnets and stationary armatures, the latter having 30 or 40 coils according to their size. The alternators are excited at 180 volts. Both the alternators and exciters are run in parallel, and the exciting circuits are controlled by Blathy automatic regulators. Instructions are given by the switch-board attendant to the engine-drivers by means of a special semaphore, consisting of a number of red and green glass panels arranged in two rows, each row bearing the numbers of the alternators. Behind these panels are fixed incandescent lamps, which are switched on or off for giving the signals. One colour is shown for running faster, and the other for running slower, and both for stopping or starting. The station has a very complete testing room and a photometer room. Three main cables leave the station and feed into an underground network in the town. The length of mains already laid amounts to 90·43 kilometres. The distance of the farthest consumer from the station is 7,200 metres. The concentric cables are double lead-covered and armoured, and are laid in brick-lined troughs.

The consumers are connected to the mains by means of a double-pole plug device, generally placed inside the transformers. The latter are either installed in the houses or in the streets.

The transformers and cables are protected against undue rises of potential by means of a sparking device placed near the transformer. This device consists of two parallel brass cylinders adjusted at some distance apart, and across which the spark will take place. One of these cylinders is connected to the inner cable, and the other to earth.

The energy supplied to consumers is measured by Blathy meters.

At the end of 1895, 544 transformer stations were installed, to which 1,237 consumers were connected; the output amounting to 3,192 kilowatts for incandescent lamps, and 2,232 arc lamps, and 60 electro-motors.

N. POPOW—ON THE DEPOSIT OF CARBON PARTICLES ON THE INTERIOR OF INCANDESCENT LAMP BULBS.

(L'Éclairage Électrique, Vol. 10, No. 4, p. 151.)

The deposit of carbon dust on the bulbs of incandescent lamps, and its influence on the light emitted, was studied in 1891 by Edward Nicholson, who then discovered that the particles of carbon were distributed over the surface of the glass in an almost uniform manner. The author's attention was drawn to some disused lamps which had a far from uniform deposit of carbon, the nature and distribution of the deposit depending on the shape of the filament. In cases where the filament was in the form of a helix, the corresponding pattern was clearly shown on the glass. The author agrees with Professor Antoni that the deposit of carbon on the glass is not only due to the vaporisation of carbon provoked by the high temperature

of the filament, but also due to electrical forces carrying the electrically charged particles of carbon towards the surface of the bulb; such forces being established when there are differences of potential between the filament on the globe, produced either by electrostatic phenomena, or by the fortuitous connection of the bulb to earth. The dark bands of carbon on the glass would correspond to the minimum distance between the surface of the filament and the internal surface of the bulb.

ANON.—THE ELECTRIC TRACTION OF BOATS ON THE BURGUNDY CANAL.

(*L'Éclairage Électrique, Vol.* 10, *No.* 5, *January,* 1887, *p.* 212.)

In his work on "La Traction Electrique," M. Paul Dupuy gives particulars with regard to the above system. The power is obtained by means of turbines placed at one of the weirs and driven by the canal water.

The turbine runs at 180 revolutions per minute, and drives a dynamo which at a speed of 600 revolutions per minute gives an electro-motive force of 300 volts.

An overhead conductor of 0·5 cm. diameter conveys the current to the propelling apparatus, which may be of two kinds—either a propelling gear fixed on the boat, or a hauling truck which moves along the towing path. The former weighs about 900 kilogrammes, and is of such a shape that it can be adapted to any boat, and two men are sufficient to fix and work it. The hauling truck runs on three wheels; the two back wheels are driven by an electro-motor. Its total weight is 2 tonnes, and it draws the boats in the same manner as does a horse. The first official trials were carried out in December, 1895, on a boat of 186 tonnes, 38·5 metres long, and 5·05 metres wide.

The maximum speed was 2,150 metres per hour with the propelling gear, and with a larger propeller this was increased to 3,000 metres. With the hauling gear drawing the boat in the opposite direction the speed was 2,480 metres per hour, in this case the wind being in opposition.

A smaller boat of 75 tonnes attained a speed of 3,350 metres per hour during the same tests with the propelling gear and a favourable wind. A speed of 4,240 metres was obtained with the larger propeller and calm weather. The same boat in calm weather attained a speed of 4,240 metres per hour with the hauling gear.

Other trials were carried out in 1896 before a Commission sent by the Belgian Government.

The hauling gear was found to work quite steadily notwithstanding the lateral pull of the towing rope, and without appreciably damaging the towing path.

The experimental results show that the hauling method is more efficient than the propelling method. The report of this Commission sums up the advantages and disadvantages of both systems.

The experiments show further that in a canal having a depth of 2 metres of water 1 kilowatt-hour is required per 100 tonnes, for a mean speed of 2·5 kilometres per hour, and the cost of working comes to the low figure of 0·00016 francs per kilometre-tonne.

H. MOISSAN—ON THE PREPARATION OF MANGANESE IN THE ELECTRIC FURNACE.

(*L'Éclairage Électrique, Vol.* 10, *No.* 5, *p.* 215.)

By the use of the electric furnace the long and difficult reduction of manganese oxide becomes an easy process. The pure protoxide of manganese is mixed with carbon and heated in the arc. With 300 amperes at 60 volts the reduction is complete in five or six minutes, and there remains at the bottom of the crucible 100 to 200 gr. of carbide of manganese. The reduction takes somewhat longer with a 100-ampere and 50-volt arc: it requires from 10 to 15 minutes.

When working with an excess of carbon, the manganese becomes saturated with carbon, and castings of the following composition are obtained:—

	1.		2.		3.		4.
Manganese	85·0	...	85·82	...	90·6	...	94·06
Carbon	14·59	...	13·98	...	10·20	...	6·35

If the reduction takes place in the presence of an excess of oxide, the quantity of carbon diminishes considerably, and in some cases only 4 to 5 per cent. of carbon is obtained. When the manganese casting thus prepared contains but little carbon, the metal may easily be kept in open vessels; but when the quantity of carbon increases, the dampness of the air decomposes it, and this decomposition is the greater as the amount of carbon approaches the composition of carbide of manganese, $C\ Mn^3$, discovered by MM. Troost and Hauteville. When it is desired to prepare a fairly large quantity of manganese by means of a more powerful arc, the efficiency of the process is diminished owing to the easy volatilisation of manganese. In order to obviate this, the mixture is heated in a covered carbon crucible. By this method metallic ingots have been obtained containing about 95·5 per cent. of manganese. By starting with natural bioxide of manganese as pure as possible, previously calcined, and mixed with carbon, and when working with currents of 500 amperes and 50 volts in a closed crucible, a casting having the following composition was obtained:—

			1.		2.
Manganese	...	...	89·78	...	91·13
Total carbon	...	...	7·59	...	6·41
Dross	...	...	2·06	...	1·78

The output amounted to 96 per cent. of the oxide treated. The author considers that this reaction is easily applicable to the industrial preparation of metallic manganese; and, as the refining of this metal takes place easily in the presence of an excess of oxide, manganese could therefore be obtained free from carbon and silicon.

H. MOISSAN—ON THE PREPARATION OF SILICON IN THE ELECTRIC FURNACE.

(*L'Éclairage Électrique, Vol.* 10, *No.* 5, *p.* 216.)

It has previously been shown that silica can be volatilised in the electric furnace with a current of 1,000 amperes at 50 volts, with an abundant formation of silica

vapour. Light filaments of silica are formed, which remain suspended in the atmosphere for some time. When the volatilisation of the silica is not complete the lower part of the crucible sometimes contains characteristic crystals of silicon such as those described by Senarmont. The first experiment shows that silica can be reduced by carbon at high temperatures. By heating in the electric furnace a mixture of rock crystal and powdered carbon placed in a carbon cylinder closed at one end, this phenomenon is much more marked. The orifice of the tube is lined with flocculent silica, underneath which are found sharply defined and almost colourless crystals of silicide, and a little lower a complete ring of black crystals mixed with molten globules. These crystals are only attacked by a mixture of nitric and fluoric acid; they ignite at ordinary temperatures in fluorine, and burn vigorously, with the formation of fluoride of silicon. The crystals are always mixed with silicide of carbon, but the crystalline dust which is gathered contains 28 to 30 per cent. of crystalline silicon. This experiment, which was repeated several times, shows that under the action of the electric arc, silica is reduced by carbon and produces silicon. When the temperature is not too high, a portion of the silicon escapes the action of the carbon, and may be found again in the form of crystals or molten globules. By cooling the vapour of silicon at the moment of its formation this process might be applied to the preparation of silicon.

— LE ROUX—ON THE EQUATION OF TELEGRAPHISTS.

(*Comptes Rendus*, *Vol.* 124, *p.* 148, *January*, 1897.)

Much work has been done in connection with the determination of an integral of the equation,

$$\frac{d^2 u}{d t^2} = \frac{d^2 u}{d r^2} - u,$$

reducing itself to a constant $f(o)$ for $r = t$, and to a given function $f(t)$ for $r = t$.

This question resolves itself into the following rather more general problem:— To determine an integral of the equation,

$$\frac{\delta^2 z}{\delta x \, . \, \delta y} + z = 0,$$

reducing itself for $y = o$ to $f(x)$, and for $y = x$ to $\phi(x)$. This is the problem of which M. le Roux gives the solution by considering the principal integrals of the equation, having partial derivatives of the second order.

T. H. MOUREAUX—ON THE ABSOLUTE VALUE OF THE MAGNETIC ELEMENTS UP TO JANUARY 1st, 1897.

(*Comptes Rendus*, *Vol.* 124, *January*, 1897, *p.* 177.)

The magnetic observations in the Parc St. Maur were continued under the

same conditions as in preceding years. The following are the absolute values of hourly observations taken during a whole year:—

	Absolute Values to 1st Jan., 1897.		Secular Variations in 1896.
Declination	15° 1 5′	...	− 5·3′
Inclination	65° 0·8′	...	− 1·6′
Horizontal component ...	0·19693	...	+ 0·00017
Vertical component ...	0·42256	...	− 0·00016
Total force	0·46619	...	− 0·00008

The secular variations result from the comparison between the present values and those of 1st January, 1896.

The observatory of the Parc St. Maur is situated 0° 2′ 23″ longitude east, and 48° 48′ 34″ latitude north.

The magnetic observations are carried out at Peripignan in the same manner and with the same instruments as at the Parc St. Maur. The following are the absolute values for the same period as the above:—

	Absolute Value to 1st Jan., 1897.		Secular Variations in 1896.
Declination	13° 53·3′	...	− 4·0′
Inclination	60° 5·2′	...	+ 1·7′
Horizontal component ...	0·22416	...	+ 0·00024
Vertical component ...	0·38962	...	+ 0·00016
Total force	0·44952	...	+ 0·00030

The Perpignan Observatory is situated 0° 32′ 45″ longitude east, and 42° 42′ 8″ latitude north.

At the Nice Observatory the methods and instruments employed are identical to the above. The following are the absolute values obtained during the same period:—

	Absolute Values to 1st Jan., 1897.		Secular Variations in 1896.
Declination	12° 15·4′	...	− 4·8′
Inclination	60° 16·5′	...	− 2·8′
Horizontal component ...	0·22304	...	+ 0·00036
Vertical component ...	0·39064	...	+ 0·00011
Total force	0·44982	...	+ 0·00008

The Nice Observatory is situated 4° 57′ 48″ longitude east, and 43° 43′ 17″ latitude north.

F. GUILBERT—THE NEW GENERATING PLANT OF THE "SOCIÉTÉ "D'ÉCLAIRAGE ET DE FORCE."

(*L'Éclairage Électrique*, *Vol.* 10, *No.* 2, *p.* 55.)

One of the systems of distribution adopted by Messrs. Hutin & Leblanc consists in producing high-tension alternating currents either directly at the generating station or by step-up transformers; and at the distributing points these currents may be transformed into multiphase currents, and rectified by means of a special collector worked from a synchronous motor.

The Hutin & Leblanc system has been applied to several stations in France, the chief one being the generating station of St. Ouen, of the "Société Anonyme "d'Eclairage et de Force." This station contains two fly-wheel alternators of the Hutin & Leblanc type (as described in the last number of the *Journal*). Their output is 250 kilowatts; the alternators are wound two-phase, each phase giving 88 volts, 1,420 amperes, at a frequency of 39 and 65 revolutions per minute.

The diameter of the rotating magnet is 5·35 metres, and its width 40 cm.

The armature is built up of 36 segments, there being 12 slots per segment, each slot containing a conductor of 710 sq. mm. section. The windings are arranged in four circuits, each having a resistance of 0·0016 ohm.

The outside diameter of the armature is 6·25 metres, and the air gap 8 mm.

The smallness of this machine is an indication of the limit of size permissible with fly-wheel alternators. The voltage of the alternator is raised from 88 volts to 6,000 volts by means of a 250-kilowatt Farcot transformer consisting of three cores, the outside ones carrying the windings corresponding to the two phases, and the centre one acting as a common core and having a section 1·41 times that of the two others. The transformer is surrounded by a wire grating. The low-tension windings are on the inner cylinders. The high-tension windings, carried on the outer cylinders. consist of six coils per phase, each coil being insulated with micanite cloth. then taped, and finally covered with thick cord, to act as a protection and as a separation between the coils. The six coils are further insulated from one another by means of insulating plates. The total height of the transformer is only 1·77 metres, and the width 1·75 metres. These transformers were tested for several hours at a pressure of 10,000 volts.

The coefficients of leakage were measured by Mr. Kapp's method, and found to be 10 per cent. with a current of 1,420 amperes per circuit; this being a low figure considering the small height of the transformer. The total drop at full load was 1½ per cent. The same tests were carried out on the above alternators as on those supplied to the Secteur des Champs Elysées.

The efficiencies of the alternators and transformers were obtained by measuring the losses separately. One of the alternators had the edges of its magnet poles very close together for experimental purposes.

Its efficiency was—

Hysteresis and Foucault currents,	6,850	watts.
$C^2 R = 2 \times 0{\cdot}0016 \times 1410^2 =$	6,500	,,
Field $= 1{\cdot}65 \times 65^2$ ...	7,000	,,
Total ...	20,350 = 92½%.	

The alternator with poles of a normal width had an exciting loss of only 2,150 watts, and a full-load efficiency of 94·2 per cent.

The transformer losses were as follows:—

Hysteresis and Foucault	5,900	watts.
Low-tension winding	1,420	,,
High-tension winding	1,560	,,
	8,880	,,

Efficiency, 96·7 per cent.

M. A. CHASSY—ON AN ELECTRO-CAPILLARY EXPERIMENT.

(Journal de Physique, Vol. 6, January, 1897, p. 14.)

The experiment consists in taking a vessel containing mercury and acidulated water; the mercury acting as negative electrode, and the platinum wire as positive electrode. A glass tube is placed at some distance from the positive electrode, and so dips into the water that its end is slightly immersed in the mercury. When a current is established, water is seen to rise inside the tube; this varies according to the circumstances, and may, for example, reach 15 cm. As the mercury does not wet the glass there exists an ultra-capillary space between the lower part of the tube and the surface of the mercury, and it is through this space that the liquid is filtered from the vessel to within the tube. The action stops when the head of water in the tube is sufficient to force the mercury out from the bottom. The above action may be made to continue indefinitely by syphoning the water out of the tube at the same rate as it enters it.

The rate at which the liquid enters the tube depends on the conditions of the experiment, but in too irregular a manner, owing to the liberation of hydrogen bubbles, to allow of an exact law being established.

With a weak current, although strong enough to promote electrolysis, the above phenomenon of electric filtration is not produced. When the current increases the phenomenon increases, which at last attains a limited value. The rate of filtration increases with the resistance of the liquid; and it is found to be proportional to the perimeter of the tube.

As an example of the magnitude of the phenomenon, in one of the author's experiments in which the action was the strongest, 700 cubic centimetres of liquid were obtained per hour with a tube 4 cm. in diameter. The passage of a current producing appreciable electrolysis is necessary; the mere polarisation of the mercury is not sufficient. M. Lippmann has shown (*Ann. de Chim. et de Phys.*, 5th series, vol. v., p. 545, 1875) that a tangential force is produced when the surface tension of the mercury varies from one point to another. The author attributes the above phenomenon to this cause.

Instead of immersing the tube in the mercury, it may be so raised that its lower end is slightly above the surface of the mercury. In this case the rise in the surface of the liquid is almost imperceptible. If, with this position of the tube, an anode be placed within it, the level of the liquid is seen to lower, the value of which is greater than the preceding rise. This result is due to the difference in the dimensions of the tube and the vessel. In this last experiment, if the mercury be replaced by a solid metal, no variation is noticed in the level of the liquid.

EMILE FONTAINE—THE INFLUENCE OF THE ELECTRICAL CONDITION OF A LIQUID SURFACE ON THE HEAT OF VAPORISATION OF THAT LIQUID.

(Journal de Physique, 3rd Series, Vol. 6, January, 1897, p. 16.)

In a recent article (*Jour. de Phys.*, 3rd series, vol. v., 1896, p. 159), M. Houllevigne showed that the heat of vaporisation of a liquid depends on the

shape of its surface. The author shows also that this value is a function of the electrical condition of the surface. The liquid is contained in a large vessel into which dips a tube of sufficiently large diameter to allow of the surface of the liquid in the tube being examined. A horizontal insulated disc is mounted in this tube above the surface of the liquid. When this disc is electrified it is possible to establish a difference between the levels in the tube and in the vessel.

M. Blondlot has shown (*Jour. de Phys.*, 3rd series, vol. iii., 1884, p. 442) that in the neighbourhood of the electrified surface, A_1, in the tube, the maximum pressure of the vapour of a liquid was, at the same temperature, lower than in the neighbourhood of the non-electrified surface, A, in the vessel, and that the alteration in the difference of level, h, between A_1 and A was connected with the electrical surface density, $\mathfrak{m}$, at A_1 by the relation,

$$h = \frac{2\pi \mathfrak{m}^2}{\Delta - \delta};$$

Δ and δ standing respectively for the specific weights of the liquid and its saturating vapour in the neighbourhood of a flat non-electrified surface.

The author further assumes that the liquid follows the following closed isothermic cycle:—

1. Vaporisation of a very small weight, $d\,w$, of the liquid at the surface A_1.
2. Compression of the vapour thus formed in order to restore its pressure from the value, F′, which it had at A′ to the value, F, which corresponds to the surface A, where the electrical density is supposed to be *nil*.
3. Transference of the weight of liquid $d\,w$ thus compressed from A′ to A.
4. Condensation of this vapour at A.

If it be admitted, according to Blake, that the liquid molecules, when leaving the electrified surface by evaporation, do not carry away any portion of the charge of the latter, there will be no necessity to take into account the work done by the electrical forces. Consequently, the quantities of heat brought into play in the four preceding operations will have the same values as those obtained by M. Houllevigne. If L and L_1 stand respectively for the heats of vaporisation at A and A′, and E be the mechanical equivalent of heat, and if h be substituted for its value in function of the surface density, then

$$L' = L + \frac{2\pi \mathfrak{m}^2}{E(\Delta - \delta)},$$

which equation leads to the following conclusion:—

When a liquid surface is electrified, the heat of vaporisation, L′, of that liquid is greater than it would be if its surface were not electrified, by the amount $\frac{2\pi \mathfrak{m}^2}{E(\Delta - \delta)}$, the temperature being assumed equal in the two cases

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L. N. VANDEVYVER—THE LAW OF PHOTOGRAPHIC ACTION OF THE "x" RAYS.

(*Journal de Physique, 3rd Series, Vol.* 6, *January*, 1897, *p.* 23.

In a recent communication by the author (*Bull. de l'Academie de Belgique*, 3rd series, vol. xxxii., Nos. 9 and 10, Sept.-Oct., 1896), he showed that the time of

exposure required to obtain a good radiograph of a portion of the human body is given by the following rule:—

"Knowing the minimum time of exposure, t, necessary to obtain a sharp "image of an object, A, of thickness E, the time of exposure necessary for an "object, B, of thickness E′, is given by the formula, $t' = t\left(\frac{E'}{E}\right)^{2}$."

The object of a further experiment was to ascertain whether the action of the x rays on the emulsion of a photographic plate varies according to the inverse square law, as in the case of an illuminated surface.

A Watson tube was employed, placed at increasing distances of 5, 10, 15, 20, 25, 30, 35 centimetres from the plate; all the impressions being made on one plate, in order that they should be comparable.

After having applied the law of the square of the distances to the time of exposure, the author found that this law did not hold good. In this particular case the exposures were far too long, the errors between one exposure and the next being very great.

After several trials, the author has arrived at the conclusion that the action of the x rays varies inversely as the distance of the tube from the photographic plate. That is to say, for a series of distances one obtains with proportional exposures a series of images which, when developed together, appear at the same time and with the same density; the chemical action of the rays having been equal for the different images.

M. BOISSEAU DU ROCHER—ON RAPIDLY INTERMITTENT CURRENTS: PHYSICAL, PHYSIOLOGICAL, AND THERAPEUTIC EFFECTS.

(*Comptes Rendus, Vol.* 124, *No.* 4, *January*, 1897, *p.* 185.)

The generator described by the author produces currents which do not alter in polarity; and, in order to differentiate them from ordinary alternating currents, he calls them "rapidly intermittent currents." These currents are obtained by modifying the static machine which is used in electro-therapeutic work. The first modification consists in placing special, slow-charge and discharge condensers in the electric field of the machine and connecting them to earth. These condensers consist of an exhausted sphere of crystal glass, in which a central metallic stem serves as pole, this being connected to earth, the surrounding air acting as a second armature. Some of the condensers contain a metallic spiral which can be used for increasing or regulating the output of the machine.

The author has found, moreover, that the power of the machine could be increased by the use of a metallic spiral; for under these conditions self-induction phenomena are produced, as pointed out by M. d'Arsonval.

The interruptor consists of an insulating sleeve, at the end of which is fixed a metallic rod carrying exciters in the form of balls, points, &c. On the metal rod is fixed a metal sphere of varying size to suit the conditions. On the insulating sleeve slides a cursor carrying a metal ball of small diameter. It is between these

balls that the sparks take place which produce the interruptions and which regulate the intermittences—the latter by the adjustment of the distance between the two balls.

With long sparks there are 1,200 interruptions per minute. When the sparks diminish in length the number of interruptions increase, and become 7,000.

These rapidly intermittent currents, produced without a change of polarity, are of considerable interest from a therapeutic point of view.

Physical Effects.—These currents decompose solid bodies, volatilise liquids, and decompose gases.

Under certain unknown conditions the following phenomena have been noticed to take place. A ball of fire, about the size of a lentil, has been seen to travel along the wire connecting the generator to the seat; this slowly descends the conductor and disappears at the seat. The phenomenon appears to be analagous to globular lightning. At times there is produced between the conductors and the disc of the machine a sharp glow comparable to summer lightning.

Pysiological Effects.—These currents deeply penetrate organisms, and are found to have marked physiological effects.

Therapeutic Effects.—The author has found a number of useful applications for these currents. They have proved of use in some cases of paralysis and nervous diseases.

They are used also for producing gaseous preparations from liquids.

The high intermittences have different appearances and properties, according to whether points or balls are used, and according to the number of intermittences

They can always be reproduced with their true characteristics, and are easy to record on photographic plates.

SELLA and MAJORANA—EXPERIMENTS ON THE RÖNTGEN RAYS, AND THE MEASUREMENT OF THE LOWER LIMIT OF THEIR VELOCITY.

(*L'Éclairage Électrique, Vol.* 10, *No.* 1, *p.* 35.)

The authors have attempted to demonstrate the deviation of the *x* rays in a vacuum, by means of a magnetic field. These rays pass through a tube 50 cm. long, closed by two aluminium discs, and within which the vacuum corresponded to 0·0005 mm. of mercury. The illumination of a fluorescent screen, which received these rays, and others which had not passed through the tube, appeared to remain stationary when an electro-magnet was excited. In a second experiment, a spark micrometer, consisting of two iron balls 1 cm. diameter, was connected between the two wires of a Ruhmkorff coil. The distance from the region of emission of the rays to the micrometer exceeds 100 cm. It is found that when only transparent bodies are placed in the path of the *x* rays the explosive distance falls to 4 cm. On the other hand, it reaches 7 cm. when the rays are intercepted. The effect is produced when the primary circuit is only closed once by hand; it is not then due to a series of discharges. Care must be taken to sufficiently remove the Crookes tube, in order to obviate disturbances in the distribution of the discharge due to variations of the magnetic field.

This method suggests a means of assigning a lower limit to the rate of propagation of the Röntgen rays. It shows that these rays have reached the micrometer, before the electric wave emitted by the poles of the coil, has had the time to produce the difference of potential necessary for the discharge of the micrometer. This time is, then, longer than that which is necessary for the propagation of the same wave as far as the Crookes tube, for the production of the cathode rays, and than that of the Röntgen rays which finally cause the propagation of these waves as far as the micrometer.

The authors, in a subsequent memoir (given below), have studied in detail the influence of the x rays on the production of the spark.

SELLA and MAJORANA—ACTION OF THE RÖNTGEN RAYS AND ULTRA-VIOLET LIGHT ON THE EXPLOSIVE DISCHARGE IN AIR.

(*L'Éclairage Électrique, Vol.* 10, *No.* 1, *p.* 36.)

Starting from the fact observed in the preceding memoir, the authors worked with the object of demonstrating the action of the x rays on the discharge current itself which produces these rays. Their investigations have led them to compare the action which they had observed, to that exercised by ultra-violet light on the discharge, and which, according to the Hertz experiments, would be in the opposite direction (*Wied. Ann.*, vol. xxxi., p. 983, and *La Lum. El.*, vol. xxv., p. 584).

It is necessary, in order to succeed with these experiments, to earth the cathode of the Crookes tube.

The interposing of screens enables it to be shown that the seat of the special action in question, is the positive pole, or the air in its immediate vicinity, to the exclusion of the negative pole and of the air which separates the two.

The interposing of an aluminium screen in front of the positive pole demonstrates clearly the part played by the x rays. The experiment succeeds when the discharges of the Crookes tube and of the micrometer are made independent to one another. The phenomenon always succeeds when in the primary circuit are placed large resistances, or self-inductions connected between poles of the same name of the tube and the micrometer. The region in which the discharge takes place is in a condition of considerable sensitiveness; the spark takes place when a piece of glass or cardboard, shaped to a point, is brought by hand to within a few centimetres of it. It is necessary to remove any foreign bodies away from the exciter, and to keep the balls well polished.

Except in one case where MM. Elster and Geitel (*Wied. Ann.*, vol. xxxix., p. 232, 1890) had observed that ultra-violet light may cause an obstacle to the discharge from an electrostatic machine, their experiments confirmed those of MM. Wiedemann and Ebert, and of Hertz, showing that the explosive distance increases when the negative pole is illuminated. This is exactly opposite to the action of the x rays observed in the preceding memoir. When illuminating the spark micrometer by an arc, the authors found, to their great surprise, that the action was in the same direction as that of the Röntgen rays. The action of the arc was always on the positive pole.

By repeating the Hertz experiment under the original conditions, with an explosive distance of a few millimetres, between fairly large balls, it is found that the Crookes tube acts in the same way as the active Hertz spark.

It was also found that the Hertz phenomenon can be reversed when the length of the spark is varied; the neutral explosive distance depends, besides, on the diameter of the balls, with which it increases. When the discharge is produced between points the Hertz phenomenon is never observed.

MM. Wiedemann and Ebert have shown that ultra-violet light not only has the effect of provoking a discharge, but also of modifying its nature, as can be proved by causing it to pass in a telephone, or in a Geissler tube, or by studying the form and the sound of the spark. The same experiments have yielded analogous results in the authors' hands, which are stated in a new memoir given below.

SELLA and MAJORANA—ON THE ACTION OF THE RÖNTGEN RAYS ON THE NATURE OF THE EXPLOSIVE DISCHARGE IN AIR.

(*L'Éclairage Électrique, Vol.* 10, *No.* 1, *p.* 37.)

In this memoir the authors study, under the respective names of the first and the second phenomenon, the production of the discharge, and all the alterations which it undergoes in the two cases where the radiations (ultra-violet and Röntgen) help it or retard it. The first action was discovered by Hertz; the second, which was observed by MM. Elster and Geitel in the case of the ultra-violet radiations, was discovered in the case of the x rays by the authors of the present memoir (see above). To study the first phenomenon, the x rays are made to act on an exciter, X, connected as a shunt to the electrodes of a Crookes tube. and beyond this tube with regard to the coil. The balls of this exciter are 52 mm. diameter; the action of the x rays lowers the explosive distance from 100 mm. to 25 mm. The second phenomenon is obtained by substituting for the negative ball, a strip of aluminium normal to the mean direction of the rays which have to pass through it in order to reach the positive ball.

If in the circuit between the two positive tubes be connected the primary of a transformer consisting of several spirals of a wire 3·5 mm. diameter, surrounded by an ebonite tube on which are wound 200 turns of a wire 0·5 mm. diameter, the secondary being closed on an exciter fitted with micrometer screw, it is then found that the secondary spark, which is protected against the radiations, always disappears when in the first or second arrangement the discharge at M is favoured; this taking place according to whether the x rays are allowed to pass or are intercepted.

The results are the same, but stronger, when a condenser is connected to the terminals of a coil. Instead of the primary of the transformer, a copper wire of 5 mm. diameter and 1·5 m. long can be connected as a shunt to the spark micrometer. The increase of impedence can also be observed by means of a small electric lamp.

Lastly, if a point in the circuit between the two positive poles be connected to one of the armatures of a Leyden jar, it is possible to take longer sparks from the second armature, in the first case when the x rays act, and in the second case when they are intercepted.

The same results have been obtained by substituting ultra-violet radiations to the x rays.

The facts appear to agree with Hertz's observations, according to which ultra-violet light cutting the trajectory of a spark appears to diminish in its oscillatory activity. The recent experiments of Messrs. Elster and Geitel also agree with this general view.

W. STROUD and J. B. HENDERSON—A SATISFACTORY METHOD OF MEASURING ELECTROLYTIC CONDUCTIVITY BY MEANS OF CONTINUOUS CURRENTS.

(*Philosophical Magazine*, *Vol.* 43, *No.* 260, *p.* 19.)

The authors have devised a satisfactory method of measuring electrolytic conductivity by means of continuous currents, so as to enable a galvanometer to be employed as the indicating apparatus instead of the telephone, which is an integral part of the Kohlrausch and similar alternating-current methods.

The fundamental feature of the new method is to obviate the detrimental effects of polarisation in the electrolytic cell by inserting a second cell, with the same size of plates, &c., but with a very different length of electrolytic conductor, in the corresponding arm of a Wheatstone bridge circuit; high voltages and high resistances being employed so as to effectually drown any residual error arising from differential polarisation.

The following was found to be the best arrangement of the bridge circuits, viz.:—Considering the arms of the bridge as r_1, r_2, r_3, r_4; r_1 and r_2 are connected together and to one pole of the battery, whilst r_3 and r_4 are connected together and to the other pole of the battery; the opposite terminals of the galvanometer being connected to the points of junction, r_2, r_4, and r_1, r_3, respectively. Using the above notation, r_1 and r_2 are equal resistances; r_3 consists of the compensating electrolytic cell and a resistance box, R, in series with it; whilst r_4 is the electrolytic cell proper, which is similar to the compensating cell, except that it is of higher resistance, this being obtained by employing a longer column of liquid between the electrodes.

The resistance box, R, is adjusted till there is a balance, when the resistance in the box is equal to the difference between the resistances of the two cells. Since r_1 and r_2 are equal, when approximate balance is obtained, equal currents will be traversing both electrolytic cells, and therefore there should be the same polarisation in each cell; and since these polarisations are clearly opposed, they should, in theory, be eliminated. It was found, in practice, that at least 99 per cent. of the polarisation may be balanced in this manner, the residual differential polarisation being then drowned by working with high resistances and high voltages: for example, $r_1 = r_2 = 1{,}000\ \omega$; difference between the resistances of the two cells $= 20{,}000\ \omega$; resistance of galvanometer $= 300\ \omega$; voltage $= 30$.

The electrolytic cells each consisted of two small, thick-walled test-tubes with necks half-way up their sides (the authors, however, now consider it more advantageous to make these connections near the bottom, so as to facilitate mixing), and into these necks are fitted the well-ground ends of a tube of nearly uniform bore, the diameter of the bore being chosen so as to give a convenient resistance with the electrolyte used. The tubes were calibrated by measuring their lengths and weighing the amount of mercury required to fill them. The lengths of the tubes were about 30 cm. for the main cell, and 5 cm. for the compensating cell. The electrodes were cylindrical pieces of platinum foil connected to the bridge through mercury cups The whole apparatus was placed in an oil bath to equalise the temperature. In two experiments on K Cl solutions of a strength of $\frac{1}{2}$ molecular equivalent per litre (taking K = 39·13, and Cl = 35·45), the following results were obtained for $\frac{k}{\mu}$ (Kohlrausch's notation), viz.:—$1{,}012{\cdot}2 \times 10^{-8}$ and $1{,}008{\cdot}3 \times 10^{-8}$; Kohlrausch's latest result being 1,009, Bouty's 1,035, and Krannhal's 1,003.

The authors also found that the use of a compensating cell was advantageous with alternating-current methods for resistances not greater than about 1,000 ω, but was no avail for higher resistances. They were not, however, able to obtain such a high degree of accuracy in this way as was possible with the continuous-current method described above.

C. MARGOT—ELECTRO-COPPERING OF ALUMINIUM.

(*Beiblätter*, *Vol.* 21, *No.* 1, *p.* 44.)

The author obtains a coherent deposit of copper on aluminium by the following process, viz.:—The aluminium is first treated with the carbonate of an alkali to make the surface porous, and then thoroughly washed in running water and immersed in warm hydrochloric acid (about $\frac{1}{10}$). After a slight rinsing in clean water, the aluminium is brought into a dilute, slightly acid solution of sulphate of copper, and, after again washing, into the galvanic bath.

L. ZEHNDER—ON THE MANAGEMENT OF HIGH-VOLTAGE ACCUMULATORS

(*Wiedemann's Annalen*, *Vol.* 60, *No.* 1, *p.* 47.)

The author gives particulars of "forming" lead accumulator plates.

In order to ensure high insulation, after "forming," the lead bridges or connecting pieces are warmed to about 120° by a Bunsen burner, so as to drive off all water, when they are rubbed while hot with vaseline, which rapidly spreads so as to cover the whole of the unformed surface. The terminal lead strips to which the copper conductors are connected must also be covered with vaseline in a similar manner, otherwise acid passes over into the mercury cups, and the copper wires dipping therein become amalgamated over their whole surface and draw up the mercury. These wires are only dipped into the mercury to such a depth that they will be amalgamated; the remaining part is then heated until all the mercury on the free surface of the wire is vaporised and the copper oxidised. The copper wire is then rubbed with vaseline

After greasing, the lead plates are then placed in the containing vessels, and the latter filled up to about 2 cm. from the edge with pure dilute sulphuric acid of 19°–20° Baumé, and a layer of paraffin oil 3–5 mm. deep poured on to the top of the acid. This layer of oil does not mix with the acid, causes the several cells to work very uniformly, enables the voltage to be maintained for a long time, and prevents evaporation and spraying of the acid.

In order to guard against spilling the acid when filling the cells by overfilling them, a special arrangement is employed. This consists of a raised reservoir from which the electrolyte is supplied to a glass delivery nozzle connected thereto through a flexible pipe which can be nipped when desired to stop the flow of the liquid. By the side of the glass nozzle is secured a second piece of glass tubing having its opening at the same level as the opening of the delivery nozzle; this second tube is in communication through a flask with an air-pump, so that when the acid rises, in the cell being filled, to the level of the nozzle, the excess is immediately removed by the suction pipe, which makes a gurgling noise and warns the operator that the supply should be stopped. By this device the cells can be rapidly and conveniently filled to the required height.

In working with the cells, the acid should be removed once a year, using the suction pipe above mentioned, this being used to simultaneously draw out the greater part of the deposit at the same time; the cells should be immediately refilled with fresh acid. The author recommends that the cells be always left in parallel when not in use, so that no single group can fall to a lower P.D. than those in parallel therewith, and the formation of sulphate remains substantially the same in all the cells. The cells are recharged, when the whole battery has reached the predetermined minimum voltage (1·8 volts), until gas is evolved from all the cells.

B. WALTER—ON THE REGULATION OF THE VACUUM IN TUBES FOR THE GENERATION OF RÖNTGEN RAYS.

(*Elektrotechnishe Zeitschrift*, 1897, *No.* 1, *p.* 10.)

This relates to a device, due to Professor Dorn, for preventing the gradual increase in the vacuum which takes place in these tubes during use. This consists in introducing a trace of caustic alkali into a small pocket in the tube, and, when the amount of residual gas in the tube begins to decrease below the proper amount, heating the alkali from the exterior by means of a Bunsen burner so as to drive out water vapour therefrom until the correct degree of vacuum is attained. This method of heating the alkali is not always possible, and is unsatisfactory, since too much vapour may be generated and the tube be temporarily rendered ineffective.

To obviate these disadvantages the pocket containing the alkali is heated continuously instead of intermittently, this being effected by means of a platinum wire 30–40 cm. long and 0·25 mm. thick, wound round the tubular pocket, which is about 6 mm. thick, and heated by passing a current through it from three accumulators, the resistance of the circuit being made up to 8 ohms. In order to

avoid shocks during regulation and perforation of the tube at the pocket, the Dorn pocket is arranged at the anode end of the tube, and during working the anode and heating coil are both connected to earth.

By using this device a Röntgen tube can be effectively worked continuously for as long a time as desired in a perfectly definite condition.

Herr HUNDHAUSEN — ON MESSRS. SIEMENS & HALSKE'S NEW SYSTEM OF INSTALLATION FITTINGS AND SAFETY DEVICES COMPLYING WITH THE REGULATIONS OF THE VERBAND DEUTSCHER ELEKTROTECHNIKER.

(*Elektrotechnische Zeitschrift*, 1897, *No.* 2, *p.* 27, *et seq.*, *and No.* 3, *p.* 41, *et seq*)

These fittings comprise terminal pieces to be sweated to the ends of cables to facilitate connection with switches, &c., and consisting of stampings which have a lug at one end with a hole in it to pass over a binding screw, &c., and lips at the other end which are bent round to form a socket to receive the end of the cable.

A clamp for making a branch connection from a cable, and which consists of a metal strap having a block between its ends bearing a terminal, is also shown.

The main feature of the system consists in the improved forms of fusible cut-out. One form of these cut-outs is more especially applicable for use with larger currents, and consists of an arrangement whereby the arc, which forms when the fusible strip (or wire) melts, is mechanically broken by a cement bridge-piece which is continuously pressed against the wire or strip by springs. This bridge, when the fuse melts, is no longer supported in its raised position, and passes down into a groove formed in the base of the cut-out, thus effectually interrupting the arc. The strip is made somewhat stouter where the bridge-piece rests upon it, so as to enable it to resist the pressure of the springs.

The other form of cut-out, which is suitable for use with small currents, and takes up very little space, consists of an approximately cylindrical block of fire-resisting, insulating material, having zig-zag passages in it to receive the fuse, so that the length of the fuse will be considerably greater than the distance between the ends of the cylindrical block where the contact surfaces are arranged. The said passages consist of an unequal number of longitudinal holes, having lateral vent-apertures, the said passages being alternately connected in pairs at the end surfaces of the block by grooves or channels. Lead wire is drawn through the passages, and the end surfaces of the block covered by insulating material, so that the lead wire is enclosed in a continuous canal; finally, metal plates are mounted on the ends of the block, and the ends of the lead wire soldered thereto

It was found that very good results were obtained with the wire passing thrice backwards and forwards between the ends, no advantage being gained by increasing the number of zig-zags; in fact, the explosive action, on fusing, was even increased by so doing. In most cases, however, two such wires are arranged in parallel between the contact plates, thus giving six longitudinal passages through the block. The terminals on the base are covered by a plate of

insulating material, so as to protect them from the action of the vaporised metal issuing from the lateral vent-apertures of the block when the fuse goes. The most satisfactory material for making the cylindrical block was found to be cement. The complete fuse, or "cartridge," is simultaneously secured in position, and its upper end connected to its terminal, by means of a metal rod passing up its centre and having a screw-threaded end which receives a nut engaging the upper contact plate of the cartridge. In order to prevent cartridges for heavy currents being placed on cut-out bases in use on circuits using smaller currents, nuts of definite thickness are placed on the central rod—a larger number of nuts for a small-current fuse, and a smaller number for a heavy-current fuse—the central hole in the cylindrical block being enlarged to a corresponding height, and a fixed number of nuts being employed for each current; so that, although a small-current cartridge could be put on a heavy-current cut-out, yet a heavy-current cartridge cannot be put on to a small-current cut-out. The nut and contact surface at the top of the cartridge is covered by an insulating cap. A number of switch-boards and other fittings are also shown and described in the paper.

E. SEHRWALD—THE BEHAVIOUR OF THE HALOGENS RELATIVELY TO RÖNTGEN RAYS

(*Beiblätter*, 1897, *No* 1, *p*. 64.)

The author has obtained the following results, viz. :—

The halogens, chlorine, bromine, and iodine, in the pure condition, are impermeable in a high degree to the Röntgen rays, in this respect being very similar to the metals. This property belongs to the atom of the halogens, and not to the grouping of the atoms in the molecule, and thus extends to halogen compounds. Cyanogen, notwithstanding its similarity chemically to the halogens, is easily permeable to the x rays.

The elements forming the basis of organic compounds—viz., carbon, hydrogen, oxygen, and nitrogen—are almost perfectly permeable; of these, however, nitrogen is the least permeable.

The shadows which soft animal tissues throw are due to the iron of the hæmoglobin and of the alkali metals—for the greater part, to the amount of chlorine they contain. Of the remaining metalloids, phosphorus and sulphur and still more arsenic and antimony, throw intense shadows, boron much less Bisulphide of carbon lets very little of the x radiation penetrate.

Sodium throws only comparatively slight shadows compared with potassium and calcium. The Röntgen rays are not able to set the Crookes radiometer in rotation.

CLASSIFIED LIST OF ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM

Appearing in some of the principal Technical Journals during the Month of JANUARY, 1897.

S. denotes a series of articles. I. denotes fully illustrated.

ELECTRIC LIGHT AND POWER.

F. Uppenborn—The Municipal Electrical Works of Munich.—*E. T. Z* part 1, 1897, p. 2 (I.).

Anon.—The Niagara-Buffalo Power Transmission.—*Ibid.*, part 2, p. 23.

O. S. Bussmann—The Glow-Lamp Question.—*Ibid.*, part 4, p. 45.

G. Richard—Arc Lamps (Barnett, Andrews, Adams, Mongin, Klostermann, Pellet) and Foote Carbons.—*Ecl. El.*, vol. 10, No. 1, p. 9 (S. I.).

F. Guilbert—The New Generator Plant of the "Société d'Éclairage et de "Force."—*Ecl. El.*, vol. 10, No. 2, p. 55 (I.).

A. Hess—The Progress of Electricity in 1896: Applied Electricity.—*Ibid.*, No. 3, p. 97.

A. Moutier—The Central Station of Buda-Pesth.—*Ibid.*, p. 103 (I.).

N. Popow—On the Deposit of Carbon Particles on the Interior of the Glass Bulb of Incandescent Lamps.—*Ecl. El.*, vol. 10, No. 4, p. 151 (I.).

G. Richard—Mechanical Applications of Electricity: Elevators (Stevens and Major, Hall, Smith, Sprague); Overhead Travellers (Berry, Tangye, and Meacock).—*Ibid.*, No. 5, p. 201 (S. I.).

DYNAMO AND MOTOR DESIGN.

Dr Behn-Eschenberg—On the Calculation of Iron Loss in Alternators of the So-called Inductor Type.—*E. T. Z.*, 1897, part 2, p. 21 (I.).

Dr. Lionil-Fleichmann—A Graphical Method of Estimating the Effective Electro-motive Force of Pressure Curves.—*Ibid.*, part 3, p. 35.

C. P. Steinmetz—Compound Alternator for Two-Phase Currents.—*Ecl. El.*, vol. 10, No. 1, p. 31 (I.).

Alex. Rothert—A New Dynamo for a Three-Wire Circuit.—*Ibid.*, No. 4, p. 172 (I.).

F. Guilbert—The Function of Condensers in the Armature Circuits of Asynchronous Motors.—*Ibid.*, No. 5, p. 193 (I.).

TRACTION.

Anon.—The Cirla System of Electric Tramways with Underground Conductors.—*E. T. Z.*, 1897, part 2, p. 22 (I.).

Ricardo Arno—An Electric Street Tramway for Single-Phase Alternating Current.—*Ibid.*, part 3, p. 35 (I.).

Anon.—The Cirla Electric Tramway.—*Ecl. El.*, vol. 10, No 1, p. 30 (I.).

— MacCulloch—The Central Station for Electric Tramways.—*Ibid.*, p. 32.

G. Pellissier—The Progress of Electricity in 1896: Electric Traction.—*Ecl. El.* vol. 10, No. 2, p. 49 (S.).

E. Bowen—The Track, and the Rail Bonds used in the Tramway Industry.—*Ecl. El.*, vol. 10, No. 3, p. 130 (I.).

Anon.—The Electric Traction of Boats on the Burgundy Canal.—*Ibid.*, No. 5, p. 212.

MAGNETISM.

F. F. Martens—The Magnetic Induction of Horizontal Discs rotating in the Earth's Field.—*Wied. Ann.*, vol. 60, part 1, 1897, p. 61 (I.).

L. Houllevigne—On the Influence of Magnetisation on Thermo-electric Phenomena.—*Beibl.*, vol. 21, part 1, 1897, p. 45.

L. H. Siertsema—On the Impossibility of Diamagnetic Substances according to Duhem.—*Ibid.*, p. 46.

C. R. Mann—On the Demagnetisation Factors of Cylindrical Rods.—*Ibid.*, p. 46.

A. Schuster—On the Magnetic Forces acting on Moving Electrified Spheres.—*Phil. Mag.*, vol. 43, No. 260, p. 1.

H. T. Moureaux—On the Absolute Value of Magnetic Elements up to January 1st, 1897.—*C. R.*, vol. 124, No. 2, 1897, p. 77.

INSTRUMENTS AND MEASUREMENTS.

Dr. B. Walter—On the Regulation of Röntgen Tubes.—*E. T. Z.*, part 1, 1897, p. 5.

F. Uppenborn—The Cable Testing Car of the Munich Municipal Electrical Works.—*Ibid.*, part 3, p. 38 (I.).

G. W. Meyer—On a Method of Estimating the Frequency of Alternating Currents.—*Ibid.*, part 4, p. 4 (I.).

R. Abegg—Dielectric Constants at Low Temperatures.—*Wied. Ann.*, vol. 60, part 1, 1897, p. 54.

E. Müller—Experimental Researches on the Heat Conductivity Constant of Air.—*Wied. Ann.*, vol. 60, part 1, p. 82 (I.).

F. Hasenöhrl—On the Temperature Coefficients of Dielectric Constants in Liquids, and the Massotti-Clausius Formula.—*Beibl.*, vol. 21, part 1, 1897, p. 37.

A. Kleiner—On a New Galvanometer.—*Ibid.*, p. 44.

— Classen—On the Protection of the Mirror Galvanometer against Earth Currents.—*Ibid.*, p. 45.

E. van Everdingen—Remarks on the Method of Observation of the Hall Phenomenon.—*Ibid.* p. 47.

P. Zeeman—Measurement of the Influence of Magnetisation, Perpendicular to the Plane, on the Light reflected from an Iron Mirror.—*Ibid.*, p. 48.

L. H. Siertsema—Measurement of the Magnetic Dispersion in Gases.—*Ibid.*, p. 48.

H. Diesselhorst—On the Potential of Circular Currents, with the Use of the Helmholtz Electro-Dynamometer.—*Ibid.*, p. 50.

P. Zeeman—Measurement of the Absorption of Electric Vibrations of different Periods of Vibration in Electrolytes of different Concentration.—*Ibid.*, p. 51.

F. T. Trouton—On the Duration of the x Rays with every Spark.—*Ibid.*, p. 60.

Greiner and Friedrichs—Manganese Glass Tubes for the Production of x Rays.—*Ibid.*, p. 63.

H. Hinterberger—On an Instrument for Measuring the Intensity of x Rays.—*Ibid.*, p. 63.

A. Oberbeck—On the Absorption of the Röntgen Rays.—*Ibid.*, p. 65.

W. Stroud and J. B. Henderson—A Satisfactory Method of Measuring Electrolytic Conductivity by means of Continuous Currents.—*Phil. Mag.*, vol. 43, No 260, 1897, p. 19 (I.).

A. Gray—On the Estimation of "the Waste Space round the Needle of a Galvanometer."—*Ibid.*, p. 36.

E. H. Barton—Absorption of Electric Waves along Wires by a Terminal Bridge—*Ibid.*, p. 39.

J. A. Bose—On a Complete Apparatus for the Study of the Properties of Electric Waves.—*Ibid.*, p. 55 (I.).

W. H. Julius—A New Method of Protecting Sensitive Measuring Instruments against Terrestrial Vibrations.—*Jour. de Phys.*, vol. 6, Jan., 1897, p. 18 (I.).

H. Armagnat—Standardised Apparatus.—*Ecl. El.*, vol. 10, No. 2, p. 70 (S.).

Anon.—Automatic Electro-magnetic Switch of the Compagnie des Compteurs.—*Ecl. El.*, vol. 10, No. 2, p. 77 (I.).

A. Armagnat—Standardised Apparatus: Voltmeters.—*Ibid.*, No. 3, p. 115 (S. I.).

Anon.—The Kelvin Meter.—*Ibid.*, No. 3, p. 128 (I.).

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A. Blondel and A. Broca—The Universal Photometer for Binocular Vision.—*Ibid.*, No. 4, p. 145 (I.).

A. Armagnat—Standardised Apparatus Ampere-Meters, Wattmeters, Ohmmeters.—*Ibid.*, No. 4, p. 160 (S. I.).

G. H. West—Automatic Commutator for the Tables of Telephone Exchanges.—*Ibid.*, No. 4, p. 175 (I.).

A. Armagnat—Standardised Apparatus: Registering Apparatus.—*Ibid.*, No. 5, p. 209 (I.)

M. G. Lippmann—Methods of Comparing, by means of the Electric Spark, the Duration of Oscillation of Two Pendulums adjusted sensibly to the same Period.—*C. R.*, vol. 124, No. 3, p. 125.

A. PÉROT and CH. FABRY—On an Absolute Electrometer for Measuring Small Differences of Potential.—*Ibid.*, p. 180.

G. MANEUVRIER and J. FOURNIER—On the Determination of the Ratio of the Two Specific Heats of Acetylene.—*Ibid.*, No. 4, p. 183.

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ANON.—Extract of the Report on the German Imperial Post and Telegraph Offices during the Years 1891 to 1895.—*E. T. Z.*, part 1, January, 1897, p. 23 (S.).

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ANON.—Telegraphy in Brazil in 1893.—*Ibid.*, p. 11 (S.).

ANON.—Telegraphy and Telephony in the Netherlands in 1895.—*Ibid.*, p. 15 (S.).

E. BRYLINSKI—On the Difficulty of Designing a Submarine Telephone Cable.—*Ecl. El.*, vol. 10, No. 1, p. 14 (S.).

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E. PIÉRARD—The Maximum Capacity of Standard Telephone Tables.—*Ecl. El.*, vol. 10, No. 5, p. 214.

ELECTRO-CHEMISTRY.

N. ZELINSKY and S. KRAPIWIN—On the Electric Condition of Salts and some Acids in Methyl Alcohol.—*Beibl.*, vol. 21, part 1, p. 39.

E. ANDREAS—Electric Excitation by Chemical Means.—*Ibid.*, p. 40.

C. J. READ—The Jacques Carbon Element.—*Ibid.*, p. 41.

C. M. GARDON—A New Method of Estimating Polarisation Capacity.—*Ibid.*, p. 41.

O. F. TOWER—An Addition to the Study of the Super-Oxide Electrode.—*Ibid.*, p. 42.

W. A. SMITH—Study of the Super-Oxide Electrode.—*Ibid.*, p. 42.

H. WEYER—Improvements in Electrodes.—*Ibid.*, p. 43.

W. LÖB—The Use of Porous Carbon Cylinders in Electrolytic Researches.—*Ibid.*, p. 43.

D. TOMMASI—On a New Electrolytic Cell.—*Ibid.*, p. 43.

CH. MARGOT—The Electric Copper-Plating of Aluminium.—*Ibid.*, p. 44.

CH. MARGOT—Remarks on "The Electric Copper-Plating of Aluminium."—*Ibid.*

E. SEHRWALD—The Behaviour of Halogens with Röntgen Rays.—*Ibid.*, p. 64.

H. GLADSTON—The Effect of the Common Rays and the Röntgen Rays on Metals and their Salts.—*Ibid.*, p. 7.

G. PELLISSIER—The Progress of Electricity in 1896: The Products of the Electric Furnace.—*Ecl. El.*, vol. 10, No. 2, p. 49.

DUGALD C. JACKSON—Electrolytic Corrosion by the Return Current of Tramways.—*Ecl. El.*, vol. 10, No. 3, p. 113.

H. Moissan—The Preparation of Manganese in the Electric Furnace.—*Ecl. El.*, vol. 10, No. 5, p. 215.

H. Moissan—The Preparation of Silicon in the Electric Furnace.—*Ecl. El.*, vol. 10, No. 5, p. 216.

ACCUMULATORS.

L. Zehnder—On the Management of High-Tension Accumulators.—*Wied. Ann.*, vol. 60, part 1, p. 47 (I.).

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P. Drude—On the Theory of Longitudinal Electric Waves in Wires.—*Wied. Ann.*, vol. 60, part 1, p. 1.

P. Glan—Theoretical Investigations on Elastic Bodies and Light.—*Ibid.*, p. 174.

M. L. N. Vandevyver—The Law of Photographic Action of the x Rays.—*Jour. de Phys.*, vol. 6, January, 1897, p. 23.

F. Guilbert—Oscillating Currents and Complex Quantities.—*Ecl. El.*, vol. 10, No. 1, p. 22 (I.).

VARIOUS.

F. Busch—A Hundred Simple Experiments on the Derivation of the principal Electric Laws.—*Beibl.*, vol. 21, part 1, p. 37.

F. C. G. Müller—New Contribution to the Technics of Instruction.—*Ibid.*, p. 39.

I. Friedländer—Old and New Experiments on Lichtenberg Figures and Breath Figures on Photographic Plates.—*Ibid.*, p. 51.

W. König—On Röntgen Rays.—*Ibid.*, p. 56.

P. Gérard—On the Photography of Invisible Bodies.—*Ibid.*, p. 56.

F. L. Phipson—Explanation of Röntgen Rays.—*Ibid.*, p. 58.

T. V. Dwelshauvers-Dery—Hypotheses and Observations with regard to x Rays.—*Ibid.*, p. 58.

— Buka—Röntgen Rays of High Intensity.—*Ibid.*, p. 61.

C. M. Gariel—On Fluoroscopy: The Use of the x Rays for the Direct Examination of the Inner Organs.—*Ibid.*, p. 63.

— Macintyre—Some Results with Röntgen x Rays.—*Ibid.*, p. 63.

P. Spies—The Excitation of Fluorescence by the Uranium Rays.—*Ibid.*, p. 65.

— Axenfeld—The Reaction of Flies on Röntgen Rays.—*Ibid.*, p. 68.

C. E. Guillaume—On x Rays.—*Ibid.*, p. 68.

— Feilchenfeld—Eczema produced by Röntgen Rays.—*Ibid.*, p. 69.

H. Hinterberger—On Experiments by the Aid of Röntgen Rays.—*Ibid.*, p. 69.

H. Hinterberger—Röntgen Pictures of Parts of Plants.—*Ibid.*, p. 70.

J. L. Hoorweg—Experiments with Röntgen Rays.—*Ibid.*, p. 70.

F. Cajori—Searching for x Rays in the Sun's Rays on Pike's Peak.—*Ibid.*, p. 71.

A. CHASSY—On an Electro-capillary Experiment.—*Jour. de Phys.*, vol. 6, January, 1897, p. 14.

E. FONTAINE—The Influence of the Electric Condition of a Liquid Surface on the Heat of Vaporisation of that Liquid.—*Ibid.*, p. 16 (I.).

C. RAVEAU—The Progress of Electricity in 1896: Pure Electricity.—*Ecl. El.*, vol. 10, No. 1, p. 5.

G. BLONDIN—On the Origin of the Röntgen Rays.—*Ibid.*, No. 3, p. 124.

M. E. VICAIRE—Experimental Study of the Water Consumption of Locomotives. —*C. R.*, vol, 124, No. 1, 1897, p. 23.

MM. POTAIN and SERBANESCO—Radiographs taken of Patients affected with Gout and Chronic Rheumatism.—*C. R.*, vol. 124, No. 3, p. 130.

M. LE ROUX—On the Equation of Telegraphists.—*Ibid.*, p. 143.

L. BENOIST—The Law of Transparence of Gases to the *x* Rays.—*Ibid.*, p. 146.

M. RADIGUET—The Fluorescence of Vitrified Substances under the Action of the Röntgen Rays.—*C. R.*, vol. 124, No. 4, p. 179.

M. BOISSEAU DU ROCHER—A Generator for Rapidly Intermittent Currents: Physical Effects; Physiological Effects; Therapeutic Effects.—*Ibid.*, No. 4, p. 185.

MM. GUNTZ and MASSON—Action of Carbonic Acid and Oxide of Carbon on Aluminium.—*Ibid.*, No. 4, p. 187

NOTICE.

1. The Institution's Library is open to members of all Scientific Bodies, and (on application to the Secretary) to the Public generally.

2. The Library is open (except from the 14th August to the 16th September) daily between the hours of 11.0 a.m. and 8.0 p.m., except on Thursdays, and on Saturdays, when it closes at 2.0 p.m.

An Index, compiled by the late Librarian, to the first ten volumes of the Journal (years 1872–81), and an Index, compiled under the direction of the Secretary, to the second ten volumes (years 1882–91), can be had on application to the Secretary, or to Messrs. E. and F. N. Spon, 125, Strand, W.C. Price Two Shillings and Sixpence each.

JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXVI. 1897. NO. 128.

The Two Hundred and Ninety-eighth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, March 11th, 1897—Sir HENRY MANCE, C.I.E., President, in the Chair.

The minutes of the Ordinary General Meeting held on February 25th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Andrew S. Dunn.
Arthur Ellis.
Arthur Henry Pott.
James Robert Sykes, jun.

From the class of Students to that of Associates –

Richard Thomas Durran.
George F. Herron.
Neville James Payn.
John J. Pease.
Percy S. Sheardown.
Bernard J. Shillito.
Charles Alfred Spon.
C. Harman Wigan.

Mr. H. Carpmael and Mr. W. P. Whitehead were appointed scrutineers of the ballot for new members.

The PRESIDENT: I have, gentlemen, much pleasure in announcing that the Council have appointed as an Honorary Member of the Institution, His Excellency Dr. Von Stephan, the Postmaster-General of the German Empire. I trust you will all approve of that course. As Mr. Preece is very well acquainted with Dr. Von Stephan, perhaps he would like to make a few remarks.

The appointment of Dr. Von Stephan as an Honorary Member was unanimously approved.

Mr. W. H. PREECE: I scarcely think, Sir, it is necessary to say much on this point now. You have submitted to the meeting the resolution of the Council, and it has been accepted. But I may say that in accepting that resolution, and in appointing Dr. Von Stephan as an Honorary Member, you have done not only a thing that is right in itself, but you have done a great honour to this Institution. There is no man in Europe at the present day who has done more for the advance of electricity than Dr. Von Stephan. He has been Postmaster-General of Germany for over 20 years. There has not been a single progressive electrical movement of any kind in Germany of which he has not been either the prime mover or supporter. He is a man, not only as a statesman, but as chief of his department, second to none in the world. I think he will certainly be here in 1901. He may be here before. Were it not for an accidental injury to his foot, which has resulted in the amputation of one of his toes, he would be coming, probably, in the next month. There is to be a great gathering at Washington in June, and nearly all the postal celebrities of the Continent are passing through England *en route* to the States. We regret that Dr. Von Stephan is laid up, and I would suggest that an expression of sympathy at his indisposition be added to the letter which the Secretary will have to write when forwarding to him the resolution which you have agreed to to-night.

The PRESIDENT: I am sure Mr. Preece's suggestion will be approved by the members.

Agreed to unanimously.

The following paper was then read:—

ON SOME REPAIRS TO THE SOUTH AMERICAN COMPANY'S CABLE OFF CAPE VERDE IN 1893 AND 1895.

By H. BENEST, Assoc. M. Inst. C.E., Associate.

In relating the experiences of these two cable repairs, the writer believes that some novel features present themselves—firstly, in the presumable existence of submarine streams, or outlets of springs, or other abnormal conditions, in a certain locality of the North Atlantic Ocean; and, secondly, in the effects of such physical conditions upon telegraph cables. Mr. Benest.

That some such conditions exist appears to be pretty certain by the evidence supplied at the points of rupture.

A brief *resumé* of the establishment of the submarine telegraph between the coasts of Africa and Brazil is here necessary before recounting the work of repairs.

This South American cable was made at Silvertown, and laid in 1892 by the India-Rubber, Gutta-Percha, and Telegraph Works Company, Limited, their s.s. "Silvertown" carrying and laying the whole cable.

The terminal points are at Saint Louis, in Senegal, on the West Coast of Africa, and at Pernambuco, on the coast of Brazil. A station is established on the island of Fernando Noronha, some 250 miles from Pernambuco, thus forming two sections.

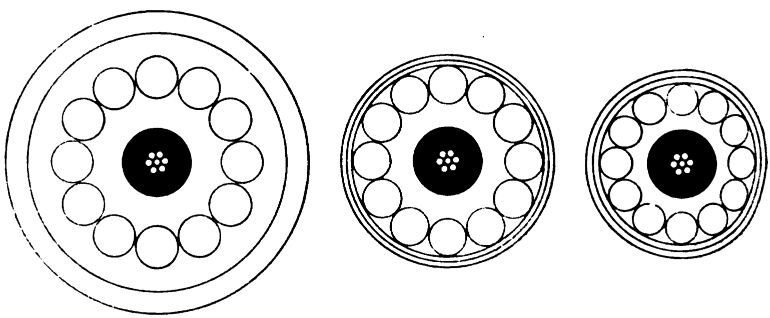
The section between Pernambuco and Noronha has a conductor of 225 lbs. of copper and a dielectric of 225 lbs. of india-rubber per nautical mile. A substantial jute bedding surrounds the core, and a sheath of 16 galvanised steel wires of 0·092 inch diameter is superposed; the breaking stress of the wire employed being 84 tons to the square inch. Each wire is coated with a tarry compound, and has a tarred cotton tape helically laid around it. The whole cable is served round with a helically laid cotton tape, and then with 20 hemp cords applied with a lay reverse to that of the tape. The finished diameter is slightly

Mr Benest. over 1 inch. This type is called light deep-sea, and its weight per knot wet in air is 43½ cwt., and in sea water 23 cwt. The specific gravity is 2·107.

The heavy deep-sea is sheathed with 12 galvanised iron wires 0·160 inch diameter, of breaking stress of 30 tons per square inch; each wire is coated with a tarry compound, the sheathed cable being served with two spiral tapes of reverse lays, with a tarry compound between them and a bituminous one outside. Its weight per knot wet in air is 60 cwt., in sea water 43 cwt.; specific gravity, 3·75; finished diameter, 0·975 inch.

The light intermediate type is sheathed with 12 No. 6 gauge 0·192 inch diameter galvanised iron wires; each wire is coated as in the deep-sea type, and the external covering consists of two tapes and compound. The finished diameter is 1·14 inches; its weight per knot wet in air is 86 cwt., in sea water 62 cwt.; specific gravity, 3·65.

The heavy intermediate type is sheathed with 12 galvanised iron wires 0·252 inch diameter, No. 3 gauge; each wire is coated as in the other types, the sheathed cable being served with two coverings of jute yarn and alternate coats of compound. Its weight per knot wet in air is 155 cwt., in sea water 107 cwt.; specific gravity, 3·2; finished diameter, 1·6 inches.



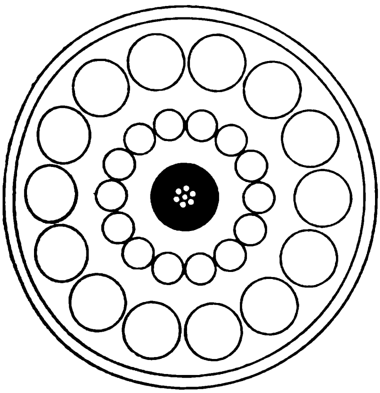
HEAVY INTERMEDIATE LIGHT INTERMEDIATE HEAVY DEEP SEA

The shore end type is sheathed first with 12 galvanised iron wires, coated as before, each 0·160 diameter, then served with a covering of tanned jute yarn. The second and outer sheathing consists of 14 wires, galvanised and compounded, 0·300 inch diameter,

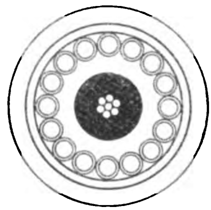
two coverings of jute yarn and compound forming the outer protection. Its weight wet in air per knot is 288 cwt., in sea water 213 cwt.; specific gravity, 4·14; finished diameter, 2·01 inches. Mr. Benest.

This description also applies to the long section between Fernando Noronha and Senegal, except that the dielectric is of gutta-percha instead of india-rubber.

For telegraphic working, the two sections are joined up at the island, messages being sent direct between St. Louis and Pernambuco.



SHORE END



LIGHT DEEP SEA

In the light deep-sea type of cable, which forms the major portion, pliability and buoyancy are combined. It is easy to coil, runs freely, has no tendency to kink, requires small retarding force in paying out, and in picking it up proves remarkably light to raise.

The length of the section between Pernambuco and Fernando Noronha is 357 nautical miles, and the distance over ground along line of cable 316·5 nautical miles. The distance in a direct course is 289 miles, and between the island and Cape San Roque, the nearest point on the Brazilian coast, 193 miles. The greatest depth encountered during the laying was 2,480 fathoms.

The island of Noronha lies N.E. and S.W. The extreme

r. Benest. length, including outlying rocks and islets at the N.E. extremity, is about 7 miles; its greatest breadth is 1¼ miles. Cape Placillière, to the S.W., is in latitude 3° 5′ S., longitude 32° 28′ W. This portion is very bold and exceedingly picturesque, consisting of bush-covered cliffs and rocky headlands, alternating with green hollows and sandy coves fringed with palms.

The length of the section between Fernando Noronha and St. Louis, Senegal, was originally 1,702·148 nautical miles, the distance over ground along line of cable being 1,548·218 miles. The total slack was originally 9·9 per cent.; the greatest average depth being 2,750 fathoms.

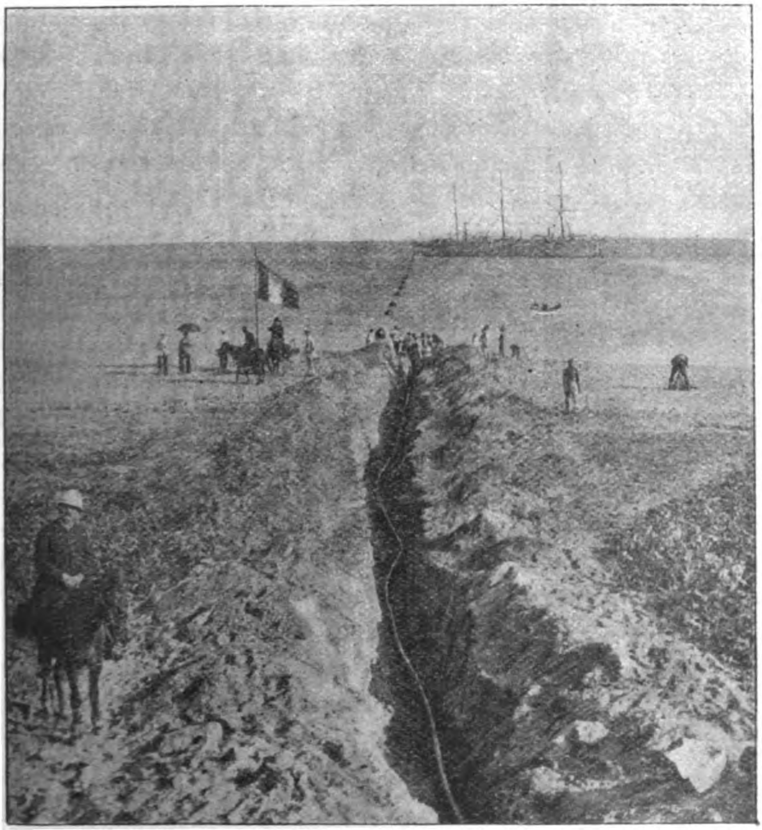
The coast of Africa in the latitude of Senegal does not lend itself to flowery description; a bare, sandy, hazy coast line, with a heavy surf continually breaking thereon, conveying to the mind an idea of desolate discomfort. Add to this a fierce heat during the day from an unclouded sun, tempered certainly by a cool N.E. wind during the fine season, or winter—from November to April—the other portion of the year being calm and rainy, with frequent tornadoes, and all is said that can be for this uninviting portion of the world.

The paying out of the long section was accomplished under most favourable conditions of weather. During eight days the depth passed over was from 2,300 to 2,800 fathoms. In nine days a length of 1,657 knots, including the shore end at Fernando, was laid; this being equal to 184 knots each day of 24 hours, or at the rate of 7·6 knots per hour.

These particulars are here set forth in order to show that the cable was well and truly laid. The agency by which it was broken, three months from the date of its completion in September, 1892, must therefore have been remarkable, especially at such a presumably safe depth as it occurred—between 1,200 and 1,300 fathoms.

News was received from St. Louis, in Senegal, on December 26th, 1892, that the St. Louis-Fernando Noronha section had broken down on that day; rough electrical observations at St. Louis indicating the break at about 130 miles distant by cable from the landing.

It appeared that on the 20th December both St. Louis and Fernando were receiving messages with some difficulty for the first time since the completion of the line, and that the speed of working had to be reduced by about one-third. On December 24th a very heavy surf was breaking on the beach at St. Louis, the precursor of a violent storm which raged until the morning Mr. Benest



Landing Shore End of Fernando Noronha and St. Louis Section.

of the 26th. Some idea of the severity of the gale can be formed from the fact that the seas washed through the roadway in front of the cable house at St. Louis, and flooded a large portion of the native town of Guet n dar.

The signals between St. Louis and Fernando had continued to

Mr. Benest. be of the same unsatisfactory character from the 20th, until the closing of the office on the night of the 25th, when the storm was at its height. On the morning of the 26th the cable was found to be broken.

The cable steamer "Dacia" was fitted out with all despatch, and left the Thames with cable and all appliances for repairs on 7th January, 1893, arriving off St. Louis on 21st January, having provisioned at Santa Cruz de Tenerife on the way south.

Tests at St. Louis showed the position of the fault to be about 118 knots distant.

While the chief electrician, Mr. E. March Webb, was testing from shore, the ship proceeded to carry out a series of profile soundings during three days, when the "Dacia" put into the port of Dakar to take on board Mr. Webb and two representatives of the South American Company, and at once resumed sounding work. Some 50 casts with the wire were taken before and during grappling operations up to recovery of first end; a regular and easy descent from shoal to deep water, with soft green and grey mud bottom of varying consistency, being found, as details given further on will show.

The mean distance overground along the line of cable from St. Louis to the fault was estimated at 100 nautical miles. A mark buoy was placed at 78 miles, with a view to grapple the cable at a point to which the line would eventually be diverted in avoiding the dangerous ground.

Unfavourable weather interfered with work until the 29th January. On the 30th, at early dawn, the mark buoy was sighted, with flagstaff broken and lamps gone. These were replaced, and in the afternoon, the sea having subsided, the grapnel was lowered in a depth of 850 fathoms. Grappling rope and chain were paid out to a length of 1,200 fathoms, and a drag was made to the S.E., the strain shown on dynamometer being from 2 to 3 tons. That evening the cable was hooked, the strain during picking up being from 3 to 4½ tons. At nine o'clock the cable was at the bows, the highest strain being 6 tons, falling to 4, and 3½ tons on slack cable coming over the ground.

After cutting the cable, and speaking St. Louis, the cable was

tested, and found to be perfect electrically. Tests for conductor resistance and capacity were taken from both ship and shore, to determine length of piece in circuit, which was found to be 87·888 N.M. The mean depth on this length is 565 fathoms, and the mean bottom temperature, from soundings, is 47° Fahr.; calculated temperature, 46·75° Fahr. Mr. Benest

The temperature and depths are set forth in detail in Appendix III.; these being a transcript of Tables A and B from Mr. Webb's electrical report, as printed in the company's log of the expedition.

Picking up the cable towards the break was now proceeded with until the following day. The mean strain while picking up was 2·6 tons, falling to 2·1 tons when at rest, the cable coming in in good condition, quite freely and coiling well. On approaching the end, at a distance of 2 miles the strain rose to 6 tons; and after some manœuvring the cable evidently cleared itself from the bottom, and the end soon afterwards came on board from a depth of 1,220 fathoms. The cable showed evidence of much rubbing, the outer cords being stripped in places, unwound, and rucked up. The tape covering for about 140 fathoms from the end was rubbed bare to the sheathing wires *on one side only.* At the broken part about half an inch of the conductor was exposed, both copper and gutta being nearly covered by the sheathing wires; the appearance indicated a tension break, with some slight torsion. The sheathing wires were well drawn down at their ends, showing a fair break at full breaking strain; in fact, from its appearance it might have been broken in a testing machine.

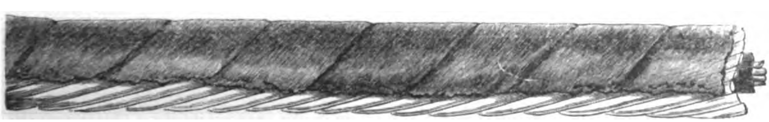


Fig. 1.
(Not to scale.)

Additional soundings were taken in a profile line from this position towards the E.S.E., and across the line of cable (see charts and profile sections A to B, and so on, Plates 1 and 2).

A marked difference was observed in the character of the

Mr. Benest. bottom on this line of soundings—a very soft green mud, and ooze, in place of the stiff green mud hitherto found.

While the sounding was going on, the recently picked up cable, after eliminating the damaged portion near the end, was joined up with the new cable ready for re-laying; 23·75 N.M. being thus utilised.

On the 2nd of February the last line of soundings was completed. A buoy was prepared for a mark, but could not be placed in position for grappling the Fernando side of the broken cable, because of weather unfavourable for observations to determine the ship's position, through a dense haze which prevailed all day, caused by fine particles of yellow dust blown from the desert land by strong north-easterly winds. Later in the day sights were obtained, and a mark buoy put down in latitude 14° 56′ N. and longitude 18° 3′ W., the depth being 1,200 fathoms. The grapnel was soon afterwards lowered in 1,300 fathoms, 1,650 fathoms of rope and chain being paid out. A drag was made to the S.E., with the strain steady at 3½ tons. A considerable swell existing, the grapnel rope soon began to suffer by hammering against the bow of the vessel, and by being rolled along the plating by the "send" of the ship. After five hours' grappling, accompanied by severe rolling of the ship, wind and sea had risen to such an extent that operations had to be suspended for a time.

On the afternoon of 3rd February, weather having moderated, fresh lights were placed upon the mark buoy, and the grapnel was lowered. A drag was again made to the S E., the strain shown on the dynamometer being 4 tons. After grappling for five hours, at a rate of about 1 mile overground per hour, the strain rose to 5½ tons. The grappling line was now shortened. The strain while shortening in was 6 tons. No appreciable progress overground was made during half an hour after this, and, the strain still rising, it was believed that cable was hooked. The operation of picking up was now commenced, a 5-ton strain being indicated, increasing to 6 tons, and to 7 tons when the bight of the cable appeared at the surface. Estimating from the approximate angle between the legs of the cable bight when above sea

surface, the strain on the cable was about 5 tons. The indicated strain fell to 6 tons when at rest, and afterwards to 5 tons, as the slack cable was drawn along the ground; thus proving that a sufficient quantity of slack had been laid originally. Mr. Benest.

Four hours were occupied in picking up, and at 3 a.m. the cable was secured by chains and drum ropes and cut at the bight. The Fernando end was taken on board, and electrical tests showed that portion of the cable to be electrically perfect.

This Fernando end was now buoyed, and picking up the loose piece towards break was proceeded with, the strain keeping pretty steady at 4 tons, varying slightly to 3½ tons. On approaching the position of the break a sounding gave 1,160 fathoms, soft mud; the strain soon after this increasing to 5½ tons; and frequent stoppages were now necessary to allow cable to break away from the bottom. The strain suddenly falling to 2 tons indicated that it had cleared some obstruction. The cable now recovered was "screwy," and soon showed evidence of much rubbing, like its counterpart on the St. Louis portion.

On the evening of February 4th the end came on board. The break was similar to, and coincident with, the other end; the sheathing wires being close around the core, the conductor scarcely showing. The gutta-percha appeared as though it had been nipped by some hard pressure. Nearly 21 miles was recovered on this piece, which, after eliminating the damaged part, was joined up, spliced to the St. Louis portion of picked up cable, and coiled down ready to relay.

Fig. 2.—Break of 1893.
(Photographed in 1897.)

The depth at this position was 1,220 fathoms, soft mud and ooze.

The buoy on the end of the St. Louis portion was now "made for," to verify its safety, before raising the Fernando end

Mr. Benest. and joining up cable on board thereto preparatory to paying out and completing repair. Contrary wind and current prevented the ship sighting the buoy that night, and it was at daylight on February 6th before it was seen.

Two freshly trimmed dioptric lights were put on the buoy, and the return to the Fernando end was at once made. Upon reaching this end at the close of the day the weather looked so promising that it was decided to at once pick up the cable and splice on. The strain while picking up was 2·9 tons, and when stopped 2·1 tons. A length equal to 1¼ knots of the cable that had been hanging from the grapnel when raising the bight was taken in, and not a sign of a kink found.

At eight o'clock, the splice being finished, the bight of the cable was slipped from forward aft, to bring cable on to the after (paying out) gear. The ship's engines were put slowly ahead at first, on account of cable being high in the tank and there being but slight cohesion between the turns of the coil.

The first course was N. 28° W. true. The speed was increased to 4 knots per hour, and later on to 5 knots, at which rate the ship was kept during the night. The distance run upon the first course was 14½ miles. The second course was N. 19° E. true; distance run, 12½ miles. The third course was N. 73° E. true, and distance run 25 miles; all by log.

During the run on this course, the ship was stopped in order to coil down in the tank a laid out splice. When all was clear, the ship's engines were put on to 40·44 and 48 revolutions, representing from 5 to 6 knots speed per hour; the course being changed to N. 62° E. true.

The weather being overcast, morning observations could not be taken. Later on the sky cleared and a position was found, but so much haze obscured the horizon that the "fix" was afterwards proved to be incorrect. It placed the ship several miles astern of position by dead reckoning. The ship's engines had meanwhile been increased to 50 revolutions, equal to 6½ knots per hour. Shortly after eight o'clock the buoy on St. Louis end was sighted about 2 miles slightly on the starboard bow. At ten o'clock the cable, St. Louis end, was on board, and, tests being satisfactory,

the other, Fernando end, was passed from aft, and final splice started; a length equal to 0·25 N.M. on St. Louis end having been hove in to ensure freedom from kinks. Mr. Benest.

In the afternoon of 7th February, the splice being finished, the final bight was slipped, and the "Dacia" set on for Dakar, leaving the mark buoys in position until learning the result of the final electrical tests from St. Louis.

On testing towards Fernando, when the cable was taken on board for splicing on to the tank section, a dielectric resistance per N.M. of 13,756 megohms, at a calculated mean temperature of 34°, was found; the copper resistance being 4·885 ohms per N.M.; mean depth being taken as 2,142 fathoms, and mean bottom temperature of 35° Fahr. The distance by cable from this point to Fernando was 1,567·4 N.M.

A length of 47 N.M. of cable was recovered, and 73·6 N.M. used in repair, 39·6 N.M. of which formed part of the 47 N.M. recovered. The length of the restored section, after repair, was increased by 26·5 N.M.; the original length being = 1,702·15 N.M. The present = 1,728·67 N.M.; add to this the length of Fernando-Pernambuco section, = 357, gives entire length 2,085·67 N.M.

A slight alteration in mean depth and bottom temperature occurred in the working out of results, viz.:—

		Mean Depth.	Mean Bottom Temp.
Original section	...	2,172 fms.	34·9 F.
After repairs	...	2,142 „	35·2 F.

Charts showing direction in which the cable was picked up and relaid are appended (Nos. 1 and 2, Plate 3). It will be observed that the cable was diverted from the original line along other courses for a distance overground = 57 N.M., and 17 miles to the westward of the position where the rupture occurred. The total slack given was = 16·3 N.M., = 28½ per cent. on the length paid out. Throughout this repair most valuable assistance was rendered, apart from the skilful navigation of the ship, by Capt. A. S. Thomson and his officers.

These repairs being completed, the vessel returned to London; but, unfortunately, further experience in this locality was to come.

Mr. Benest. Two years later—in March, 1895—a second interruption to the South American cable occurred, when the "Dacia" was engaged on some repairs to the Cadiz-Tenerife section of the Spanish National Telegraph Company's system, and experiencing very rough weather off Cape Trafalgar in attempting the passage of the Straits of Gibraltar to escape from a south-westerly gale of cyclonic violence.

To convey an idea of the startling suddenness with which this storm almost overwhelmed the ship, it is worth recording that at 8 a.m. a mark buoy and its moorings had just been picked up. The wind was S.W., afterwards veering to W.S.W. and W., fresh, squally, and cloudy. At 10 a.m. it blew a strong gale, with a heavy sea; at noon, a heavy gale; half an hour after noon, a fierce hurricane, with a tremendous sea: the ship rolled to such an extent that a big cutter in the davits was carried away by the boat filling, and she was smashed to bits against the side. The decks were deluged by huge seas, and cattle were washed overboard. The steam launch started adrift through the ship labouring. Vivid lightning, with terrific peals of thunder, accompanied the storm, which had to be faced, as it would have been impossible to have made the passage of the Straits, or to have entered the harbour at Cadiz. Everyone worked with desperate energy. Officers and staff toiled for hours getting tarpaulins into the mizen rigging to keep the ship's head to the sea, the after-sails being split and mizen gaff disabled. Broken ribs, hurts, and bruises to the crew and cable hands occurred while endeavouring to re-secure the heavy steam launch and other gear.

During the evening several steamers drew near, and there was a narrow escape from a serious collision, as the steamers were almost unmanageable. Some of them must have collided during that night with fatal results, for several steamers proceeding from the Mediterranean were never heard of from that day. The unfortunate Spanish cruiser "Reina Regente" had taken the Moorish envoys back to Tangier the previous day, and was upon her return to Cadiz—a five hours' run—when she foundered with 423 souls. Some 2,000 people in Seville and Cadiz were put into

mourning through that awful disaster. The "Dacia" weathered the storm with difficulty, and the next morning made her way through the Straits, running before the moderating gale to Gibraltar, where news of the interruption to the South American cable was received. Later on, at Cadiz, on completion of the repairs to the Spanish cable, orders, with detailed instructions, were received from London to proceed south and effect repair. The ship left Cadiz on April 10th, for a position in latitude 15° 1′ 7″ N. and longitude 18° 4′ 7″ W., about 20 nautical miles to the south-westward of the scene of the former operations, touching *en route* at Santa Cruz de Tenerife to coal and provision. On the 19th April the ship was at the position indicated, a mark buoy being put down and grappling commenced. Mr. Benest.

In view of this second interruption occurring after an interval only of two years from the first repair, it was decided to make a long detour, and while carrying out this operation to utilise temporarily a cable laid along the African coast between St. Louis and Cape Verde Point.

The first drag was made to the westward in depth of 1,250 to 1,300 fathoms, green mud. A heavy intermittent swell, with moderate breeze, and current setting to the northward. Strain indicated on dynamometer varying to as high as 6 tons. Grappling rope being severely knocked and rolled against the bow. Ship rolling heavily.

Conditions not being favourable for securing cable if it were hooked, the grapnel was hove up, and work suspended for the time being.

It was observed that the sea was of a dirty green colour—an unusual appearance in deep water. After dark it became highly phosphorescent, with a strong pungent odour—the more so the nearer to the sea surface; the crew of a ship's boat alongside being severely affected by it.

Grappling was resumed on the 20th, and continued until the morning of the 21st, without result.

The climatic conditions at this position (40 miles W.N.W. from Cape Verde), in depth of 1,200 fathoms to 1,300 fathoms, on the evening and during the night of the 20th, were worthy of

Mr. Benest. remark. The sun set like a pale distorted moon, sea and sky being of an ashen grey, and the horizon shrouded in mist A clammy atmosphere, striking chill and raw. A strong odour of phosphorus, suggestive of gases liberated by myriads of decaying organisms. The scene was most weird and uncanny—Dantesque, in fact. Grey, dull, and silent; not a spark of life in sun, sea, or sky. Darkness, necessitating the cheerful brightness of the electric light, was a welcome change. Throughout the night similar conditions prevailed. On the 21st the atmosphere cleared, the sea assumed a brighter tint of green, and the sun shone; but this change also brought a strong breeze, rendering grappling out of the question. Some few soundings were taken to discover the nature of bottom to the south-westward.

On the 22nd, after a brief lull, the breeze sprang up again, with the usual swell at times, indicative hereabouts of a strong westerly set; the ship being within the influence of the northern equatorial current which sweeps along the north-west coast of Africa, past the Cape Verde Islands, turning to the westward about the latitude of Cape Verde. Part of this current is driven by the north-east trade winds into the Caribbean Sea, but most of it sweeps north-westward outside the West Indies towards the North American coast, as explained by Ferrel's law. This meeting, or diverging, point of the current may probably account for some of the unusual conditions of climate met with.

The next drag was successful in hooking the cable, which was at the ship's bows at an early hour on the 23rd; the northern, St. Louis end, being buoyed, and the Fernando end, after testing, being at once spliced up with the section of new cable in tank for paying out towards Cape Verde, off which place it was the intention to break into the coast line of cable from St. Louis to Cape Verde, and join Fernando thereto, thus restoring communication at the earliest possible moment.

This operation was of a delicate nature, inasmuch as the cable from Fernando had to be buoyed, the coast cable above referred to grappled up, cut, and the northern (St. Louis) portion of this cable joined on to Fernando end, which had to be raised and the bight lowered after making the splice.

The work of paying out cable from the recovered end was successfully performed at a speed of from 5 to 6 knots per hour. A dense haze over the land in the early morning disappointed expectations in a looked-for guide—the Cape Verde light. The morning observations were not reliable either, and the cable distance had to be taken for determining the distance run. The Fernando end was buoyed in 200 fathoms when 47·28 N.M. had been payed out, and ship, by dead reckoning, should have been within a mile of the line of the St. Louis-Yof coast cable, to which connection had to be made. Mr. Beneat.

However, noon observations—latitude 15° N., longitude 17° 30′ 7″ W.—placed the ship 2 miles to the westward of this line. Grappling for the coast cable was started in the afternoon and continued until the evening of the 24th, when, having made six drags and broken up some grapnels, the cable was hooked, only to lose it while heaving it up. Soon afterwards it was hooked again, and raised; the bight being cut, the loose south end buoyed, and the St. Louis portion picked up for 4 miles, when the ship was headed towards buoy on Fernando end, paying out cable. At 8 a.m. the Fernando end was on board. Joint and splice were made, and the bight of the cable lowered to the bottom by a buoy rope, and the end buoyed so as to facilitate the raising of the bight again.

By the use of this portion of the coast cable the circuit between Pernambuco and St. Louis was now continuous, and the "Dacia" was set on for Dakar, to make sure of communication being perfectly restored, and to ship stores sent out from home.

On the 25th the ship arrived there, and received a satisfactory report from St. Louis as to the working of the improvised line of cable between that point and Pernambuco.

Not being able to ship stores, on account of Custom House formalities, instructions were sent through the Government land line to St. Louis to look out for signals on the broken part of the cable on the 27th, and the ship immediately left for the grappling ground.

Position 15° 1′ N. and 18° 4′ W. was reached by forenoon on 26th, the cable on the loose piece towards the break being then

Mr. Benest. taken up. On testing this cable, the length to the break was found to be 35 knots. Recovery of cable was at once started, and continued until the 27th, when the cable parted near the dynamometer under a strain of 7¾ tons, after all means to break it out of the ground had been tried. About 3 knots had been picked up under a strain of between 4 to 5 tons, and four hours' time was occupied in the recovery of this short length.

The ship was now set on for a position in 15° 50′ N. latitude and 17° 11′ W. longitude, to grapple the other portion of the cable from the landing at St. Louis.

On the 28th the ship was found, by morning observations, to be 8 miles north of position required. A mark buoy was soon placed near the line of cable, and preparations were made for grappling, which, however, was not found practicable, on account of strong north-east wind and a heavy swell.

Early on the 29th the grapnel was lowered, and a drag made to the south and eastward. All day was occupied in grappling. but without result. Wind increasing and sea again becoming too heavy, grappling was out of the question. Several soundings now taken across the supposed line of cable showed that the ship had been grappling to the eastward of, and had not actually crossed, the line; the depths now found not agreeing with those shown on the chart near this locality, the casts taken giving, respectively, 714, 641, 589, 552, and 492 fathoms. The position of the 641 fathoms sounding, as afterwards determined, was 1¼ miles to the eastward of the actual line of cable, and the sounding of 492 fathoms 4 miles to the eastward of it. At the position where the mark buoy was placed on the supposed line of cable a depth of 575 fathoms was found, and this proved to be 3½ miles to the eastward of the actual line, which was equal to the error of chronometer as subsequently determined.

On 1st May, there being no signs of the weather moderating, an attempt was made to steam up towards St. Louis, but the ship rolled to such an extent in the high beam sea that the idea was given up, and the ship was kept away for Dakar, where she arrived on the following day.

On 4th May a start was again made, after having got fresh rates of corrections for the chronometers, the standard having been 13 seconds in error, or 3¼ miles—due, no doubt, to the violent oscillations caused by heavy rolling of the ship. Mr. Benest.

On arriving at the scene of operations, the weather showed signs of improvement, and grappling was resumed, with good hopes of success.

On the 5th the cable was hooked, and up to the bows at daylight. On cutting the bight and testing towards St. Louis, the cable between the ship and that point was found electrically perfect. The St. Louis end was then buoyed, and picking up on the loose piece towards the break proceeded with. The strain being high—from 4 to 5 tons—it was necessary to work the gear at low speed.

Fig. 3.—Break of 1895.

Fig. 4.—Break of 1895.
(Photographed 1897.)

It was 7th May before the broken end was reached; 78·5 N.M. having been recovered. Quite 2½ hours were spent in getting in the last mile. Like the other side of the break, for a distance of 3 miles it was a tussle to break the cable out from the bed of mud and detritus that evidently covered it and held it

Mr. Benest fast; the most careful manœuvring being required. Patience was at last rewarded, and by dint of coaxing and handling the ship it gradually broke away—literally foot by foot—until, with a jumping strain of from 2 to 5 and 6 tons, it finally cleared itself. The end when it was hove on board showed evidence of some severe mauling: the sheathing was squeezed flat, and the core protruded in several places.

Extract from Log, May 7th, 1895, *South American Repairs.*—4.25 *p.m.*—" Fault passed off drum. The fault shows that some " hard substance had pressed against the bight of cable with a force " greater than the breaking strain of cable, forcing the core out on " the opposite side of sheathing for about 12 inches and breaking " it; probably a large rock borne down on the cable by a mass of " mud moving at a right angle or part of a right angle to the cable. " Cut fault out = 3 fathoms.

" 4.31 *p.m.*—Resumed picking up; cable coming in much " strained and entirely stripped of serving.

" 4.42 *p.m.*—End of cable came in board bearing evidence of " having been caught round a rock and broken by heavy " strain. *All wires and core broken off flush. Cut half a fathom " off cable.*"

A sounding taken immediately on the spot disclosed a depth of 1,574 fathoms, green mud, when 1,300 to 1,400 fathoms might have been expected. It may be here remarked that on examining the dried specimen some time afterwards it was found to have changed colour, and upon a preliminary microscopical inspection it showed a broken up, fragmentary deposit of sand, diatomaciæ, and globigerinæ.

Now that the ground was cleared, all cable recovered, and a length amply sufficient spliced up for running out, the next thing to be done was to join and splice up this cable on board to the arm of the main South American cable buoyed off St. Louis, and pay out south to the position of the temporary junction between the cable from Fernando and the St. Louis portion of the St. Louis-Yof Bay coast cable.

On Thursday, the 9th May, a lull in the usually fresh breeze, and a diminution of the heavy swell, permitted ship to be taken

up to the buoy on the cable end, and in due course the cable was got on board. Mr. Benest

Electrical tests on this portion now disclosed a fault towards St. Louis, but not pronounced enough to localise with sufficient exactitude by tests from the ship only. To pick up towards St. Louis would have been inconvenient; so, as the position of the fault was uncertain, and the weather continued to improve, it was decided to join up this end with cable in tank and pay out south, localising the fault afterwards. To this end it was proposed by the chief electrician, Mr. J. Rymer Jones (after laying cable to position), to get the bight of the other cable up and cut it, using the portion of the St. Louis-Yof coast cable to St. Louis as one side of a loop, while the portion of the St. Louis-Fernando cable just laid served as the other half. Loop tests would then be carried out on board to localise the fault in the St. Louis end of the South American cable, and Fernando would afterwards be joined through to St. Louis as before.

The work of splicing, slipping cable from forward aft, and paying out towards position of buoy on the bight south was carried out successfully; but, as wind and sea had again increased during the day, it was found impracticable to approach the buoy, so ship was steered close past it to about one mile south, and the cable just laid from the St. Louis end buoyed at 7.30 p.m.

Early in the morning of Friday, 10th May, this buoy was picked up, the cable raised, and 2 miles of it hove in and coiled down in main tank. Then the ship was steamed towards the buoy on the bight, paying out cable. On reaching the buoy it was picked up, the bight of cable raised and taken on board, cut, and testing proceeded with to localise the fault in the St. Louis portion, after five hours' work in getting the cables clear, as they crossed and re-crossed at the bow, and a fresh breeze with some sea and swell obtaining the while.

The cable was now joined up through the same circuit as before, after an interruption of telegraphic communication of under four hours, the bight being again lowered to the bottom with a wire buoy rope, and buoyed.

The "Dacia" arrived at Dakar that night—11th. On the

Mr Benest. following morning an electrician, with a jointer and three cable hands, were despatched to St. Louis by rail to cut out fault on beach, should it be get-at-able, and put in a piece of new cable.

Meanwhile, the superintendent at St. Louis was instructed, through the Government telegraph line from Dakar, to make a first cut at 12 feet seaward of cable hut, and then test; then, if fault should be still in the cable, to cut again further seaward at high-water mark, and test again to the sealed end. Results of the loop tests on the St. Louis portion of the main cable located the fault on the beach near the cable house, which was afterwards proved to be the case.

Instructions were now wired to St. Louis to commence watch upon the St. Louis-Fernando cable to the sealed end at 10 a.m. on Tuesday, 14th May.

While *en route* for this position the ship was detained for the purpose of making good the Yof (Cape Verde) end of the coast cable, a portion of which was found to be badly worn. Thi portion was taken out, and, the ground being cleared, ship was then set on for the aforesaid end of the St. Louis-Fernando cable proper. This end being taken up, about a mile of it was hove in, and the ship then headed for the buoy on the bight of the (*pro tem.*) St. Louis-Fernando cable, paying out cable from the bow. The buoy was reached and slipped just before darkness set in; the bight was raised, taken in board, cut, and Fernando called, but, owing to some misconception of orders transmitted through Pernambuco, over an hour elapsed before Fernando answered the call. Tests were then carried out, and, cable being found perfect electrically, the joint was at once started between the two portions of the St. Louis-Fernando cable. The splice was finished about midnight, and the bight slipped at 1 a.m. on 15th May, 1895; thus completing a very interesting, if arduous, piece of work.

The length of the St. Louis-Fernando Noronha section, on the completion of this, the second, repair, shows a decrease of slightly more than 10¼ miles; the length of the section after the repair of 1893 being ... ... 1,728·67 N.M.

After repair of 1895	1,718·40	„
Decrease in length	10·27	„

The length of cable recovered during repair of 1895— Mr. Beneest.

On Fernando side of break	33·004	N.M.
„ St. Louis „ „	79·209	„
Total	112·213	„
Recovered cable relaid	56·37	N.M.
New cable laid	47·27	„
Total cable laid and relaid in repair ...	103·64	„

The electrical conditions of the cable as a whole, according to careful and repeated tests in each direction, may be considered unaltered, and its mechanical state sound.

A somewhat detailed account of the cable operations has been given, as many members present may not, perhaps, be familiar with the method of carrying out work of this nature on board a cable steamer.

Mention has been made of yellow fogs. This is a very curious phenomenon, coating the ship and rigging in a few hours with a fine impalpable dust. One way to collect this dust is to cut the fibres from outside the manila ropes and extract the sand from these.

But a better plan, as suggested by Mr. Warren, would be to use a clean glass, or ebonite tray, smeared with glycerine, wash out the deposit with distilled water into a bottle and seal it up.

A description of this atmospheric dust is given by the late Charles Darwin, F.R.S., in his book "The Voyage of "H.M.S. 'Beagle' round the World," which the writer hopes to add to the interest of this subject by repeating.

It was while the "Beagle" was in the neighbourhood of the Cape de Verde Islands. Mr. Darwin says :—

"Generally the atmosphere is hazy, and this is caused by the "falling of impalpably fine dust.

"The morning before we anchored at Porto Praya I collected "some of this brown-coloured fine dust, which appeared to have "been filtered from the wind by the gauze of the vane at the "mast-head.

Mr. Benest. "Professor Ehrenberg finds that this dust consists in great part "of infusoria with silicious shields, and of the silicious tissue of "plants. In five packets which I sent him, he has ascertained no "less than sixty organic forms. The infusoria, with the exception "of two marine species, are all inhabitants of fresh water. I have "found no less than fifteen different accounts of dust having fallen "on vessels when far out in the Atlantic. From the direction of "the wind whenever it has fallen, and from its having always fallen "during those months when the harmattan is known to raise "clouds of dust high into the atmosphere, we may feel sure that it "all comes from Africa. It is, however, a very singular fact that, "although Professor Ehrenberg knows many species of infusoria "peculiar to Africa, he finds none of these in the dust which I sent "him; on the other hand, he finds in it two species which hitherto "he knows as living only in South America. The dust falls in such "quantities as to dirty everything on board, and to hurt peoples' "eyes; vessels even have run on shore owing to the obscurity of "the atmosphere. It has often fallen on ships when several "hundred, and even more than a thousand, miles from the coast of "Africa, and at points sixteen hundred miles distant in a north and "south direction. In some dust which was collected on a vessel "three hundred miles from the land, I was much surprised to find "particles of stone above the thousandth of an inch square mixed "with finer matter. After this fact one need not be surprised at "the diffusion of the far lighter and smaller sporules of the "cryptogamic plants."

The results of a recent microscopic examination at Silvertown, by Mr. Warren, of some of this fog dust collected on board the "Dacia," prove that particles of some mineral substance nearly the thirtieth of an inch square are present. Some slides that Mr. Chapman, F.R.M.S., has kindly examined are, he says, of much interest, and consist mainly of volcanic dust, with traces of diatoms, chiefly fresh-water.

Slide A.—Sponge-spicules; olivine (?), felspar, and pumice fragments.

" *B.*—Spore-like bodies, spherical, pale brown, sometimes with a darker cortex, often broken; sponge-spicules; augite, olivine, felspar, pumice.

Slide C.—Quartz, felspar, augite. Mr. Benest.

„ *D*—Quartz, hornblende.

„ *E.*—Fragments of diatom frustules, chiefly of fresh-water species, as *Surirella bifrous.*

Taking a general survey of the soundings over the entire area covered by the repairs, it will be seen that the bottom gradients are easy.

At the position of the break of 1893, in 1,220 fathoms, all the soundings around, with one exception, show a gradual descent towards it. From the S.S.W. the grade is about 1 in 21—an approximate angle of slope of 2° 50′. From the south-eastward the grade is about 1 in 7—an approximate angle of slope of 8°; this is the sharpest slope met with. From the eastward towards the 1,220-fathom spot the grade is about 1 in 8½—an approximate angle of slope of 7°. The exception is between the depth of 1,280 fathoms and the position of the break, where the slope is reversed, showing a grade of about 1 in 17—an approximate angle of 3° 30′; but this is only a slight dip in the huge furrow which may be conceived to exist here. This furrow, or depression, in the ocean floor is shown in the contour lines of the chart now before you (Chart 2, Plate 3).

At the position of the break of 1895, in 1,574 fathoms, the gradients of the sea bottom sloping towards it from the S.W. round by S., and S.E. to N., do not in any one case exceed 1 in 26—an approximate angle of slope of 2° 30′. The slope of the nearest profile line, A to B, is but 1 in 34—an approximate angle of 1° 30′.

While picking up the cable towards the break during both repairs of 1893 and 1895, when within 2 to 3 miles of the end on either side, the strain indicated on the dynamometer increased from 2 to 5, and to 6 tons. It is therefore only natural to suppose that the cable was embedded upon each side of the furrow.

Considering the very slight grades, comparatively speaking, generally obtaining over the ground, it might be presumed that, whatever the disturbing influences might be, they would be purely local. If this be so, it is remarkable that similar conditions were met with, and that a violent destructive effect should occur at two spots some 17 miles distant, with a difference in depth of from

Mr. Benest 200 to 300 fathoms, and coinciding approximately with the direction of a profile line from the coast.

Following the contour lines towards Cape Verde, clear evidence is shown of the gully formation, with a depth of over 700 fathoms at the mouth of it. The actual depth from a flying sounding taken while paying out the St. Louis portion of the main cable was 675 fathoms in latitude 15° 11′ N., longitude 17° 27′ W.

The depth nearer in to the coast is shown to be 531 fathoms. To the N. by W. of this position is a sounding showing 112 fathoms at a distance of 1¾ miles, and at 1 mile S.S.E. a depth of 115 fathoms.

If the profile line from A to B is prolonged right away to the coast, it will be seen that it will nearly cut these soundings of 675 fathoms and 531 fathoms.

It may therefore be said that this gully formation extends to within a few miles of the shore, but, judging by the period—over 12 years—during which the coast cable has lain across here in 700 fathoms in perfect safety, the diverted course of the South American cable being now alongside it, it would appear probable that these two cables may be lying upon a ledge above, and overhanging, a possible submarine river outlet; so that it might be conceived that a vast mass of mud and detritus from this source may flow, however slowly, along a time-worn furrow on the sea bottom.

A dotted line is to be noted on the chart which may be taken to indicate a probable channel way, or submarine ditch, that may be imagined at some remote period to have been grooved out by the constant action of a rush of water from a river outlet. This outlet may have been submarine or at sea level, in connection with the lagoons situate as shown, which in turn were connected with a great inland river. The Admiralty charts, and those of the French Depôt Général de la Marine, give indications in support of such a supposition.

A very remarkable phenomenon strongly favouring the theory that a submarine river outfall now exists was witnessed during the afternoon of the 23rd April, 1895, while grappling for the coast cable, in position latitude 15° 0′ N., longitude 17° 31′ W., 13 miles from the coast.

The ship was gradually surrounded by great quantities of vegetable growth, having the appearance of river weed—a whitish worm-like weed, the roots of which were similar to some that had been met with previously on a soft mud bottom to the south-westward of the Guadalquiver River. There were also birds' feathers, pieces of orange peel, whole and broken gourds, scraps of carpet, pieces of driftwood, small branches, &c.; and the colour of the sea had changed to a dirty brownish green, indicative of the presence of fresh (river) water. On the following morning all this had disappeared, and the sea had regained its usual tint of a pale green. Mr. Benest.

The nearest river outlet is that of the Senegal, 75 miles to the north-east from this position, and it would appear most unlikely that such flotsam as pieces of carpet could have been carried by the coast current, which sets to the S.S.W., to so great a distance.

No recurrence of this phenomenon took place during the four weeks that were spent in carrying out cable work in the neighbourhood; nor had such a thing been noticed before, so far as records go.

If the coast current had brought these masses of weed and refuse of human habitation out of the Senegal River, it would have been a more or less constant and familiar appearance, as would also the colour of the water; but the discoloration of the sea surface, with the accompaniment above described, would appear to be due to a sudden outburst somewhere in the vicinity, below sea level.

While upon the subject of submarine gullies, and of the probability of their formation by river outlets, whether at or below sea level, it may be mentioned that many subterranean rivers are supposed, with good reason, to exist. These have their outlets in some cases in the form of artesian wells.

Off Pescadores Point, on the coast of Peru, and off the Rovuma River, on the East Coast of Africa, similar conditions have been met with in repairs to cables. At the latter place much trouble had been experienced with the cable between Mozambique and Zanzibar, and the conclusion arrived at was that the cause was

Mr. Benest. fresh water making its way to the surface of the bottom, disturbing the level and fracturing the cable.

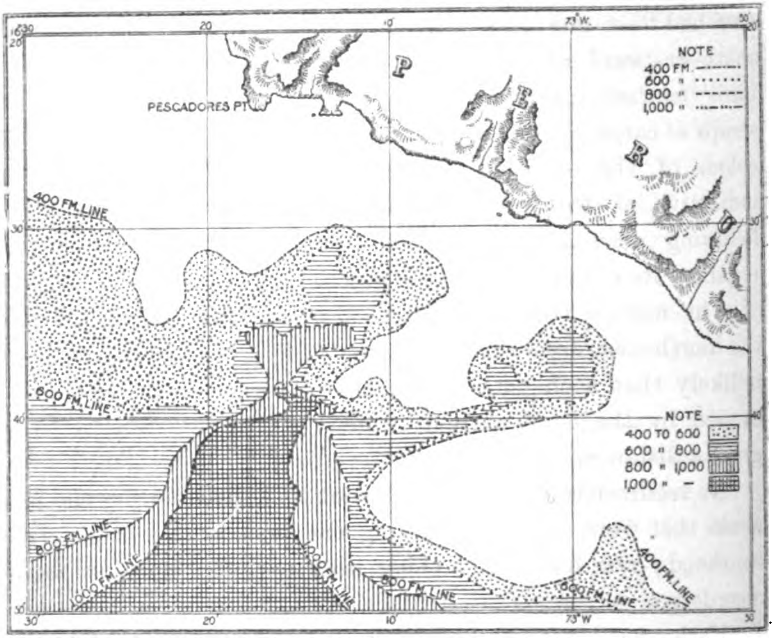


FIG. 5.

Immense masses of mud are in continual movement off the Congo River, and extend to some hundreds of miles to the westward; but here is a very deep river debouching right into the sea, and scouring out a channel hundreds of fathoms in depth.

The West African Telegraph Company's cable between Loanda and the island of Sao Thomé has been broken seven or eight times since its laying in 1886, and some hundreds of miles of cable have been used in repairs.

Some valuable remarks upon these and other gully formations may be seen in Mr. E. March Webb's Report upon the Congo Repairs in 1887, printed and bound in book form, a copy of which will be found in the Institution Library. An extract from an article on "Land Slopes separating Continents, and Ocean "Basins," which appeared in the *Scottish Geographical Magazine*

for May, 1887, by Mr. J. Y. Buchanan, F.R.S., is quoted by Mr. Webb, and, as this refers to the Guinea coast, it will contribute to the interest of this paper to repeat it here:— Mr. Benest.

"Along the Guinea coast as far as Cape St. Paul, the 100-"fathom line is at an average distance of 15 miles from the shore; "and on the south-west coast, between the Niger and the Congo, "it is from 30 to 40 miles from the shore. At these localities "there are remarkable exceptions. Off Grand Bassam the 100-"fathom line approaches within a quarter of a mile of the shore, "and the curving in shorewards of the contour lines produces the "very remarkable submarine gully known by the name of the "'Bottomless Pit.' From the 'Buccaneer' five lines of "soundings were run across it at distances of 2 miles between "the lines. At 1 mile from the shore the width of the gully "is under a mile, with a depth of 150 fathoms; at 8 miles "from the shore its width is 1½ miles, with a maximum observed "depth of 327 fathoms; while 2 miles further seaward its "width has increased to 4 miles, with a maximum observed "depth of 452 fathoms. The accompanying sketch plan of the "contour lines gives a better idea of this extraordinary feature "than can be conveyed by description. The bottom consisted "everywhere of a soft dark-coloured mud, and the slopes of the "sides averaged in many places 2,000 feet per mile."

The writer has been favoured with the views of the President on this subject, which Sir Henry Mance will doubtless give to the meeting himself. The illustration before you is an enlargement of his rough sketch, which explains itself. I hope that the President will pardon me for quoting some of his words, but they are so singularly *apropos* it would be a pity not to. He says: "Now, without giving these submarine fresh-water streams the "dignity of calling them rivers, we may easily imagine (in fact, "we know to a certainty they exist) streams of water making "their way for many miles out to sea before breaking ground."

It is said that many rivers in Australia run underground, and in a paper read in July last year before the Queensland Royal Society by Mr. R. L. Jack, the Government Geologist, dealing with the question of submarine leakage of artesian water in

Mr Benest. Australia, it was shown that an abundant supply of artesian water could be tapped by bores sunk in the dry interior. The water is held in porous strata underlying impermeable strata. The outcrops of the porous strata have been to some extent mapped, and

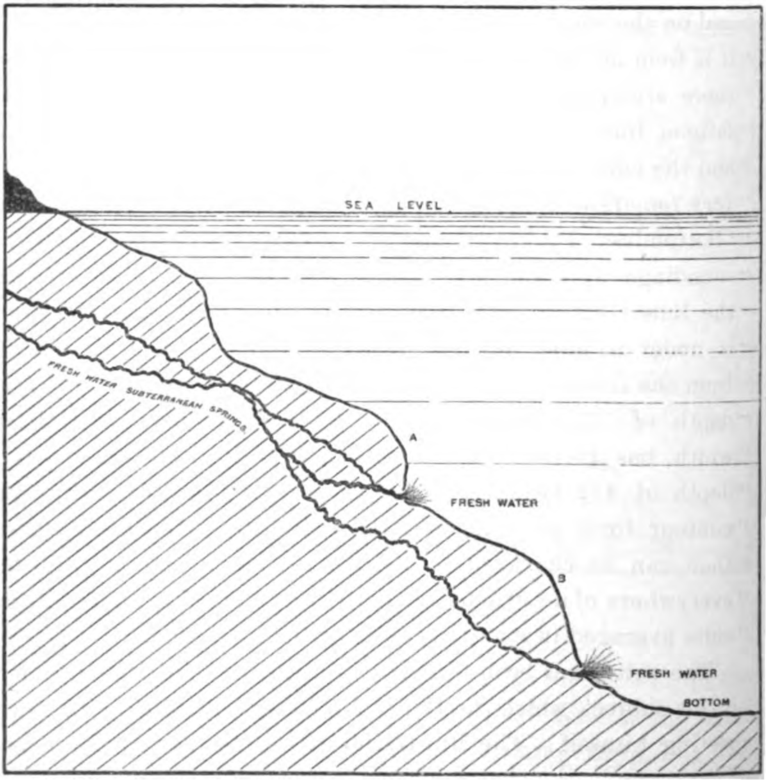


FIG. 6.

are found at altitudes sufficient to give a "head" capable of forcing the water to the surface in the lowlands of the West. Outcrops of porous rock have been found on the eastern side of the interior of the island continent, at a distance of hundreds of miles, and these absorb the flood waters passing over them along the creek and river beds, and carry them down beneath the more impermeable rocks forming the surface of vast areas in the interior.

The Upper Flinders River when crossing in deep gorges the

beds of porous rock rapidly dwindles away, enormous quantities of water being absorbed by these bibulous rocks. Mr. Benest.

The question arises, Where does the draining out take place? The outflow on the land is not sufficient to account for the loss. The rocks, it is concluded, must outcrop somewhere below sea level, and the constant pressure of water from the head sources on land results in a steady outflow of fresh water at this lower level.

Far out in the Gulf of Carpentaria, beyond the influence of flood waters from rivers that run into the gulf, fresh water, it is said, can be drawn up in a bucket. Rocks at a depth of over 1,900 feet close to the edge of the gulf contain artesian water. Off the Victorian coast, it has been said, fresh water may be obtained from a so-called spring out at sea. In both these cases it seems very probable that a portion of the great quantity of water absorbed by the porous strata may eventually find its way out beneath sea level.

In order to obtain some evidence as to whether shore deposits, or may be even mineral substances, were carried out into deep water by the agency of a submarine river outfall, the writer has, through the friendly offices of Mr. M. H. Gray and Mr. Rudler, both of the Royal School of Mines and of the Royal Geological Society, been favoured with the assistance of Mr. Fred. Chapman, A.L.S., F.R.M.S. Mr. Chapman's report upon a most careful microscopical analysis of several specimens of the material composing the sea bottom at and near the locality of the breaks and in the neighbourhood of Senegal is appended to this paper, to which the writer feels it is a most valuable contribution. For the much-esteemed help of Dr. W. F. Hume, A.R.S.M., F.G.S., as well, in the elaboration of Mr. Chapman's notes, the thanks of the writer are cordially tendered.

A brief reference to Mr. Chapman's report will make perfectly clear to the members present the highly interesting nature and composition of the muds of the sea bottom in the vicinity of Cape Verde.

As to which of these deposits, animal or mineral, have been carried out by strong submarine currents it would be perhaps difficult to determine.

Mr. Beneat. Regarding animal remains, it will have been remarked that sponge-spicules are present in nearly all of the specimens; in some these are both broken and perfect. Now, in the perfect state, the presence of these is indicative of shallow water—100 fathoms or so. In the broken state, it would appear as though they had been subjected to current action, or other rough treatment: this latter may have been the voracity of fishes that had probably swallowed them in shallow depths, and had ejected the imperfectly digested remains in water of greater depth, where they have settled to the bottom and now form part of it. The fact of their being found at these depths in a perfect state would seem to show that submarine currents *had* been the cause of their being carried, literally, out of their proper depth.

As regards the mineral deposit, it is interesting and significant to note that in Mr. Chapman's report he says: "The minerals "observed are in nearly all instances such as result from the "disintegration of *drifted fragments* of the volcanic rocks of the "neighbourhood."

This brings to mind the probability of volcanic disturbance having been the cause of the rupture to the cable. The Atlantic Ocean North and South, from Iceland to the Cape of Good Hope, is included in the region of the Western Hemisphere subject to earthquake disturbances; the Azores, the Canary Islands, and the Cape Verde Islands being centres where shocks may be experienced of a severity to cause great damage. In "The Realm "of Nature," by Dr. H. R. Mill, it is stated that "many geologists "believe that sea water filtering through the bed of the ocean, or "buried to a great depth in the lower layers of terriginous "deposits, causes explosions in the intensely heated regions "below, and that all earthquakes originate from this cause."

It is possible that sea water filtering through the sea bottom into a moderately heated portion of the earth may cause explosions of a minor kind that would cause local disturbance, and an alteration to the features of the surface floor.

This might probably cause great upheavals of mud, and displacements of the sea bed, to the extent of many thousands of tons at a time—more than sufficient to rupture a telegraph cable were it fifty times the strength.

Before diverting the St. Louis-Fernando cable to the westward off Cape Verde on the occasion of the first repair in 1893, it was well considered that two ways presented themselves of so doing—either up the bank to the eastward, or over a comparatively flat part of the ocean floor to the westward, into deeper water. By carrying the cable directly up the bank, the possibility of its becoming involved in greater masses of slowly moving mud (should this be the correct theory) presented itself; whereas, farther out and in greater depth, it might be inferred that these masses of moving mud would be more distributed, and exert less force upon anything lying across their path. The first-mentioned plan was abandoned in favour of the latter, but this had failed to place the cable out of reach of the destructive influences, for, as here related, it broke down again on the 10th March, 1895—two years and one month from the date of its restoration in February, 1893. Mr. Benest.

Compared with its first brief space of three months' working, this second term was quite a long lease. It is now, as the writer has shown, on its third trial over a route to the eastward, and clear of both the former tracks.

Various conjectures as to the causes of the sudden rupture of the cable in 1893 were at the time expressed; one being that of sunken wrecks, as, in the days of slave-trading with the West Indies, many vessels were destroyed by scuttling and burning, on many parts of the African coast, by British cruisers. A curious condition of the cable near the breaks was the rubbed and chafed appearance of the outer covering *upon one side only*. The outer taping being rubbed away and the sheathing brightened gave the cable an appearance of having surged along, or been drawn under, some hard body under great strain. This may have been caused by smooth rock, or boulders on the bottom.

There is not much more that the writer can say, except to draw attention to a few striking facts in connection with these very sudden cases of interruption.

Firstly.—It will have been noticed that very severe weather had obtained prior to, and at, the time of the first break in 1893.

Secondly.—It will also have been noted that the second

Mr. Benest. interruption occurred during the great storm of March 10th, 1895; this storm having been experienced in the South, as the weather reports from the Canary Islands at the time testified.

Thirdly.—The remarkable climatic conditions, and the disturbed state of the sea surface in the neighbourhood of the break in 1895, together with the unusual phenomenon observed near to the coast in the vicinity of the gully formation during the repair to, and diversion of, the cable on that occasion.

Fourthly.—That a length of cable was recovered under heavy strain for a distance of 2 to 3 miles upon each side of the break in 1893, and also in 1895; the contour lines clearly indicating hollows between higher levels on the bottom at the positions where the ends came up.

Fifthly.—The distance and direction between the position of break of 1893 and that of the break of 1895 coinciding more or less approximately with the profile line A to B, and through that line towards the mouth of the gully formation to which attention has been directed.

Sixthly.—The remarkable difference in the character of the breaks, the one of 1893 being a clean tension break, the one of 1895 showing evidence of violent action.

And, lastly.—The equally remarkable difference in the character of the sea bottom at the position of the two breaks, as disclosed by the microscope; the locality of the 1893 break showing an *abundance* of mineral deposit, and that of the 1895 break a *scarcity*.

No particular cause for these breaks can be insisted upon, because there is no direct evidence in support of any. Nevertheless, there are indications of a character that may encourage discussion, and so help to throw more light upon the origin of this, so far, unaccountable destructive agency on the sea bottom.

To Captain D. Morton and his officers during this, as well as the preceding repair to the Spanish National cable, great credit is due for the careful navigation and excellent seamanship displayed both in connection with the work and in times of difficulty.

In concluding, the writer would here wish to record his deep sense of obligation to the India-Rubber, Gutta-Percha, and

Telegraph Works Company for the kind facilities accorded him towards the preparation of this paper and its illustration, also to those members of the Silvertown staff who have so cordially assisted in its production. Mr. Benest.

APPENDIX I.

NOTES ON SAMPLES OF VOLCANIC MUD AND GLOBIGERINA OOZE FROM NEAR CAPE VERDE—SOUTH AMERICAN CABLE REPAIRS, 1893 AND 1895—BY FREDERICK CHAPMAN, A.L.S., F.R.M.S.

SOUNDING No. 3 D.—1,288 fathoms. Latitude 14° 58′ 1″ N., longitude 18° 18′ W.; 37 miles from Cape Verde. *Globigerina* ooze. *Mineral particles* somewhat numerous for an ooze with *globigerina*. Fragments of *felspar* and *green augite* are frequent, and *chlorite* (?). Also short rectangular or rhombohedral crystals with a strong refraction. *Dolomite* (?)—(see note on Sounding No. 55)—fairly common. Flakes of *glauconite* are frequent, but none noticed exhibiting any regular form as of a cast of an organism. Particles of brown volcanic glass.

Coccoliths and *Rhabdoliths.*

FORAMINIFERA—

Spiroloculina limbata (d'Orbigny), very rare.
Miliolina venusta (Karrer), very rare.
Cornuspira carinata (Costa), very rare.
Bulimina affinis (d'Orbigny), very rare.
„ *pyrula* (d'Orbigny), very rare.
Lagena lœvis (Montagu), very rare.
Uvigerina pygmæa (d'Orbigny), frequent.
Globigerina bulloides (d'Orbigny), var. *triloba* (Reuss), common.
„ *Dutertrei* (d'Orbigny), common.
Orbulina universa (d'Orbigny), frequent.
Pullenia quinqueliba (Reuss), rare.
Truncatulina tenera (Brady), very rare.
„ *Wuellerstorfi* (Schwager).
Pulvinulina Micheliniana (d'Orbigny), very common.

Mr. Benest. SOUNDING NO. 20.—1,574 fathoms. Latitude 15° 16′ 9″ N., longitude 18° 13′ W.; 50 miles from Cape Verde. *Globigerina* ooze. *Mineral contents* are very scarce: *Orthoclase* and *plagioclase;* occasional fragments of *augite*, *dolomite* (?). Fragments of brown glass. Fine washings reveal *coccoliths* and *rhabdoliths;* also broken SPONGE-SPICULES, and RADIOLARIA.

FORAMINIFERA—

Bigenerina capreolus (d'Orbigny), very rare.
Bulimina aculeata (d'Orbigny), very rare.
„ *inflata* (Seguenza), frequent.
Bolivina punctata (d'Orbigny), frequent.
„ *robusta* (Brady), common in fine washings.
Cassidulina subglobosa (Brady), common.
Lagena striata (d'Orbigny), very rare.
Uvigerina pygmæa (d'Orbigny), common.
„ *angulosa* (Williamson), very rare.
Globigerina bulloides (d'Orbigny), frequent.
„ „ var. *triloba* (Reuss), very common.
„ *Dutertrei* (d'Orbigny), frequent.
„ *cretacea* (d'Orbigny), very rare.
„ *rubra* (d'Orbigny), common.
„ *conglobata* (Brady), very common.
Orbulina universa (d'Orbigny), rare.
Pullenia sphæroides (d'Orbigny), very rare.
„ *quinqueloba* (Reuss), very rare.
„ *obliquiloculata* (Parker and Jones), frequent.
Truncatulina Wuellerstorfi (Schwager), frequent
Pulvinulina Hauerii (d'Orbigny), very rare.
„ *Micheliniana* (d'Orbigny), common.
Nonionina umbilicatula (Montagu), very rare.

Also some valves of OSTRACODA.

SOUNDING NO. 30.—1,210 fathoms. Latitude 15° 15′ 8″ N., longitude 17° 54′ 8″ W.; 38 miles from Cape Verde. Volcanic mud. A large proportion of minerals: *Quartz*, *felspar* (*orthoclase*

and *plagioclase*); *tourmaline* frequent in well-defined crystals; Mr. Beneat. *olivine*—fragmentary; *dolomite* (?) in small rhombohedra. Fragments of brown volcanic glass. In the fine washings there are broken and sometimes perfect SPONGE-SPICULES.

RADIOLARIA, frequent.

FORAMINIFERA—

Bolivina punctata (d'Orbigny), frequent.
„ *robusta* (Brady), common.
Chilostomella ovidea (Reuss), very rare.
Globigerina bulloides (d'Orbigny), rare.
„ „ var. *triloba* (Reuss), rare.
„ *cretacea* (d'Orbigny) frequent.
Pulvinulina exigua (Brady), very rare.

SOUNDING No. 31.—1,080 fathoms. Latitude 15° 18′ N., longitude 17° 53′ 4″ W.; 39 miles from Cape Verde. Volcanic mud. *Quartz*, *felspar* (*sanidine*), *olivine*. Occasional crystals of *dolomite* (?), in one instance two *rhombohedra* conjoined. A somewhat large crystal of *zircon*, rounded, but still showing evidence of prism faces combined with pyramid of the opposite order. Fragments of brown glass.

DIATOMS, rare.

RADIOLARIA, rare.

Broken SPONGE-SPICULES, common.

FORAMINIFERA—

Bulimina pyrula (d'Orbigny), very rare.
Globigerina bulloides (d'Orbigny), common.
„ „ var. *triloba* (Reuss), common.
„ *inflata* (d'Orbigny), very rare.
„ *conglobata* (Brady), very rare.
„ *Dutertrei* (d'Orbigny), common.
„ *cretacea* (d'Orbigny), frequent.
„ *rubra* (d'Orbigny), rare.
Orbulina universa (d'Orbigny), common.
Sphæroidina bulloides (d'Orbigny), very rare.
Discorbina Vilardeboana (d'Orbigny), rare.

SOUNDING No. 48.—1,280 fathoms. Latitude 15° 17′ 9″ N., longitude 17° 56′ 5″ W.; 40 miles from Cape Verde. Volcanic

Mr. Benest. mud. *Mineral particles* numerous, consisting of *quartz, olivine, dolomite* (?), *glauconite* in flakes. Fragments of a brown glass.

DIATOMS, occasional and fragmentary.

RADIOLARIA, rare and usually broken.

SPONGE-SPICULES, numerous and broken.

FORAMINIFERA—

Bulimina inflata (Seguenza).
Globigerina bulloides (d'Orbigny), rare.
„ *rubra* (d'Orbigny), very rare.

Also the spine of an ECHINODERM.

SOUNDING No. 49.—1,180 fathoms. Latitude 15° 17′ 8″ N., longitude 17° 55′ 3″ W.; 38¾ miles from Cape Verde. Volcanic mud. The minerals consist of *quartz, felspar, augite, tourmaline, glauconite*, and *dolomite* (?). There are also DIATOMS and broken SPONGE-SPICULES in the finer washings.

FORAMINIFERA—

Bulimina pyrula (d'Orbigny), very rare.
Globigerina bulloides (d'Orbigny), rare.
„ *rubra* (d'Orbigny), rare.
„ *cretacea* (d'Orbigny), rare.
Pulvinulina Menardii (d'Orbigny), rare.

SOUNDING No. 50.—1,080 fathoms. Latitude 15° 17′ 2″ N., longitude 17° 52′ 7″ W.; 37½ miles from Cape Verde. Volcanic mud. Minerals present: *Quartz, sanidine, tourmaline, topaz* (?), *dolomite* (?). Well-defined *glauconite* casts of foraminifera are present, which show stoloniferous connections, and occasionally the form of the foraminifer, whilst many of the latter are seen to be filled with *glauconite*. Highly refractive, irregular grains are abundant in this sample. Some of the blue-green flakes here present may fairly be taken by their optical characters to be chlorite. Also fragments of brown volcanic glass. In finer washings are broken SPONGE-SPICULES and RADIOLARIA.

FORAMINIFERA—

Bulimina inflata (Seguenza), very rare.
Uvigerina pygmæa (d'Orbigny), very rare.

Globigerina bulloides (d'Orbigny), rare. Mr. Benest.
„ „ var. *triloba* (Reuss), rare.
„ *Dutertrei* (d'Orbigny), very rare.
„ *rubra* (d'Orbigny), rare.
Pulvinulina elegans (d'Orbigny), very rare.
„ *Menardii* (d'Orbigny), very rare.
Rotalia Soldanii (d'Orbigny), very rare.

SOUNDING No. 51.—1,200 fathoms. Latitude 15° 16′ 8″ N., longitude 17° 51′ 2″ W.; 36¾ miles from Cape Verde. Volcanic mud. Minerals present: *Quartz*, *sanidine*, *dolomite* (?), and *glauconite*. Also fragments of brown volcanic glass.

DIATOMS, RADIOLARIA, and broken SPONGE-SPICULES.

FORAMINIFERA—

Bulimina inflata (Seguenza), very rare.
„ *ovata* (d'Orbigny), very rare.
Globigerina bulloides (d'Orbigny), very rare.
„ *rubra* (d'Orbigny), very rare.
„ *Dutertrei* (d'Orbigny), very rare.
Pulvinulina canariensis (d'Orbigny), very rare.

SOUNDING No. 55.—1,470 fathoms. Latitude 15° 13′ N., longitude 18° 17′ 9″ W.; 52¼ miles from Cape Verde. Volcanic mud. The minerals found in this sample were *quartz*, *sanidine* (rare), and *tourmaline*. "*Dolomite* (?)—(not satisfactorily deter-"minable); cross-section extinction straight across angles; "apparently in rectangular blocks, no cleavage, rather high "polarisation colours, at times numerous enclosures; extinction "straight, parallel to long axis, no cleavages seen."—W. F. H.

Also some brown volcanic glass.

Coccoliths in the fine washings.

Broken SPONGE-SPICULES are common also in the finer washings.

FORAMINIFERA—

Globigerina bulloides (d'Orbigny), frequent.
„ *Dutertrei* (d'Orbigny), very rare.
„ *rubra* (d'Orbigny), rare.
Pulvinulina Menardii (d'Orbigny), rare.

Also a valve of an OSTRACOD.

Mr. Beneat. SOUNDING No. 56.—1,515 fathoms. Latitude 15° 17′ 7″ N., longitude 18° 16′ W.; 53½ miles from Cape Verde. Volcanic mud. *Minerals* somewhat rare and minute. *Quartz*, *felspar*, *olivine*. Also fragments of brown volcanic glass.

DIATOMS and broken SPONGE-SPICULES in the finer washings.

FORAMINIFERA—

Globigerina bulloides (d'Orbigny), rare.

„ „ var. *triloba* (Reuss), rare.

„ *Dutertrei* (d'Orbigny).

„ *rubra* (d'Orbigny).

Discorbina Vilardeboana (d'Orbigny), very rare.

Pulvinulina Menardii (d'Orbigny), rare.

SOUNDING No. 47.—1,220 fathoms. Latitude 15° 18′ 2″ N., longitude 17° 57′ 5″ W.; 39½ miles from Cape Verde (all distances from Almadie Point). Volcanic mud. *Mineral contents: Quartz*, abundant in angular grains and flakes; *augite*, *tourmaline*, rhombohedra of *dolomite* (?). Fragments of brown volcanic glass.

SPONGE-SPICULES, broken.

FORAMINIFERA (in fine washings)—

Bolivina robusta (Brady).

Globigerina bulloides (d'Orbigny).

From a consideration of the foregoing details, the volcanic muds appear to extend to a somewhat greater depth (1,515 fathoms) at this locality than was found to be the case in the "Challenger" soundings (1,150 fathoms).* We also have here indications of glauconite in the volcanic muds down to 1,280 fathoms, but at this depth it occurs only in flakes, and very rarely; at a depth of 1,080 fathoms glauconite casts of foraminifera were noticed. The minerals observed are in nearly all instances such as result from the disintegration of drifted fragments of the volcanic rocks of the neighbourhood.

The silicious organisms found in the above deposits are usually in a fragmentary state, which may probably be due to the

* "Deep-Sea Deposits" (Murray and Renard), p. 154.

attrition caused by the movement of the mud by strong sub- Mr. Benest.
marine currents; but the direct evidence in support of this is not very great.

In concluding these notes, I gratefully acknowledge the valuable co-operation of Dr. W. F. Hume, A.R.S.M., F.G.S., in the determination of the minerals observed in the several washings.

LANTERN SLIDES EXHIBITED.

No.	Description.
1.	S.S. "Silvertown."
2.	„ "Silvertown's" main tank.
3.	„ „ paying-out gear.
4.	Types cable S.E. and L.D.S.
5.	„ „ H.I., L.I., and H.D.S.
6.	The Citadel, Fernando Noronha.
7.	The Town, „ „
8.	The Peak, „ „
9.	Shore end landing (distant view).
10.	„ „ „ (near view).
11.	Cable House, Fernando Noronha.
12.	Shore end landing, St. Louis.
13.	S.S. "Dacia."
14.	„ "Dacia's" picking-up gear.
15.	Sketch of cable with sheathing rubbed bare.
16.	Two ends of broken cable.
17.	Paying-out gear, s.s. "Dacia."
18.	"Reina Regente."
19.	S.S. "Dacia" in heavy weather.
20.	Damaged portion of cable.
21.	Broken end of cable, St. Louis side (1895).
22.	Desert dust.
23.	„ „
24.	„ „
25.	„ „
26.	Contour lines off Cape Verde.
27.	„ „ „ Pescadores Point.
28.	Fresh-water submarine spring.
	Slides showing foraminifera.
	Ornamental design.

Mr. Benest.

APPENDIX II.

Detailed Particulars of Soundings.

Profile Line of Sounding from the Westward towards Cape Verde.

Section A to B.

Commencing in latitude 15° 21′·2 N., longitude 18° 9′·5 W.

No.		Depth.		
1	...	1,340 fathoms	...	No specimen.
2	...	1,450 "	...	Soft green mud.
3	...	1,413 "	...	Stiff green mud.
4	...	1,200 "	...	Soft green mud.
5	...	1,160 "	...	" "
6	...	1,100 "	...	" "
7	...	990 "	..	Stiff green mud.
8	...	910 "	...	" "
9	...	835 "	..	Soft green mud.

Ending in lat. 15° 9′·3 N., long. 17° 45′·4 W.

647 fathoms. Lat. 15° 7′·30 N., long. 17° 35′·2 W.

675 fathoms. Lat. 15° 2′·3 N., long. 17° 27′·0 W.

531 fathoms. Lat. 15° 0′·0 N., long. 17° 21′·5 W.

54 fathoms. Lat. 14° 58′·0 N., long. 17° 11′·5 W.

Land.

Profile Line of Soundings from Eastward across Line of Cable. Mr. Benest.

SECTION C TO D.

Commencing in latitude 15° 6′·6 N., longitude 17° 50′·9 W.

No.		Depth.		
10	...	880 fathoms	...	Soft green mud.
11	...	960 „	...	Stiff green mud.
12	...	1,200 „	...	„ „
13	...	1,210 „	...	„ „
14	...	1,340 „	...	„ „
15	...	1,450 „	...	„ „
16	...	1,500 „	...	Stiff grey mud.

Ending in latitude 15° 7′·7 N., longitude 18° 17′·7 W.

Completion of Profile C to D.

Profile from the Westward towards Cape Verde.

SECTION E TO F.

Commencing in latitude 15° 5′·3 N., longitude 18° 12′·4 W.

No.		Depth.		
17	...	1,420 fathoms	...	Soft green mud.
18	...	1,370 „	...	„ „
19	...	1,300 „	...	Stiff green mud.
20	...	1,210 „	...	„ „
21	...	1,100 „	...	„ „
22	...	940 „	...	„ „

Ending in latitude 14° 53′·9 N., longitude 17° 56′·8 W.

Completion of Profile E to F.

A Line of Soundings from the South-West along the Line of Cable.

SECTION G TO H.

Commencing in latitude 14° 50′·7 N., longitude 18° 10′·9 W.

No.		Depth.		
23	...	1,380 fathoms	...	No specimen.
24	...	1,350 „	...	Stiff green mud.
25	...	1,305 „	...	No specimen.
26	...	1,200 „	...	Stiff green mud.
27	...	1,190 „	...	„ „

Mr. Beneat.

No.		Depth.		
28	...	1,150 fathoms	...	Soft green mud.
29	...	1,120 "	...	Very soft grey mud.
30	...	1,210 "	...	" "
31	...	1,080 "	...	" "
32	...	1,080 "	...	" "
33	...	990 "	...	Soft grey mud.
34	...	880 "	...	Very stiff grey mud.
35	...	810 "	...	" "
36	...	790 "	...	Stiff grey mud.
37	...	790 "	...	" "
38	...	840 "	...	" "
39	...	860 "	...	Green-grey mud.

Ending in latitude 15° 33′·0 N., longitude 17° 41′·6 W.

Completion of line, from S.W. to N.E., over a distance of 50 miles.

Profile Line of Soundings across Line of Cable.

Commencing in latitude 15° 33′·1 N., longitude 17° 47′·7 W.

No.		Depth.		
41	...	990 fathoms	...	Stiff green mud.
42	...	880 "	...	" "
43	...	800 "	...	Soft grey mud and sand.
44	...	785 "	...	Stiff mud.
45	...	850 "	...	Soft green mud.

Ending in latitude 15° 27′·0 N., longitude 17° 39′·6 W.

Completion of Profile No. 5.

(Not illustrated by section.)

Soundings to Eastward across Line of Cable.

Commenced immediately the St. Louis side of the broken cable (end) came on board (1893).

Latitude 15° 18′·2 N., longitude 17° 57′·5 W.

No.		Depth.		
47	...	1,220 fathoms	...	Soft green mud.
48	...	1,280 "	...	Soft grey mud.
49	...	1,180 "	...	Soft green mud.
50	...	1,080 "	...	Soft grey mud.

Note.—This specimen much softer than preceding ones.

51	...	1,200 fathoms	...	Very soft green mud.

(Not illustrated by section.)

Mr. Benest.

Soundings along course of Diversion to the Westward of the original Line (1893).

Commencing in latitude 14° 57′·2, longitude 18° 13′·5.

No.		Depth.		
52	...	1,380 fathoms	...	Soft grey mud and green ooze.
53	...	1,320 ,,	...	Soft green mud and green ooze.
54	...	1,390 ,,	..	Soft grey mud and green ooze and sand.
55	...	1,470 ,,	...	Light green mud and green ooze.

Note.—This specimen has a yellowish tinge on top.

56	...	1,515 fathoms	...	Yellow and green mud and ooze.
57	...	1,460 ,,	...	Green mud.
58	...	1,320 ,,	...	,,
59	...	1,210 ,,	...	Soft green mud.
60	...	1,185 ,,	...	,, ,,
61	...	960 ,,	...	Grey mud.
62	...	862 ,,	...	,,
63	...	870 ,,		Green mud.

Completed near to buoy on St. Louis end of cable. Latitude 15° 34′·2 N., longitude 17° 42′·0 W.

(Not illustrated.)

APPENDIX III.

Lengths, Depths, and Temperatures (St. Louis End).

Table A.

Lengths, Depths, and observed Bottom Temperatures from Cut North of Break to St. Louis.

N.M.		Fathoms.		Degrees Fahr.	
6	...	7	...	66·0	South end, St. Louis.
5	...	25	...	62·0	
16	...	260	...	53·0	
13·8	...	500	...	47·0	
4	...	700	...	41·0	
28	...	800	...	40·5	
15	...	880	...	40·3	Buoyed end.
87·8		565		49·9	

Mr. Benest.

Table B.

Lengths, Depths, and observed Bottom Temperatures along the route of Cable laid during Repairs between the Cut on Fernando Side of Break and the Cut on St. Louis Side.

N.M.		Fathoms.		Degrees Fahr.	
4·6	...	860	...	40·3	St. Louis end.
6	...	970	...	39·7	
18	...	1,175	...	38·8	
8	...	1,323	...	37·4	
16	...	1,395	...	37·0	
5	...	1,425	...	37·0	
16	...	1,340	...	37·4	Fernando end.
73·6		1,258	...	38·5	

Mr. Buchanan.

Mr. J. Y. BUCHANAN, F.R.S.: I have listened with very great pleasure and instruction to the paper of Mr. Benest on the remarkable breaks which took place in the South American cable on two occasions—one very shortly after the laying of the cable, and the other after it had been repaired, and to the best of belief had been laid in a bed on which it was likely to find safe and lasting rest. This expectation, however, was not borne out, and the cable broke again, and was repaired in the way and under the difficulties which have been set forth in the paper. The great value, in my opinion, of papers of this kind is the addition they make to our knowledge of an almost unknown region—the bottom of the ocean. This was absolutely unknown 25 years ago, and when we reflect that it accounts for more than three-fourths of the surface of the globe, it is not too much to expect that it will take some few years before we know very much about it. The only way by which we can come to any likely conclusion as to what goes on at the bottom of the sea is by working out carefully, as has been done in this case, the accidents which occur to submarine cables. In deep-sea investigations we sound and we dredge. The sounding lead only remains a few minutes down at the bottom, and comes up with a sample of mud. The dredge

may remain a few hours, and comes up with a sample of the sea-bottom, and of the animals and—if there are vegetables—vegetables that are there. But we have nothing remaining down there for any length of time which can give us an idea of what is actually taking place at the bottom except the submarine cable. A submarine cable is an exceedingly expensive article, and is therefore worth looking after very carefully. When an interruption happens to it, it causes an enormous amount of inconvenience to a very large number of people, and that is an additional reason for looking after it. But it might be picked up, repaired, and put down again, and nothing more said about it, and we should be very little the wiser. When, however, attention is paid to every particular—to the position of the break, the circumstances attending the grappling and picking up of the ends, especially the teachings of the dynamometer, to the features of the broken ends, and the character of the bottom on which it lies—and when all the circumstances are duly chronicled, we begin to get material for forming an idea of what may possibly go on at the depths of the sea. For my part, I can see no other way of arriving at this knowledge. It is, of course, a very slow way, and full knowledge of what goes on at the bottom of the sea will probably be reserved for posterity. But if we cannot enjoy the knowledge ourselves, that is no reason why we should not do our best to procure it for those who come after us, and there is no better way of doing it than by carrying out the investigations in the conscientious and painstaking way in which they have been done in this expedition. I may perhaps say a few words about the purely oceanographic particulars which have been brought out in the paper, which are of great interest, and go to increase our stock of knowledge on the subject. With regard to submarine gullies, I may be pardoned for saying that the delineation of the particular one shown is not very sharp. A number of soundings are in close juxtaposition, and are of considerable difference of depth. No doubt there is a gully there, but it does not seem to be very steep. These gullies form a remarkable feature of the submarine coasts of the continents, and the two that have been called attention to by Mr. Benest on the coast of Africa are, I suppose, the most

Mr. Buchanan.

Mr Buchanan. remarkable that have been proved to exist—that is, the "Bottomless Pit" and the estuary of the Congo. The one at the mouth of the Congo is a particularly noticeable one, because the deep water, of something like 200 fathoms, stretches right in beyond the coast line, and for some 20 or 25 miles inland. The Congo is one of the greatest rivers in the world, and brings down a very large amount of fresh water, which is generally charged with *débris* of the land; but of course it flows out on the surface, and the fresh water of the Congo cannot be both flowing at the surface and grinding out the bottom. The surface water of the river, although fresh, is not drinkable: the fresh water of the river is mixed with the salt water of the ocean, which is picked up while within its estuary. This indicates that, while there is a fresh water stream running out, there is a salt water stream running in below. If the Congo had a dam put across its mouth at Shark Point, where it passes the border of the continent, reaching down to the bottom, it would fill up with fresh water. If the dam were pierced, then the salt water would run in below and the fresh water would run out above. This goes on quite quietly by the interchange of the salt water and the fresh water, and the consequence is a current which does not, in my opinion, run out at the bottom, but prevents the sediment settling there, and instead of digging a hollow it builds two walls—one on each side. It would occupy too much time and be quite foreign to the purpose of this paper to describe any further the configuration of the bottom of the coast of that part of Africa, which gives evidence of a large building up of sediment; and that sediment, from a telegraph engineer's point of view, was sufficiently interesting from the fact that it was so soft that the sounding weights frequently would not disengage: that is to say, it had the fluidity of water, and there was nothing to catch the sounding rod as distinguished from the weight. We know that all purely marine muds settle down in a state of very considerable consistency, and there is no question whatever of the sounding rod not sticking sufficiently to slip the weights. The "Bottomless Pit" is at a place where no river comes in directly at this moment, but a river comes in behind the lagoons,

and it may be that a similar action has taken place there. It may also be that in former times there has been a river at Yof Bay to produce a similar effect. I hardly think we are justified in supposing that the break in the cables in this case has been caused by a submarine river coming out. The possibility of it is not by any means excluded, because there is a difference in the density of sea water and fresh water. If we put fresh water at 1,000, sea water would be 1,027. If you had 1,000 feet of sea water, it would be counterbalanced by 1,027 feet of fresh water. That might very easily be found, and therefore it might happen; but I think, until we have better evidence of it happening, we ought not to ascribe too much importance to it. I do not know, Sir, that I have any further observations that would interest this meeting, except to repeat that I am very much interested in the paper. Mr. Buchanan.

The PRESIDENT: I see that Admiral Wharton is here, and perhaps he would say a few words on the subject of the paper.

Admiral W. J. L. WHARTON, C.B., F.R.S.: I really do not think, Sir, that I can add very much to the knowledge of the meeting. I came here to listen and to learn, for I take a very great deal of interest in anything that goes on either on the sea or under the sea I quite agree with Mr. Buchanan that it is to the work of the cable companies that we must look for the soundest evidence as to what is going on under the sea. I think it is mere speculation to talk about the causes of these breaks. As I understand Mr. Benest's maps, here the cable in both cases was more or less hung across this gully—I think that is so, Mr. Benest? Admiral Wharton.

Mr. BENEST: Not quite so; I did not mean to convey that idea at all. I simply meant to convey the idea that the cable was embedded on each side of the break. Mr. Benest.

Admiral WHARTON: But it crossed the gully at right angles?

Mr. BENEST: I do not assume there is any gully existing where the cable crossed. The gully comes nearer to the coast. There are differences in level, probably, at the bottom, but to a very slight degree. A study of the contour curves on the map would probably give you a better idea. If you notice the way the contour curves run in towards the gully, you will find that all the lines run from the seaward direction; but out in deep

Mr. Benest. water I imagine the floor is comparatively level—no sharp gully at all.

Admiral Wharton. Admiral WHARTON: I think, then, the break is entirely incomprehensible. I do not believe—as I say, it is only a speculation—I do not believe in the power of the submarine outbreak of fresh water at that depth to move matter very violently along the bottom. I will not deny that water may break out, but I should not expect it would have very much power to move matter. I think it would find its way out in the line of least resistance, which is towards the surface, and I think we must look for some other cause for these very remarkable breaks. It is not very clear to me that the remarkable compression of the cable, as we can see in the piece lying on the table, may not possibly be caused by inequalities on the bottom, or by the mere hauling up; for the strain is very heavy, as shown by the dynamometers, and it is quite possible that that compression might be caused by the act of recovering the cable.

The PRESIDENT: The discussion is adjourned to the 25th March. I have much pleasure in announcing that the scrutineers report the following candidates to have been duly elected:—

Foreign Member:

Xavier Gosselin.

Associates:

Alexander Davidson. | Philip Millais Benest.

Alfred Seymour Hewett.

Students:

Comer Sandys Ball.	Francis R. Gibbons.
Arthur O. Berry.	G. D'A. Meynell.
Edward H. Freeman.	Gilbert A. Pechey.

Menzies A. Stapley.

Adjourned to March 25th.

The Two Hundred and Ninety-ninth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, March 25th, 1897—Sir HENRY MANCE, C.I.E., President, in the Chair.

The minutes of the Ordinary General Meeting held on March 11th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Students to that of Associates—

Gerald C. Allingham.
C. F. H. Bayly.
Frederick W. Dalton.
William D. B. Duddell.
William James Grey.
Reginald C. Harpur.
Gustave J. F. Lemmens.
Richard E. Skipwith.

Messrs. A. G. Newington and Ernest Scott, Associates, were appointed scrutineers of the ballot for new members.

The SECRETARY read the following letter received from Dr. Von Stephan, whose election as an Honorary Member of the Institution was announced at the last meeting:—

BERLIN, W.,
March 15*th*, 1897.

DEAR SIR,—I had the pleasure to receive your kind letter, dated 13th of March, by which I was informed that the Institution of Electrical Engineers has unanimously elected me an Honorary Member.

The honour thus unexpectedly conferred upon me by an Institution whose roll shows the names of so many scientists of world-wide fame has been highly gratifying to me, and I feel proud to have been received into a Society, the members of which have done so much to pave the way I have been treading from

the beginning of my official career, and which I have pursued unflinchingly with the object in view to promote to the best of my ability the telegraphic science as a sure means to establish peace and goodwill between the nations of the earth. If I have succeeded in furthering these ends ever so little—and the testimony coming from so competent a body as your Institution is apt to remove any misgivings on that head—I have not lived in vain.

I therefore accept gratefully the Honorary Membership of the Institution of Electrical Engineers, and beg to ask you to be good enough to make yourself the interpreter of my grateful feelings to all the members of the Institution, and especially to those who presented me.

I likewise wish to tender my heartfelt thanks for the kindly sympathy expressed on account of my present illness. I may add that I am slowly—very slowly—mending. May the Lord grant that your hope for my recovery may be realised!

With kindest regards, I remain, dear Sir,

Yours very sincerely,

Dr. von Stephan.

The Institution of Electrical Engineers:
F. H. Webb, Esq., *Secretary*,
London.

The President: Gentlemen,—Some weeks ago I had the pleasure of announcing to you that the Council had under their consideration the qnestion of increasing the premiums which we offer every year. I am now authorised to inform you that they have decided on making a substantial increase, the particulars of which the Secretary will read to you.

The Secretary: The premiums to be offered annually, commencing with the session 1897–98, are as follows:—

For Papers read before the Institution.—The "Institution "Premium" will be increased from £10 to £25; the "Paris "Electrical Exhibition Premium" will be increased from £5 to £10; the "Fahie Premium" will be increased from £5 to £10.

For "Original Communications," not read, but accepted for publication in the Journal of the Institution, *two Premiums*, viz.:—One of £10; one of £5.

For Papers read at the Students' Meetings, three Premiums, viz.:—One of £10; two of £5 each.

Hitherto papers by Members of the Council have been ineligible for premiums, but this rule has excluded so many valuable and important papers that the Council consider it

advisable to amend it in the following manner, which they believe will equally attain the object for which the original rule was framed by them :—

Amended Rule.—"No member who has contributed a paper "shall be a member of any committee to make recommendations "as regards the award of premiums for papers read during the "session in which he has so contributed a paper."

The PRESIDENT: Gentlemen,—I have been desired by the Council to make a communication to you, which I am sure will be heard with regret by a great many. The announcement I have to make is that of the approaching retirement from the service of the Institution of our Secretary, Mr. F. H. Webb. You will no doubt understand that, as we increase in numbers and the basis of the Society broadens, the duties of the Secretary become very much more arduous. Mr. Webb is in his 73rd year, and approaching that time of life when we may all of us reasonably look forward to a well-earned rest. It must be gratifying to Mr. Webb to feel that as we have increased in numbers he has at the same time been steadily adding to his list of personal friends; for I am sure that every member of this Institution, and more especially those who have come frequently in contact with him, regard our Secretary with the most kindly feelings. But, gentlemen, we must resign ourselves to the inevitable. The Council have thought it only right that when Mr. Webb does leave us at the end of his 20th year of service, in February next, he should, in recognition of his past services, be awarded a liberal pension; no one who is not actually behind the scenes can understand the great amount of work that has to be done during the session. We feel that the man who occupies the office of Secretary should be in every respect as good a man as we can possibly afford. I do not intend this evening to attempt to do justice to Mr. Webb's services to the Institution. That task will no doubt devolve on someone more competent than myself at some later period.

We shall not, I hope, altogether lose the benefit of Mr. Webb's assistance and advice in the future, and that to some extent tempers the regret which we shall all feel in parting with an old

and valuable servant, who has been with the Institution from its younger days, and has witnessed its growth into the important Association which it is at present. I have mentioned the subject to-night because the Council have thought it right to intimate to you that they would be willing to receive applications from those who desire to become Mr. Webb's successor. The matter will be advertised in the technical Press very shortly, but we thought it would do no harm if we mentioned the subject to you to-night. The qualifications which the Council consider necessary will be stated in the advertisement, but I may now say that what we desire to obtain is, not only a man with a liberal education, with some knowledge of French and German, but a gentleman in touch with the profession, and possessed of tact and business capacity. I may also mention that we propose to give a salary commencing at £400 a year. The advertisements will tell you when and to whom the applications should be forwarded. I do not think I have anything more to say on this point, except to express my personal appreciation of the good work which you, Mr. Webb, have done for the Institution.

We will now continue the discussion on Mr. Benest's paper on "Some Repairs to the South American Company's Cable off Cape "Verde in 1893 and 1895."

Mr. Bright. Mr. CHARLES BRIGHT: When recently lecturing to an Imperial Institute audience on "Sixty Years of Submarine Telegraphy," Professor Ayrton remarked, "Happy is the cable which has no "history." Though this was perfectly true from a business point of view, a cable indulging in a certain amount of history serves its purpose in the cause of scientific investigation, and, as we see, to the advantage of our Institution.

It is so long since we have been favoured with a paper connected with submarine telegraphy that, if only in breaking the spell, we should be greatly indebted to Mr. Benest.

The author has given us a succinct account of two important repairing expeditions, but a natural modesty has prevented him mentioning the important part he played in them. It was my privilege to be associated with the first of these expeditions. This—a very speedy and successful affair—was under the control

of Mr. Benest, and I believe that the second repair was Mr. Bright. additionally carried out by him.

In the 1893 expedition I also have a lively recollection of the navigating skill displayed by Captain Anthony Thomson, R.N.R., who put the s.s. "Dacia" so accurately in position that both ends were hooked at the first drag, and very soon after the grapnel had reached bottom.

Those of us who have visited that part of the West Coast of Africa in the latitude of Senegal must cordially agree with the author that it does not lend itself to flowery description. Mr. Benest, however, waxes quite eloquent—not to say poetic—in parts of his paper. His vivid description of the atmosphere in one place, and of a storm in another, are only worthy of a Kingsley, a Marryat, or a Russell.

The author has laid before us certain recorded facts connected with the two expeditions, and, whilst leaving us to draw our own conclusions, he more especially throws out for our consideration and discussion one or two alternative theories explanatory of the phenomena experienced.

In the first place, it is evident that in these regions a submarine cable is liable to become deeply embedded in its soft bottom, and that recovery therefrom is likely to be as tedious and hazardous an operation as Messrs. Siemens Brothers have recently found it in the Amazon River.

It would seem that these breaks to the South American Company's cable in 1893 and 1895 require to be explained in some other way than by an ordinary gully due to a strong outflow from one of the neighbouring rivers. The case is different, in fact, to that of the break in the West African Company's cable off the mouth of the river Congo in 1887, the nature of which was so concisely set forth by Mr. March Webb in the course of his engineering report.

It appears that the cable between St. Louis and Yof Bay has given no sort of trouble, though laid some 12 years ago, and though close to the mouth of the river Senegal. Here the cable rests on a ledge running out from the coast, and in this position appears to be free from disturbance.

Mr. Bright. As I understand the author, he suggests that these interruptions must, therefore, have been brought about by a submarine river outside the ledge or by a sudden outburst due to volcanic or other causes somewhere in the vicinity. The bulk of evidence seems to favour either of the latter views; and they are strengthened by the immunity from further trouble which the South American cable has enjoyed since this part of it was placed on the same platform with the coast cable referred to.

I am, however, unable to follow the author in all the evidence he brings to bear on the question in support of this theory. No doubt it "pans out" all right; but at present I seem to confront the somewhat novel, if not weird, spectacle of large consignments of domestic carpet, orange peel, and birds' feathers issuing from an artesian spring or submarine ditch!

There are, again, one or two sentences in various parts of the paper as to the meaning of which I am in some doubt, and about which I should be glad of a little more enlightenment.

On page 217 these words occur: "The indicated strain fell "to 6 tons when at rest, and afterwards to 5 tons as the slack "cable was drawn along the ground, *thus proving that a suffi-"cient quantity of slack had been laid originally.*" Whilst raising no sort of question as regards the latter fact, I do not follow how the circumstances named would prove anything more than that the slack for a few miles each way was ample; unless the recovery was made quite close to the end, in which case it seems to me the circumstance would imply nothing at all as regards the average slack of the whole cable.

The paper seems to suggest a comparatively high speed for the laying vessel during paying out. It may be questioned, however, whether, under *all* circumstances, such a plan would recommend itself. *Assuming* the required slack is successfully got out, it is also doubtful whether an *increase* of quantity forms a good substitute for going slowly over the ground, especially when approaching the coast at each end.

Mr. Preece—one of our land and submarine telegraph pioneers—was, I believe, the first to call attention, as early as 1860, to

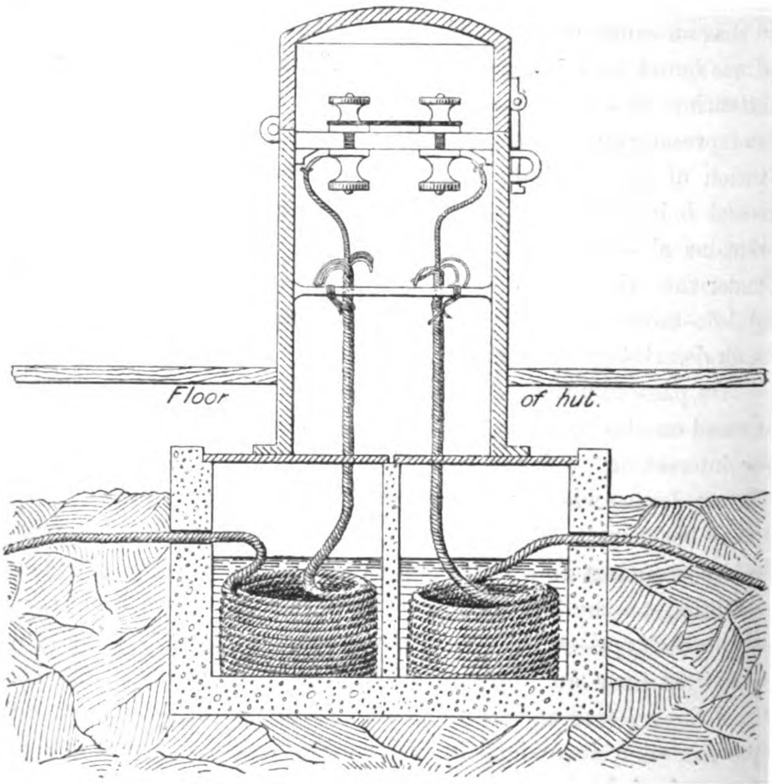
the vital importance of an elaborate submarine survey previous to laying a cable. Mr. Bright.

On page 226 the author says: "It may be here remarked "that on examining the dried specimen some time afterwards it "was found to have changed colour." This induces me to call attention to what I fancy must be the best plan for collecting and preserving specimens of the bottom. By this the preservation of the specimen is much more perfect. The containing vessel being air-tight, the chemical composition of the sample remains absolutely unaltered up to the moment of examination under the microscope. This method is due, I believe, to Captain Wilson-Barker, R.N.R., and as he is here to-night I will refrain from describing it myself.

On page 228 the author describes how the fault was eventually located on the beach 12 feet seaward of the cable hut. It would be interesting to learn from that indomitable fault-tester, Mr. Rymer-Jones, whether this was a fault due to white ants in the sandy beach, or to what cause. With reference to faults in beach cables resulting from organisms in the sand, I should like to call attention to the fact that even where the cable used ashore is of a suitable type—with, say, an outer steel taping—ants and other atrocities have an unpleasant habit of creeping along the core from the exposed ends in the hut. To my mind, the only way to prevent such incursions is to have the ends in the hut brought to an air-tight iron case, as shown by the accompanying figure, the coil of slack cable in circuit outside the hut being placed in a cement tank filled with water. I devised this arrangement some time ago, and fancy it would serve as a reliable safeguard against the above-mentioned very frequent source of trouble.

Going back to page 211, where the author briefly describes the type of cable, he says: "It requires small retarding force in "paying out." This, again, is another of the few sentences which I am unable to follow as it stands, for I scarcely think it will be suggested that a cable is designed to suit the brake power on board the laying vessel. Surely the brake power is designed rather to suit the type of cable, and, if necessary, altered

Mr. Bright accordingly, as has recently been done on several cable ships to meet the requirements of cable-laying in greater depths.



Bright's Terminal System for Cable Ends at Hut.

(This is only a rough sketch for giving a general idea, none of the various parts being proportionately to scale.)

The fact that in 1892 the iron wire used in the sheathing of the light deep-sea type of this cable was guaranteed to bear a strain equivalent to 84 tons per square inch before breaking. whereas now the same can be guaranteed at 90 tons per square inch, serves to illustrate that, though no complete revolution has occurred of late years in submarine telegraphy, steady improvements in the materials have taken place year by year.

The South American cable was, perhaps, mainly interesting in the fact that india-rubber was given a fresh trial for insulating

purposes in the shorter section, a great part of which is in quite deep water. This being so, it would be useful if the author could give us particulars of any experiments which may have been made in connection therewith. It will be remembered that the late Sir William Fairbairn and the late Sir William Siemens obtained materially different results in their experiments on the effect of pressure on india-rubber. The bulk of evidence appears to be in favour of Siemens's results, pointing to a slight decrease in the electrical resistance of rubber under water pressure, but any further enlightenment on the subject would be most valuable just now. Mr. Bright.

For several years we have been periodically scared with suggestions of the supply of gutta-percha coming to an end. This may, of course, actually take place before the much-discussed trans-Pacific cable ever takes a concrete form, considering that schemes for the latter have been mooted ever since the idea was first ventilated, by the late Mr. Cyrus Field, in 1870. This being so, and the electrical advantages of rubber from a working speed point of view being of some value for a great length, it will be obvious (1st) that the behaviour of rubber in deep water is one of great importance, and (2nd) that any individual who could guarantee a rubber-insulated cable equally with one of gutta-percha might be at a great advantage in the matter—if by that time any cables at all are necessary for the purposes of ocean telegraphy!

Since the laying of the cable between Fernando de Noronha and Pernambuco, the Silvertown Company have, I understand, also laid an india-rubber cable in deep water off the coast of Cuba. Beyond these, nothing much has been done in rubber for deep-sea telegraphs since those of Messrs. Hooper.

It has been within my province to observe the comparative behaviour of rubber and gutta-percha, alternately on dry land and in swamps, in the course of a supplementary expedition with which I was encharged in connection with a part of Mr. Benest's work. It is to be hoped, therefore, that I shall not be guilty of over-greediness in asking the author to add to the useful matter he has given us by expanding somewhat as regards the Pernambuco

Mr. Bright. rubber cable and any experiments that may have been made with it.

I should also like to know whether any experiments in the way of leaks at Fernando have been carried out in working the South American Company's line. This would be particularly interesting at the present moment, when various leak experts are busy criticising the embryo "rapid" cable of Professor Silvanus Thompson.

The statement that "the speed of transmission compares "favourably with other transatlantic lines," strikes me as a little too general, if not actually misleading, unless accompanied by further particulars. Presumably this means as regards *manual* transmission only. Most of the modern North Atlantic cables, however, are almost invariably worked automatically. The result on two of them, with much heavier cores than that of the South American cable, is a speed of nearly 50 words per minute —some way in advance of what could reasonably be expected with 2,075 nautical miles of a core whose conductor is 225 lbs. copper to the same weight of dielectric, which, no doubt, well meets the ordinary exigencies of traffic. I doubt if the electrical values involved by the latter could permit of more than half the above speed, if as much.

Altogether, the type of the South American cable was probably one of the best and most carefully designed of any in existence. This was only to be expected when a company has the advantage of being—somewhat uniquely—under the direction of men with a *technical* knowledge, like our senior Vice-President (Mr. R. Kaye Gray, M. Inst. C.E.), besides our worthy President and Mr. J. Y. Buchanan, F.R.S., with Mr. Edward Parsoné as secretary, and with the additional advice at hand of Messrs. Clark, Forde, and Taylor. The same circumstance readily explains the prudent and carefully planned course adopted for the last repairs.

Professor John Milne, F.R.S., has recently expounded on submarine earthquakes. His lecture, together with Mr. Benest's interesting and instructive paper, render it no longer necessary to search for mysterious or malicious agencies in connection with ruptures in our submarine cables.

In Mr. Wilkinson's excellent recently published book we were furnished with a number of graphic illustrations incidental to cable work. Mr. Benest has now added to these by the life-like lantern slides which have been flashed before us during the reading of this paper, for which we are much indebted to the Silvertown Company as well as to the author. Mr Bright.

Mr. W. H. PREECE: Mr. President and gentlemen,—It has been said that a man who succeeds in making two blades of grass grow where only one grew before is a benefactor to mankind, and I am quite sure that the reader of a paper before this Institution who adds many facts to where none existed before is a great benefactor to his profession. With this particular paper we have had a considerable increase to our knowledge, and I think that our first debt of gratitude is due to the India-Rubber and Gutta-percha Company, who have allowed these facts to be published. Those in the profession will know that from time to time we are deeply indebted to that company for the most complete, accurate, and well-prepared accounts of the works they have done; and it is only to be regretted that there are no other contractors in this country who follow such a very excellent example. In the next place, our gratitude is due also to the writer of the paper, who, in the language just used by Mr. Bright, has not only been occasionally poetical, but certainly always instructive and invariably interesting. I think that the world in general, and geographers in particular, are greatly indebted to the telegraph engineer; for if it had not been for the telegraph engineer, and the necessity of supplying the world with submarine cables, we never should have known much about the constitution of the sea, and little or nothing about the nature of the bottom. The fact of the expansion of our industry all over the globe has necessitated such expeditions as that of the "Challenger." Mr. Preece.

But even these, great as the works have been, are nothing compared to the knowledge of the sea, and bottom of the sea, that has been acquired through the operation of the telegraph engineer. Mr. Benest has endeavoured to persuade us that he has discovered submarine rivers in the ocean. There is no doubt that there are springs to be found, for fresh water has been brought up; but how

Mr. Preece. far Mr. Benest's facts are due to submarine rivers, or to currents carried down for some distance, I am not prepared to say. All I say is that Mr. Benest gives us very good evidence to believe that in this particular part of the ocean there must be a considerable current of matter-bearing water. In the first place the temperature was high. We know the mean temperature of the ocean to be something like 35° Fahr., but here we have at a considerable distance from the shore a temperature of 49·9°. Again, he gives us accounts of the water being discoloured and heavily laden with detritus at the bottom, consisting of green mud of different consistencies—all, to my mind, illustrating the existence rather of currents of the ocean than of subterranean and submarine rivers. Again, in his very graphic descriptions of storms and fogs, and the phosphorescence of the sea, he leads us to conceive that there may be some connection between the weather and the breakage of the cable. I am not quite sure that we can regard these as proofs of any connection between them. They are probably remarkable coincidences, although one cannot help feeling that the discoloration of the water, the breakage and burying of the cable, must be due to some great convulsion that has taken place at the bottom.

Perhaps one of the most interesting parts of the paper to many will be the accurate and clear description that the author gives of the cable—its specification and its qualities.

Again, I am a little bit surprised that in laying this cable it was laid at such a speed. He mentions that the cable was laid at an average speed of, I think, 7·6 knots. He mentions knots per hour, but the word "knot" means "nautical miles per hour," and when we speak of knots *per hour* we can commit gross inaccuracy. The word "knot" is improperly used very often in this paper, though the author speaks of nautical miles. Sometimes we speak of "nauts," but we are always wrong when we speak of the unit of distance as a "knot." Knot means a velocity and nothing else. Now, apart from the speed, which is 7·6 knots, there is also the slack. The cable has lasted for a good many years, and, fortunately perhaps, a good deal of knowledge was possessed of the

bottom of the line taken; but, as a rule, it is a great risk in crossing an unknown ocean to lay your cable at such a speed as 7·9 knots, and with so small a slack as 9·9 per cent. Mr. Preece.

To come to the question of repairs, I don't think anybody for one moment can doubt, after the clear description we have received, but that earthquakes or landslips occurred at the bottom of this particular part of the ocean—some volcanic eruption or earth upheaval. This, probably, was a kind of terrestrial boiler explosion, but we do not know what; all that we do know is that from some cause or other there was a very violent convulsion at the bottom. The mud was stirred up in such a way as to produce the effects that Mr. Benest has referred to, and the result has been the rupture of the cable and its burial in mud. There can be no doubt that these cables have been destroyed by terrific mechanical tension. The fact of the occurrence of a volcanic eruption of this kind is confirmed by what we have observed on the East Coast of Africa, and also with regard to the cables between the Straits Settlements and Australia. I think we may say now that the existence of these volcanic eruptions has been proved, and it must make us think twice before we design or lay cables along unexplored oceans, especially where these oceans are subject to volcanic action. We have heard a good deal lately of the Pacific cable. The Pacific cable is to lie for a thousand miles or more along an ocean which is known to be covered with volcanic remains of different kinds. We do not know when these volcanic actions took place. They may have been in prehistoric periods, and they may have ceased; but, at any rate, we do know that there are to be found in the Pacific Ocean volcanic remains, for every atoll, every coral reef, that is formed generally has volcanic material for its groundwork.

Lastly, our knowledge of the nature of the bottom has been very greatly increased by these submarine cable operations. I remember distinctly a great many years ago—I think in the year 1859—I read in this very room, before the Institution of Civil Engineers, a paper on the maintenance and repairs of submarine cables, and I then ennunciated the doctrine that it was absurd to attempt to lay a cable unless we knew something of the nature of

Mr. Preece. the bottom, and that an engineer did not fully realise his business unless he commenced first by making an accurate survey of the ground he was going to cross. You would scarcely credit it, but my proposition was received with laughter, and was treated with that ridicule that ignorance always treats words of wisdom that come from experience. Experience, gentlemen, is the root of all our engineering wisdom; and it often happens, especially with electricians and telegraph engineers—who are bound to be before the world—that when they enunciate new doctrines they are treated as I was treated in this room. The knowledge that we have obtained from this paper of the furrow off Cape Verde, and the two accidents which occurred, require, I think, a little further illustration. I am glad to see behind me Mr. March Webb, who has had a considerable experience in these waters, and I would ask him if he would confirm what Mr. Benest has submitted, and if he is inclined to favour us with further and other views.

Mr. Webb. Mr. E. MARCH WEBB: It is but a truism to remark that Mr. Benest's paper contains many subjects of great interest to submarine cable engineers, and among them perhaps the most interesting—or, rather, the one which attracts most attention—is the question as to the cause of the break in the South American cable. The nature of the bottom, its contour lines, its gradients, afford us no clue, and we can but theorise as to the cause of the rupture.

Now from one point of view this uncertainty may be of advantage, since a wider field is offered to the imagination, and consequently a freer scope is given to discussion than would be the case were no uncertainty attached to the events described in the paper. But, on the other hand, these conditions bring very forcibly home to us how little we really know of the changes which, from one day to another, may take place in the ocean bed, and how possible it is to be misled, notwithstanding the greatest care given to sounding work and in the making of general investigations.

It is to be regretted—and I am sure that most cable engineers will agree with me—that papers such as the one read by Mr. Benest are not more frequently placed before us, for then, I think,

in the course of time there might be collected evidence Mr. Webb. sufficient to prove or disprove theories which may have been formed, conclusions one may have come to. The lack of papers possessing the necessary qualifications is due in great measure to the reticence generally observed on the occasion of a break or fault in a submarine cable, and in great part is also due to the hurry in which repairs have usually to be effected, the prime motive being the speedy re-establishment of communications; altogether insufficient time being afforded for any extra sounding work, or for such absolutely necessary investigations as temperatures and densities.

And here perhaps Mr. Benest will permit me to make a friendly criticism. It seems a pity that on the occasion of these repairs some little time had not been given to bottom and serial densities. It is probable—at all events, very possible—that had these investigations been made some further light might have been thrown on the question.

Without going so far as to accept in its entirety Mr. Benest's theory as to a submarine outflow of fresh water in the form of a considerable stream, the evidence Sir Henry Mance has given us would seem to corroborate the theory. We are told that in the Persian Gulf, at a considerable distance from the mainland, a column of fresh water rises from the bottom, and the body or volume of this water is great enough to preserve its sweetness amidst the surrounding salt water. Now I do not think we are in a position to set a limit to the distance from shore at which such an outflow may take place, nor can we well draw a hard-and-fast line as to the volume of that outflow.

The sounding chart which has been offered for our inspection shows the existence of a gully, clearly and distinctly defined, terminating in front of and at no great distance from the lagoons mentioned in the paper as having no visible or apparent outlet. This gully seems to indicate the cutting or scouring-out action of a river at some former epoch; at all events, it is no far-fetched theory to adopt this view. Now it is to be remarked that a river, whose higher reaches are crossed by the railway between St. Louis and Dakar, discharges in the wet season into these lagoons; but

Mr. Webb. in the dry season the current is lost in the sandy bed long before reaching the neighbourhood of the coast.

I do not think any stress is to be laid on the circumstance that these lagoons have no visible outlet; but the fact that a river exists behind them,—that they are in a direct line between that river and the head of the gully,—would suggest a connection, and is of a certain significance.

We have enough evidence to show that the sea bottoms off those localities on a coast line where a disused river mouth exists, where the river itself is lost through filtration before it reaches the coast, and where a gully more or less distinctly defined is met with, are only too frequently dangerous to the life of a cable. It does not seem necessary that the cable should cross the gully at steep gradients, or even cross the existing gully at all, to render its position dangerous; but it does seem essential that there should, at all events, be present traces of the scouring-out action, at some former period, of a river current to account for the perilous nature of these localities.

To further explain my views, I would remark that there are many localities where the physical conditions might lead one to believe that at some former period, when the contour lines of the sea bottom and of the land, and the physical features in general, were different, a river discharged into the sea, cutting for itself a channel now met with in the form of a gully. In course of time, and owing to climatic and physical changes, the river was compelled to take a subterranean course, and to discharge its current beneath the sea somewhere in the neighbourhood of the head of the gully, once its channel. The silting up of that portion of the channel in more immediate proximity to the coast can be readily explained by the action of coast currents and of waves.

With reference to rivers having an underground course, a very striking example is found a mile or two to the north of Arica, a port on the Peruvian coast. The bottom of the river valley is filled up with loose sand, and no evidences of the proximity of water are apparent. But in digging down to a depth of some 15 feet, when a firmer stratum of sand is met with, a continuous supply of fresh water is discovered, and a current is distinctly

observable, as the water rapidly filters through the sand into and out of the pit. This subterranean stream is met with as a rapidly flowing river some considerable distance inland among the higher foot-hills of the great mountain ranges, but it speedily disappears on entering the sandy and rainless coast region. I would add that the river valley is a deep one, and well defined from the coast inland through the mountains. Mr. Webb.

There are some gentlemen present to-night whose experiences, if they can be persuaded to relate them, in relation to the possibilities I have suggested, would be of great interest to us, and might perhaps assist in the solution of the problem Mr. Benest has bequeathed to us.

Captain WILSON-BARKER: I am indebted to the kind thought of your Secretary for the privilege of being present at the reading and discussion of Mr. Benest's interesting paper. Capt. Wilson-Barker.

I am not qualified to tackle the technical and most important part of the paper from the standpoint of this Society, but I may perhaps be allowed, as one having some experience of cable work, and being greatly interested in the extension of our knowledge concerning the depths of the sea, to make some remarks on the interesting questions which Mr. Benest has so well pointed out.

I am fully alive to the possibilities of deep-sea river outlets, but I am inclined to think that they can hardly produce the effects on the cables which are here set before us; but am much more inclined to think that hot and chemical springs, such as those at Bath and elsewhere, could produce such effects.

Mr. Benest remarks on the extraordinary weather and sea effects off the Cape de Verde Islands—phenomena which are well known to navigators, and which are largely due to geographical position and the curious combination of air and sea currents.

In connection with the dust, it is curious to note the banks of sand piled up into dunes on the island of Las Palmas, which, report says, is blown over from the mainland of Africa; it is certainly not produced in the immediate neighbourhood.

Not the least interesting part of Mr. Benest's paper is that on the soundings. I should here like to take the opportunity of

Capt. Wilson-Barker.

saying how much more our knowledge of the depths of the sea might be extended by those engaged in the laying and repairing of cables. In the latter case especially might our knowledge be greatly extended. What opportunities occur, say, to the doctor on board, who has time, when the operations are going on, for collecting and securing specimens as the cable is being picked up! It would be quite out of place here to dilate on the wonderful and beautiful things that come up on a cable—objects the beauty of which can only be seen when the creatures are alive.

I cannot refrain from agreeing with other speakers in saying how foremost the Silvertown Company has been, not only in carefully surveying proposed cable routes, but also in giving the results to the world, thus assisting in a very practical manner the contouring of ocean depths.

One word on the recording of soundings. There is much room for improvement here, and it would be of great value if a little more care were taken in observing the general nature of the sounding. For instance, a sounding will often be noted down as coral when there is not a particle of coral in it, the coralaceous appearance being due to the presence of polyzoe and calcareous sea-weeds.

I have seen large foraminiferæ recorded as grains of sand in a sounding in which there were only minute traces of sand. A cursory glance with a pocket lens would have decided the question at once.

Mr. Bright has alluded to my plan of preserving soundings,* and it is that the sounding as it comes from the sounding tube should be forced into a glass tube and secured with corks at both ends: this would show at a glance the general nature of the bottom deposit, and form a very handy and convenient way of storing them. The specimen I show explains this, the label being placed on the top end of the sounding.

As a further record there is no difficulty in securing in a few moments a microscopic photograph of a portion of the sounding, as the one I will pass round illustrates.

* Fully described in *Science Gossip*, April, 1892.

The presence in this specimen of foraminiferæ and their fragments is clearly shown. Capt. Wilson-Barker.

Mr. Benest is to be congratulated on his most interesting paper, and I have much enjoyed the privilege of hearing it and taking part in the discussion.

Mr. H. C. DONOVAN: I must compliment Mr. Benest on his able and interesting paper, and am glad to add my thanks to the manager of the India-Rubber and Gutta-Percha Company for his kindness in enabling all this valuable information to be placed before us. Mr. Donovan

The scour mentioned by Mr. Benest reminded me of a cable between Martinique and Dominica which had to be repaired in 1873. It was picked up in shallow water close to Martinique. The amount of animal and vegetable growth upon it was most extraordinary, especially in sponge and coral. As the water deepened this growth gradually changed to a short, fine colourless grass. At a depth certainly over 1,000 fathoms the cable, which had hitherto come up in good condition, began to show signs of rough usage; the outer serving being completely gone, and the iron wires much damaged, and scoured bright. The geographical position of this cable proves that the destructive action referred to by Mr. Benest could not possibly have occurred in this case, and some other explanation, therefore, is necessary.

The enormous amount of denudation brought down by African rivers must account for many breaks which have occurred to cables on both the East and West Coasts.

The granite outcrop so frequently met with in Africa proves this denudation. I lately met with a geologist who had travelled much in South Africa, and who said that the greater part of this continent was denuded down to the granite.

There is one feature shown in some of the breaks in cables which I deem worthy of attention—that is, where the iron wires are found to be eaten down to fine needle points, only in patches. On one occasion these patches so frequently occurred that the engineer reported that it was, in his opinion, due to the cable being in proximity to submarine copper outcrops; but these patches had been met with in other places where the bottom

Mr. Donovan. consisted of soft ooze and free from rocks. In conversation with Professor Murray on this subject, that gentleman doubted the copper outcrop idea, but was inclined to attribute it to the action of sulphuretted hydrogen given off from some decomposing animal or vegetable matter near the cable.

Capt. Thomson. Captain ANTHONY S. THOMSON: Papers like this of Mr. Benest are to be cordially welcomed, because they may—I hope in this case they will—throw much light both on the causes of cables breaking so frequently when they ought not to do so, and also on the configuration and peculiarities of the sea bottom in which they rest. There certainly seems to be a great connection between the two. I rather wish Mr. Benest had stuck to his guns a little more steadfastly in the matter of the submarine river, so that we might have had that question thoroughly threshed out. It is so very easy to bring in volcanic action to account for an effect, the real cause of which is not apparent. Whenever a cable breaks, as a rule, we hear of bad weather prevailing in the neighbourhood, and we also hear of the volcanic eruption which is supposed to cause the mischief. There can be no direct connection between the bad weather and the volcano. Then, again, there are places where cables undoubtedly are broken by some movement of the sea bottom. In the case of the Congo gully, for instance, it seems tolerably certain that the repeated breaking down of the cable is attributable to this cause. Although the presence of the submarine river inferred by Mr. Benest may be more than doubtful in this particular locality, we certainly find conditions, such as the configuration of the coast line, a large river in the vicinity, and these curious lagoons on the shore, which do point to something like a gully similar to that of the Congo, but on a much smaller scale, having existed at some former period.

I cannot help thinking that the probabilities are in favour of the break having been caused by a movement of the bottom rather than by anything due to volcanic action. But I would just throw out this suggestion; is it possible that, in the case of a cable already weakened and supporting a great strain, the vibration of the cable itself caused by a heavy surf hammering on the beach

for several hours continuously, would be sufficient, not to be the cause of the break, but to determine the time of the break? This might possibly account for cables so frequently breaking during bad weather. The suggestion thrown out by the last speaker about the scour at the bottom of the deep sea is a very valuable one. All our information goes to show that the sub-surface currents of the ocean are much more important than has been generally supposed; and it is quite possible that there may be strong currents at a depth of 1,200 fathoms. Mr. Preece said he thought the high bottom temperature showed the existence of current. That is, perhaps, a doubtful point, especially as the temperature mentioned seems fairly normal for the given depth. Capt. Thomson.

The configuration of the coast line, and the directions of the surface currents, give rise to a large eddy in the neighbourhood of Cape Verde, which favours the deposition of the mud and detritus brought down by the Senegal, and it is quite possible that the mud may have a slow, but almost irresistible, outward motion. Of course the gradients are very slight, but the submarine springs which Mr. Benest supposes to exist may ooze up and disturb the mud, helping to retain the original inequalities of the sea bottom; consequently the mud may not move uniformly over an extended area, but along such lines of least resistance, just as accumulations of snow or ice do on the land. I suggest that possibly the slow outward motion of the muddy bottom in which the cable is embedded may be the cause of the clean breaks, which, occurring on apparently good bottom, are so difficult to account for.

Mr. Benest is to be congratulated on the capital profiles of the sea bottom, drawn to natural proportion of vertical and horizontal scale, which he has shown us this evening, and I should like to express the pleasure it has given me to listen to his most interesting and valuable paper.

Captain Goodsall: I have perused the paper read at the last meeting of the Society by Mr. Benest, and desire to congratulate him on its able contents. It shows from an engineer's point of view that interruptions in cables are due to certain causes, but I Capt. Goodsall.

apt. oodsall. am bold enough to say that any sailor who has had any experience in cable repairs would altogether differ from him as to the causes which in his opinion were the reasons for the interruption and breaking of these cables.

It would be impossible in the limited time at my disposal to enter fully into the many points of interest raised by Mr. Benest's lengthy paper, therefore I shall take his seven facts in which he sums up the result arrived at, for friendly criticism.

His first and second points relate to the action of storms as causes for the interruption of the cables in question; and upon this the main, if not the whole, scope of his paper depends.

Now, in my opinion, storms have nothing whatever to do with cables parting in the depth of water in which the breaks occurred. Because the force of the greatest storm does not affect the water in the form of storm waves to a greater depth than about 20 fathoms; and, indeed, I doubt whether storms influence the sea at a greater depth than 12 fathoms.

I make this statement because, having occasion to repair a cable in the Indian Ocean shortly after the south-west monsoons, the cable during the violent weather, with a tremendous sea on, was unworkable, but as soon as the sea subsided the electric current was able to pass through. When the cable was picked up the outer covering was fairly good, the G.P. perfect, but the copper conductor broken. As it rose and fell, it, of course, broke contact, and no messages could be got through; as soon as it was quiescent on the bottom the cable could be used. The cable further seaward was well covered with its original compound. Had there been much action of the sea this would have been broken off.

His third point is the effect of climatic conditions as a cause of breaks in the cable, and to a certain extent his inferences have weight, but again my opinion of climatic conditions is at variance with his.

Mr. Benest says that the disturbed state of the sea is the cause, but, as I have stated, this had little to do with the breaking of the cables; the real cause being the heavy flush of *débris*, sand, mud, &c., which after heavy rains is carried down

the Senegal River, whose estuary enters the sea in a south-westerly direction, and sweeps against and then flows south-south-westerly in conjunction with the North African current. Capt. Goodsall.

His fourth point is the most important of his seven facts, and explains, to my mind, the true cause of the break in the cables, not only in this particular case, but in other cases which in an experience of over 17 years in all waters, shallow and deep, have come under my notice, viz, cable suspension; and Mr. Benest clearly has this in view when he says: "The contour lines clearly "indicating hollows between higher levels on the bottom, at the "positions where the ends came up."

The heavy strain for a distance of two or three miles off the break in 1893, and also in 1895, was, in my opinion, due to suspension of the cable over hollows, and the weight of refuse matter from current drifts, and to no other cause whatever.

The remarkable difference in the character of the breaks—the 1893 being clean tension cut, and the 1895 showing evidence of violent action—might at first sight contradict the theory of cable suspension being the cause of breaking; but if we admit that the clean break was due to suspension pure and simple, the fact of the 1895 cable being an irregular break—for that, I take it, is what Mr. Benest means by "evidence of violent action"—can be explained in this manner.

The 1895 cable was not laid in the same position as the 1893 cable, but more to the westward; and probably, the break having occurred, the ends were worn irregularly by contact with hard patches of ground, or the hollows were not so deep, and, consequently, there was less strain on the cable, which parted gradually.

I have placed on the table a drawing, kindly lent to me by the Eastern Telegraph Company, which will illustrate a cable broken by suspension, and you will notice the clean tension break of that cable.

I may mention in passing that this cable was recovered off Rovuma Bay in 750 fathoms, mud bottom.

The difference in the character of the position at the two breaks is peculiar, being microscopic; and I should like to hear

Capt. Goodsall. how Mr. Benest can, from the extremely small quantity of earthy matter which could be placed even under a 1½-inch object glass, deduce two facts the importance of which cannot be underrated if true—and he believes them to be true, for he has italicised them—viz., that the locality of the break of 1893 showed an abundance of mineral matter, and the 1895 a scarcity. Now these statements are entirely opposed to the conditions of the cables when picked up, the 1893 having a clean break, whereas, if the sea bottom was mineral, it should have had an irregular and chafed break; while the 1895 cable, on a soft bottom, should, instead of an irregular break, be just the opposite, viz., a clean tension break.

I cannot but think that the abundance or scarcity of mineral deposit depended solely on the difference in the position of the cable laid in 1893 and the one laid in 1895.

It is certainly strange that Mr. Benest makes no mention of suspension of cables as a cause of break, although he must be aware of the importance it plays in the life of a cable. His whole theory is fixed on the "destructive agency of the sea "bottom"—it is his mainstay and sheet anchor; but I believe, and indeed have proof, that a cable, when resting on a level deep-sea bottom, is absolutely without movement or wear and tear of any description.

This specimen of cable, which was recovered after being submerged 10 years, shows the non-destructive agency of the sea bottom at 2,000 fathoms, for when recovered it had upon it the line wash put on it when manufactured.

To sum up, the breaking of cables is, according to Mr. Benest, due to an unaccountable destructive action on the sea bottom simply; whereas my contention is that all such mechanical breaks in depths of 1,000 fathoms (more or less) are due to suspension.

Capt. Lugar. Captain LUGAR: I have listened with considerable interest to the paper read by Mr. Benest at our last meeting, and as it coincides somewhat with my own experience in another part of the world, I would like, with your permission, to say a few words on this subject.

While in command of the Central and South American

Telegraph Company's repairing steamer "Relay," I was called Capt. Lugar. upon, in April, 1891, to repair a section of cable connecting Payta, in Peru, with Santa Elena, in Ecuador. By tests from Payta, the fracture in cable was located at a position nearly due West, about 10 miles, from the small harbour of Talara, between Cape Blanco and Parina Point, in Northern Peru. (Talara, about this time was celebrated for the great quantities of petroleum found in the vicinity.) This section of cable was noted for the regularity of its rupture nearly every year, about the end of March, or early in April.

After arriving on the "ground," I took a series of soundings to determine position of gully said to lie off this coast. I then grappled for cable on either side, and succeeded in getting each bight to the surface without any difficulty; but, on picking up towards the fractured ends, I found the last half-knot on both sides deeply embedded in mud and clay, wires scoured quite bright, cable flattened in several places, and very "screwy," some of the wires broken and "rucked" up near the end, and showing unmistakable signs of having undergone great tension and considerable rough usage.

The weather was fine, light breezes and smooth water; in fact, in this locality gales are unknown, and rain seldom falls near the coast, but beyond 50 miles inland from Talara, at times the downpour is exceedingly heavy.

In repairing this cable I relaid the inserted piece some considerable distance further west, where, from the soundings I obtained, I thought the cable would be fairly clear of future trouble.

However, in the latter part of March of the following year—1892—it broke again, apparently from the same cause as in previous years. I had a similar experience in recovering the fractured ends. I devoted all the time I could spare to sounding, and traced the sides and bottom of the gully from about half a mile of the entrance to Talara Harbour to about 12 miles west. I noticed in nearly every case, the specimen of bottom brought up from the deep part of gully, was coarse gray sand, and small stones; that of the sides a very tenacious clay; and from the comparatively level part further away, soft green mud. The piece of

Capt. Lugar. cable inserted this time was laid about three miles more west, in over 1,000 fathoms depth of water, with an abundance of slack, and I felt confident we should be free from trouble in this locality for a year or two at least.

We returned to Callao on the evening of April 1st, and before coming to anchor, I received a message from shore, that the section we had just repaired was getting weak, and by the time I arrived at our Callao office, communication had been completely interrupted.

Mr. Kingsford, the company's engineer, took some tests from Chorillos, and reported break at practically same distance as before. I believe we actually found the fracture in the middle of piece inserted by us a few days previously. We found the cable flattened near the ends, and about 400 fathoms of either side stripped to the bare wires, which were quite bright; and this had been done inside of five days.

This time I carried the inserted piece seaward into about 1,400 fathoms, and nearly seven miles outside the position of last fracture, and I believe this section has not been interrupted off Talara since.

Some few months later, during a conversation with one of the officials of the Talara Petroleum Company, about the nature of the bottom outside their harbour, he informed me that a Peruvian half-caste he had employed at the wells asserted, that beyond the Amatape Mountains, which lie at the back of Talara, there existed a chain of lakes, which had an outlet through a hole in the mountain side, and that canoes and paddles lost on the lakes had been found on the coast between Talara and Parina Point.

This evidence, I think, goes far towards proving the existence of a submarine river in this locality, and that the greatest outflow takes place in the months of March and April: we have had fair proof of its action by the many interruptions to the cable laid across its outlet. These months coincide with the time of heaviest of the rainy season in the Cordilleras and Amatape Ranges.

Before concluding, I may cite one other submarine stream that has come under my personal notice. While at anchor off the

small but lofty island of Saba, in the West Indies, I visited by boat, a spot in the sea about one-third of a mile from the shore on the south-west side of the island. I there found the water bubbling up in small circles. I sampled some, and found it slightly brackish to the taste. I tried several different parts, with same result. However, I was informed by the gentleman of colour who guided me to the spot, that sloops and schooners often filled up their barecas from this submarine stream. Capt. Lugar.

Mr. R. K. Gray: I have had some experience at sea in connection with the repair of cables, and I am particularly interested in Mr. Benest's paper owing to my connection with the India-Rubber and Gutta-Percha and Telegraph Works Company. I thank Mr. Preece very much for his kind remarks with regard to this company, and I also thank the other speakers for what they have said in relation to the liberality of the company in giving information. I hope that the company will continue to do so, and that its action in this respect will be of advantage to the Society. Mr. Bright asked a question about india-rubber, and perhaps I may reply to it. I am glad to see Mr. Hooper present, as it gives me an opportunity of certifying to the excellence of his manufacture. As a director of one of the telegraph companies, I recently heard read a report that a cable laid by Mr. Hooper in 1875 was in very good electrical condition. Mr. Hooper will know the cable I refer to. This experience of an india-rubber-insulated cable after 22 years' submersion seems worthy of record. Mr. Gray.

A point raised by Mr. Preece was in connection with the speed of paying out cables. To a certain extent I am responsible for laying cables at high speeds. I remember that 13 years ago, after we had made a very careful survey of the bottom between the Canary Islands and Senegal, we thought it perfectly prudent, with big cable tanks and a well-equipped ship, to pay out cable at a very high speed; and while passing over a portion of that ground we paid out 219 knots in 24 consecutive hours, or at a speed slightly exceeding 9 knots an hour. In 1882, in the Pacific Ocean, between Buenaventura and Panama, under similar circumstances, 150·3 knots were paid out during 17

Mr. Gray. consecutive hours, or an hourly rate of almost 9 knots. After an experience of 13 years in one case and 15 years in the latter, we have had no occasion to regret our action. We have frequently paid out as much as 10 knots in one hour. Mr. Preece is correct in stating that 7 knots per hour is the usual paying-out speed; but the difference in risk between paying out at 7 knots and paying out at 10 knots is really insignificant, if the laying ship possesses large tanks and is well found in men and machinery. The angles at which the cable enters the water differ very little within these speed limits, and consequently the effect produced by the inequalities of the bottom is very much the same. In irregular, uneven ocean floors our experience teaches us that the speed of paying out should not exceed 1½ or 2 knots per hour.

From these statements it will be gathered that with a normal ocean floor, when a speed of 5 or 6 knots per hour is permissible, the only limit to be placed on the paying-out speed is purely due to the capabilities of the ship and the skill of the men engaged in the operations.

The question, however, that Mr. Benest raises, and which has been the principal subject of the discussion, is a very interesting one, and that is, the cause of some breaks in submarine cables. If it be remembered that a break may cost, more or less, £5,000 or £6,000, it will be seen that it is a very important thing to diminish these breaks as much as possible, and such papers as that under discussion tend towards this result. I know, for instance, of a case in which the Eastern and South African Companies' cables were interrupted for eight years in succession off the Rovuma River, on the East African Coast. Had the cause of these breaks been well understood, some £40,000 or £50,000 expended in repairs might have been saved, and the laying of a duplicate cable costing about £90,000 would have been avoided. I had some correspondence with the late Sir James Anderson about cable interruptions, probably 10 or 12 years ago, and Sir James told me that the reports he received led him to think that something happens at the bottom of the sea which was similar to the welling up of the water that he had often observed in the Solway Firth. I rather crossed swords with him on this point,

telling him that, although things were similar, they were not quite the same; because in the case of the interruptions off the Rovuma River there was a column of water of about 800 fathoms rising from the ground where the springs showed themselves, and that the head of water necessary to create the surface disturbance which he clearly said occurred must have been very superior to that resulting from what might be called the normal hill-level in proximity to the shore line. To account for these geysers—or submarine river outlets, as I prefer to call them—or, rather, as evidence of their existence, we have just heard the last speaker say that certain interruptions to cables occurred on the coast of Peru in the month of March. My own experience on the West Coast of Africa has been that interruptions have generally occurred about the month of March. All the interruptions which I refer to as happening off the Rovuma River occurred about the month of March. Mr. Gray.

I may tell you, besides, that the latitude of the Arica River, the latitude of the Congo River, and the latitude of the Rovuma River are practically the same. Of course the positions do not actually agree, but they are practically in the same zone, and lie between the tropics of Capricorn and Cancer. I think it is good evidence that, if a certain thing occurs at different portions of the earth, and at the same period of the year, its occurrence has something to do with the period. Captain Lugar has told us, and I think others will tell us, that in the tropics, as elsewhere, the month of March is a rainy month; and therefore I think it is fair to assume that heavy falls of rain take place about this period in the unknown interiors of the continents of Africa and America; while on many of the shore lines, near where the cables have been broken, there is nothing but sand for miles inland, and comparatively little rain. The rains from the interior find their way to the sea by surface rivers in some cases, and I believe by subterranean rivers in others; this subterranean flow being not merely percolations through porous strata, but large volumes of water flowing through caverns and crevices in the earth's crust, which find their outlet at sea, and have their source in the mountains.

Mr. Gray. If one accepts this view, one must also recognise the variation which will take place in the head of water from one season to another, and, as a consequence of this variation, the intermittent outbursts under the sea.

The existence of small subterranean flows is, of course, a well-acknowledged fact, and within the last eight or ten years a Mr. Martel, assisted by Mr. Gaupillat, has been making considerable cave-hunting investigations. These he describes in "Les Abîmes" (Paris, 1894).

In Austria, France, Belgium, and also in some parts of Germany and Greece, the existence of curious cracks and caves has been observed. Mr. Martel has even gone down into things which are known in Yorkshire as "swallow holes," some 700 or 800 feet, and by lowering a collapsible canoe, very light in structure, he has been able to navigate in subterranean lakes for two and three miles. If these things exist on land, why not at sea? If limestone formations contain caves and subterranean conduits—and there is no reason why limestone should not exist under the sea bottom—then cavities must occur on continental slopes.

With regard to the particular break to which Mr. Benest refers, there are two or three points connected with the depth contour lines which are worth noting, if I may encroach upon your time. The river shown on the map of Cape Verde Point which has been referred to is probably some small stream fed locally, but the lagoons are created by springs from artesian water. If you carry your eye from these lagoons to the sounding of 230 fathoms, where the buoy is shown on the chart, you will have traversed a crust of water-covered shore sand. When you go seawards of the buoy on the same line you have the head of a large gully. That gully, I believe, by the formation in its neighbourhood, can never have been formed by a surface river, because one finds 230 fathoms increasing almost precipitously to 670 and 760 fathoms.

I believe that at about 600 fathoms from the surface, and about 60 or 70 from the bottom, the outlet of the river will be found, and that at certain seasons of the year—in the month of March

probably—a geyser-like or volcanic-like effect is produced. In the last repair to which Mr. Benest refers, I have acted on this belief, and had the cable laid inshore and at a higher level than the supposed outlet. On other similar occasions I have recommended that a like course be adopted. Sir James Anderson took action in regard to the last repair off the Rovuma River which was more or less founded on this belief. In all cases where the situation has been dealt with on the assumption of the existence of submarine river outlets on continental slopes, and in which the outlets have been localised by sounding, the recurrence of breaks such as those referred to by Mr. Benest has been checked, if not overcome. The cable that was laid between Zanzibar and Mozambique—the first cable—as I said, broke down eight years in succession. Sir James Anderson, after this sad experience, decided to relay it inshore on hard sand. I understand that since it was laid inshore, some 10 years ago, it has never broken down; I believe this is due to the cable being relaid in a less depth than the submarine river outlet. The outlet continues to periodically vomit its *débris*, but the cable lies out of danger. I need not go too far into the geological aspect of these outlets; this portion of the subject is more fitted for the Geological Society than for the Institution of Electrical Engineers. Still, it is well to start such a discussion here, for we are so very much interested in telegraph cables that it is our duty to discuss, however inadequately, any subject affecting such large interests. To cite another instance, I may mention that when the São Thomé Loanda cable broke, off the river Congo, we relaid it towards the shore, and towards the outflow of this enormous surface river. The cable, which had broken down twice in the same neighbourhood in 15 months, now lasted five or six years. Judging from to-day's evidence, I think it was not laid sufficiently inshore. The repairs passed into other hands before the line was again broken, and we could not carry out our theories. During the seven years that followed the cable broke down about as many times; in one case I believe it broke down twice in two months. The reason for these breaks I believe to be that at each repair it was laid below the submarine outlet. The Central and South American Company, off a point, Esmeralda, in Ecuador, had a similar experience, and surmounted

Mr. Gray.

Mr. Gray. the difficulty by laying the cable above the outlet. From this evidence I think we may consider that the rainy season in the interiors of continents, and the existence of subterranean rivers, account for a great many interruptions to cables which are laid parallel to the coast line. As for cables laid in the deep ocean—across the Pacific, which Mr. Preece has referred to—the case is totally different. Cables such as those laid to Bermuda and other low-hilled countries are not affected in the same way as those laid parallel to the coast lines of continents, and I think a very great deal of the well-known immunity from interruption enjoyed by the deep-ocean cables is due to the fact that in these deep waters one gets further and further away from subterranean and submarine rivers and their consequent landslips.

Capt. Goodsall. Captain GOODSALL: With regard to the cable off Rovuma River, it was laid in 1879, parted in 1882, parted again in 1883, again in 1884. I repaired it again in 1885, and then suggested that the line would be better inshore. Nine months later the cable again parted, for the sixth time. I then made a thorough survey—it is always prudent to make thorough surveys for cable route—and carried the cable inshore, but took care, by placing buoys previous to the laying of the cable, to get the line between the 200- and 100-fathom patch, making a point as near to the shoulder of the bank as possible. The cable was so laid, but on circling off the mouth of the river, unfortunately, a portion of it got into shallow water; it was picked up again, and relaid in the line first thought of. The cable was finally repaired in March, 1886. It kept in thorough repair, I believe, until last year—1896—when it parted.

All these breaks occurred at the rainy season of the year.

The PRESIDENT: There is an interesting letter from Professor Milne, which the Secretary will read.

The SECRETARY: Professor Milne, F.R.S., F.G.S., writes as follows :—

Professor Milne.

SHIDE HILL HOUSE, NEWPORT, I.W.,
March 10*th*, 1897.

DEAR SIR,—I beg to thank the President and Council of the Institution of Electrical Engineers for the kind invitation to attend the meeting on the 11th, which serious illness in my home prevents my accepting.

For years past I have not only been recording earthquakes by means of ordinary seismographs, the movement due to which can be felt, but I have also been recording larger disturbances which can *not* be felt. These latter have their origin in the deep sea far from land, and a few of the dates and hours have corresponded with the times at which cables were broken. Mr. Benest describes a break in March, 1895. From the locality in which it occurred, it was probably due, as he suggests, to a submarine landslip, which is not necessarily seismic. On March 5th, however, at 22 hours Greenwich mean time (March 6th, 10 a.m.), a larger unfelt disturbance shook Europe, and probably the world. It would therefore be of interest and value to me to know the day and hour on which the 1895 break took place. Professor Milne.

I should much like to bring to the notice of the Institution the remarks on page 8 of the pamphlet which I send under separate cover. Regretting that I am unable to attend,

Yours faithfully,

(Signed) JOHN MILNE.

F. H. WEBB, Esq.

Extract from Lecture on "Recent Advances in Seismology," delivered by Professor John Milne before the Royal Institution on February 12th, 1897 :—

> From the times at which movements were recorded at different stations, it would seem possible to localise the origins of disturbances which in many instances are submarine. This would throw new light upon changes taking place in ocean beds, lead to the identification of districts which those who lay cables are desirous of avoiding, and sometimes enable us to attribute cable ruptures to natural rather than to artificial causes.

Professor Milne further writes, on March 30th :—

> The Seismological Committee appointed by the British Association have already issued a circular to a number of existing observatories, asking for co-operation in a seismic survey of the world, one object of which is to localise submarine earthquakes and volcanic foci, and the value of the records which would be obtained if instruments could be established at some of our cable stations can hardly be over-estimated.

Mr. E. W. PARSONE : The paper under discussion is one of special interest to me, because I have on several occasions been employed in the repair of cables under circumstances similar to those dealt with. Captain Benest has referred to breaks in the cable which was laid in 1875 off Pescadores Point, which is situated about 60 miles north of Mollendo. During the laying of this cable, on arriving at the position in question, a sudden Mr. Parsone.

Mr. Parsons. deepening of the water was observed, and the ship's course was altered to the eastward, so as to bring the line of the cable nearer the shore. This section of cable, however, gave a considerable amount of trouble, interruptions at the locality I have mentioned being of not infrequent occurrence. Soundings in the neighbourhood gave very irregular depths, and indicated a channel some 40 miles distant from the coast, somewhat similar to the "Bottomless "Pit," on the West Coast of Africa, which Capt. Benest has described. On shore in this neighbourhood there are some indications of an old river bed, and, I believe, some lagoons in the interior; but it is not a flowing river—that is to say, the water does not reach the sea, but apparently loses itself, like many other rivers, before making connection with the salt water. The cable in this neighbourhood was repaired many times, and after the first one or two there was no excuse for saying that the cable had broken from insufficiency of slack. There was, in fact, abundance of slack for all the inequality of bottom. But still it broke, and was frequently found buried, and only got up from its bed with great difficulty. Also, when tortured, it brought up with it masses of vegetable matter, in the form of branches and trunks of trees, which had to be cut away with axes before the cable could be got inboard As there was no growth of this kind for miles north and south along the coast, I was at first led to believe that this vegetable matter had been brought up from the south by currents, and, on becoming waterlogged, had found a resting-place in this submarine gully, not being able to get out of it, so to speak; but the accumulated masses were evidently the remains of olive trees, which do not grow along the coast to the south, although round the Arequipa district, some 80 miles inland, olive groves abound. Of course it is difficult to imagine that such should be the case; but if there is any foundation for the theory we have heard advanced, it would look as if these remnants of olive trees had drifted with the surface river water so far as it came from the interior, and disappeared with it into the subsoil, to emerge with it at sea by a submarine exit. To further support this idea, the breaks in this cable generally occurred after freshets due to rain

in the interior, and the cable could never be depended upon for any length of time, until, in the locality I have indicated, the course was diverted towards the shore, and as near thereto as it was possible for the ship to get. Since that has been done no further trouble has been experienced, and, in fact, all anxiety with regard to it has been allayed. That big rivers do absolutely disappear is, I take it, an accepted fact. The river of Peru—the Rimack—at one time or another had an estuary some 30 miles in extent, as can be seen by the formation of the coast north and south of Callao, but I doubt very much if at present this river actually makes connection with the salt water except by percolation. In the interior it is a powerful mountain torrent, but at Lima, except during the seasons of melting snow and heavy rains, it is a mere driblet; and the force of this was humorously illustrated by an American, who, when shown the very fine bridge which spans it, said to his Peruvian friend, "I guess you'd "better sell your bridge or buy a river." No doubt in time the Rimack will entirely disappear. Mr. Parsons.

I will now allude to a similar sort of thing on the West Coast of Africa, which has not, I think, been mentioned in this paper. In 1888 I was commissioned, in connection with the southern lines of the West African Telegraph Company, to make a report on the general features of the coast between Benguela and Mossamedes, and a *resumé* of this report was published in the Geographical Notes of the *Royal Geographical Society's Journal* in July, 1888, as follows:—"Some 10 miles to the north of Mossamedes a remarkable "break occurs in the cliff, which opens out an extent of valley, "with some vegetation and low-growing trees. This is the valley "of the river Giraul, which for many years has been quite dry at "its mouth; in the interior occasional pools are found in its bed, "and a stream is sometimes formed after heavy rains; the water, "however, never reaches the coast on the surface, and no doubt "finds an exit subterraneously, to discharge itself into the sea. "It is a curious fact, notwithstanding, and one worthy of note, that "an old anchor with heavy wooden stock has been found some 20 "miles up this river embedded in the ancient channel, which "leads to the belief that at some past date this river was navigable

Mr. Parsons. "for a considerable distance from the sea." And, in view of the cable which was to be laid between Benguela and Mossamedes, I pointed out the necessity of taking very careful soundings in this locality, which I estimated to be situated some 10 miles north of Mossamedes. The cable was laid in 1889, and it had the curious record of being broken in this neighbourhood—1st, on 15th June, 1890; 2nd, on 19th December, 1891; 3rd, on 19th May, 1892; 4th, on 15th December, 1894; and the mean distance of these breaks from Mossamedes was 10·6 nautical miles. Since the last break, the remedy has, I believe, been found in taking the cable closer inshore.

I was also entrusted with the first repair of the S. Thomé-Loanda cable, which was broken off the mouth of the Congo on 11th February, 1887, less than six months after it was laid. Following the instructions of Mr. Robert Gray, who was strongly supported in his opinion by Mr. J. Y. Buchanan, the course of the cable was diverted inshore some 40 miles towards the mouth of the river. Where it crossed the Congo channel, soundings showed a very deep ravine, as may be judged from the fact that at the northern bank it had a depth of 60 fathoms, and at the southern a depth of about 50 fathoms—these two positions being about eight miles apart—while midway between them the maximum depth of the channel was just 1,000 fathoms, being a variation in depth of at least 900 fathoms in four miles over water. Of course the difficulty presented itself of allowing sufficient slack, and sufficient could not be laid, however slowly the ship was steamed, on account of the strong current from the river, if the cable were laid direct from point to point. The cable was therefore laid from the northern bank to exactly mid-channel, where it was cut and buoyed, which allowed the cable to sag back to the northern bank of the channel. The ship then joined up the southern end of the cable, and laid towards the buoyed end, which was picked up, and the final splice made above the deepest water, the ship's head being kept up stream during the operation. We then had a cable running down the north bank, and a cable running down the south bank, with the bight coming up to the bows of the ship, practically forming a **W**; and when this bight was cast adrift, it was easy to imagine it would find the bottom

with sufficient slack. The proof of the efficacy of this measure was that the cable had a period of non-interruption of five years and six months. It has never had such an uninterrupted period since. In fact, in 1893, it was interrupted in this locality three times in less than 4 months. Mr. Parsons.

I am, unfortunately, not in a position to be able to solve the problem of the theory set forth in this paper. It, however, merits all the consideration it has received; and I think also that members of the Society who may be engaged in cable repair work, where similar features present themselves, would do well to remember what has been said on the subject, and not fail to collect and take note of any further facts bearing on the matter they may meet with.

Mr. JOHN RYMER-JONES [*communicated*]: Before expressing an opinion as to the cause of the fault, it will be well to describe the appearance of the cable recovered, and also the condition of the bottom in the vicinity of the fault, as shown by soundings; and one or two other points which have guided me to the conclusion arrived at. Mr. Rymer Jones.

(*a.*) The bottom mud brought up when sounding, and on the grapnel and cable, shows that the cable had been lying on a very soft bed of green-coloured ooze.

(*b.*) On the Fernando end the length of cable in circuit from Drag 4 to the fault, as shown by tests from the ship ... ... N.M. = 34·75 approx

Length picked up (by drum measurement) when the cable parted = 32·989
Length *not* recovered = 1·761
} = 34·75 ,,

Of the 1·761 N.M. *un*recovered, 1,460 fathoms reached from the ship's bow to the bottom.

Depth obtained by sounding when end came inboard ... ... = 1,460 fathoms.

(*c.*) On the St. Louis side the length picked up from Drag 13 towards the fault (by drum measurement) = 78·506 N.M.

Mr. Fymer-Jones.

Depth where end came up ... ... = 1,574 fathoms.

This end was twisted spirally, and had evidently broken away from a rock on the bottom under the very heavy strain to which it had been subjected.

(d.) At 169 fathoms from this broken end were 3 fathoms of damaged cable. Besides the hemp cords being stripped off, the outside tape is wholly removed at, and close to, the damaged place. The remainder of the 3 fathoms still has the tape on for half of its circumference; but, for the other half of its circumference, the tape has been worn off in a regular straight line.

Where the taping is removed the sheathing wires also have a good deal of their tape worn off, leaving the wires bright. The tape being worn in such a straight line seems clearly to show that this portion of the cable has been held, and the bottom mud moving past it. Besides the damaged part referred to, the cable is a good deal flattened in one or two other places of these 3 fathoms, showing evidence of great strain.

The damaged place is not more than 30 inches long, and forms two complete turns of a slight spiral form, which leaves little doubt that the cable has been bent on a rock at this point. For the whole 30 inches the cable is flattened, and the tape which covered each sheathing wire separately has been worn off the outside surfaces of all the wires, which are more or less bird-caged for the whole 30 inches, but especially in the centre of the spiral referred to. The core has been forced through for $14\frac{1}{2}$ inches—*i.e.*, $6\frac{1}{2}$ inches on one side, and 8 inches on the other side, of the centre of exposure. The core has wholly parted under what would appear to be a *shearing action*, since all the conductor wires, and also the gutta-percha at both ends, are similarly scarfed for $\frac{3}{4}$ inch.

The conclusion arrived at as to the cause of this break is: Owing to the inclined plane on which the cable was laid at this point, the mud has carried the cable along until its downward progress was arrested by a projecting rock which held the cable firmly, while, on either side, the cable has continued to move

down the incline. On one side the cable has been twisted in a direction to unlay and open the sheathing wires, till the continued pressure has forced the core out, and further twisting has then either wrenched the core against the sharp rock and cut it through as with a knife, or the cut has been made by one of the sheathing wires. That the surface of the conductor was large when the fault was localised from St. Louis seems tolerably certain by the remarkably steady tests obtained; and that the cable hereabouts was deeply embedded is shown by the high strain when picking up. Mr. Rymer-Jones.

Soundings where the ends came up gave respectively 1,460 and 1,574 fathoms, which are considerably greater than most other soundings recorded in the neighbourhood; so that it is not improbable that even greater depths may exist, forming a sufficiently steep incline for the cable to slip down on the well-lubricated bottom until arrested in its descent by a rock. Not only would the part hung up have to bear the weight of the cable on either side, but fields of the fluent bottom mud being periodically detached from points higher up the incline—as from a land-slip—would bear down and produce a pressure which the portion of the cable held by the rock would be unable to support.

If this be a reasonable conclusion, the question arises, "Why "should the bottom move?" to which I reply, that it is easier to imagine so fluent a mass subsiding from time to time than remaining stationary, even though the incline be not very great. But if some other motive power be necessary, probably the coast current setting to the S.S.W., and sweeping round Cape Verde, accumulates, and keeps moving, sufficient mud to cause a periodic subsidence down the gully in the direction of the fault. At any rate, it is a noteworthy fact that the date of the fault is coincident, to a day, with the terrible storm so graphically described by Mr. Benest. If still further cause of movement be sought for, probably the northerly equatorial current turning in a westerly direction past Cape Verde will provide it, for it seems not unreasonable to suppose that considerable disturbance of the bottom mud may be set up some miles out from the shore by the meeting of these adverse currents, and keep

Mr. Rymer. the mud from nearer inshore gravitating seawards in the direction of the faults under discussion. This will account for the fact that the 113 knots of cable recovered was for the greater part buried, and was picked up at a strain of about 4 tons; while, moreover, the mud was washed off by the water when picking up, leaving the compound on the cable clean and bright, without any animal or vegetable life adhering, except occasionally a few very small anemones, jellyfish, or limpets, whenever in places the strain fell, showing that the cable was unburied for short distances.

The foregoing explanation of the movement on the bottom is not intended to exclude the theory advanced as to a submarine stream or water welling up from the bottom and causing the movement. On the contrary, there seems some evidence in favour of this view; but, as it is not of a very conclusive character in the present instance, I am inclined to make the sea currents accountable for stirring up and accumulating the very soft mud which periodically subsides and has twice broken the cable.

Mr. Warren. Mr. THOMAS T. P. BRUCE WARREN [*communicated*]: When we recover the broken ends or damaged portions of a cable, we have in most cases no great difficulty in accounting for the causes which have led to the disaster; but, where the defective portions have not been recovered, and a broken end is brought on board, we are guided by the ends of the iron or copper wire as to whether the break has been freshly done or has been exposed for some time on the bottom. If time and circumstances permit, a fresh haul is made, which is repeated until the defective portion is recovered; in some cases repeated breaks lead to the abandonment of the position.

If the indications of the grapnelling operations show that we are working on an even bottom, we may fairly conclude that the cable has been buried in some tenacious or heavy deposit, and in hauling through this accumulated deposit the cable may be loaded to its breaking strain; hence in such a case we have to pick up slowly, so that the strain due to the deposit is restricted to a shorter length of cable.

If a cable is buried in a diatomaceous or foraminiferous

deposit, we are not hampered with these difficulties; but if a cable is covered with a thick deposit having a specific gravity of 2·5 to 3·5, it is easy to see that a few tons strain may be involved by simply lifting off the bottom. Mr. Warren.

I have been led to these considerations from the examination of a bottom given to me a few months ago by a friend, and which was recovered from a depth of 1,800 fathoms in the North Atlantic—latitude 49° 0′ N., longitude 43° 40′ W., 30′ from edge of Flemish Cap Bank, and 300 miles from St. John, Newfoundland. I believe this is the only case where we have a clear proof of injury to a cable from a submarine volcanic disturbance.

I do not mean to imply that the fault may have been caused by the eruption at 1,800 fathoms, but that the volcanic *débris* may have accumulated in shallow water and has been pushed off or slipped into deep water, and ultimately has buried the cable.

An important consideration arises from this as to the part which volcanic action has played in the formation of these banks in different parts of the world.

It has been said that sponge-spicules are not found in very deep water. As a few are present in this sounding, it would rather confirm the idea that the volcanic *débris* has collected in much shallower water; this *débris* in slipping away has got mixed with the foraminiferæ and other organic forms on the bottom, or which it has met with on its way.

It is not very difficult to understand how accidents may happen to submarine cables by the shifting of this volcanic mud, which may carry heavy boulders, or even masses of rock, before it.

Instead of recording these soundings as giving mud, sand, gravel, clay, &c.—which are most indefinite—I think Mr. Benest has inaugurated a system which is most valuable, not only to telegraphy, but to those dynamical problems which are connected with the formation of sedimentary rocks and strata.

The results of a chemical, microscopical, and magnetical examination show that this deposit contains—

Fragments of crystallised quartz.

White or cloudy granite.

White granite, with red oxide iron adherent.

Mr. Warren. Cloudy granite, coloured with red oxide iron.

Silicate alumina, with oxide iron, lime, and magnesia, in some cases adherent.

Silicate manganese.

Magnetic oxide iron.

Oxides titanium and chromium.

The presence of most of these were confirmed by blow-pipe reactions on pieces picked out under a low magnifying power.

The absence of definite geometrical forms, and some of the above strongly adhering to each other, confirms the fact of some of these substances having been fused. Being mixed with fragments of foraminiferæ shows that the disturbance must have been recent, and no doubt coincident with the failure of the cable. A few sponge-spicules are present.

As these mineral substances are associated with the hardest rocks, it is difficult to see how they could have been reduced to such a fine state of division by any ordinary action on the bottom.

The finer portions gave a brilliant display of colours when viewed with polarised light, but in the absence of angular measurements I do not think it would be prudent to speculate on what minerals are present.

An experienced mineralogist may possibly recognise many of these forms of coloured minerals under polarised light.

Capt. Morton. Captain D. Morton, s.s. "Silvertown" [*communicated*]: Mr. Benest in his paper mentions the probable existence of a subterranean river outlet under the sea off Pescadores Point, Peru. In connection with this part of the coast, I had a remarkable experience while in command of the West Coast of America Telegraph Company's repairing steamer "Retriever." During a repair to that company's cable on the 4th March, 1884, in 650 fathoms of water, 11 miles off Pescadores Point, and while picking up towards the break, and when close to it, the cable came up completely rounded with twigs and branches of olive trees to such an extent that we had to send men over the bows with axes to clear it away, to allow the cable to come in over the bow-sheave. On continuing to pick up, the cable parted under great strain, the end being no doubt buried. The Ocoña River, 13 miles

north from the position of the break, does not flow into the sea, but into a basin or lagoon a quarter of a mile from the sea, and during heavy rainstorms in the mountains this river is transformed into a torrent carrying everything with it, and, rapidly pouring into the basin or lagoon, raises the water surface above the sea level, and no doubt when a certain pressure is reached forces its way out through some passage under the sea, causing an upheaval of the bottom; and when the pressure is relieved by the water in the basin or lagoon again reaching the sea level, a subsidence of the sea bottom takes place, carrying the bight of the cable with it, and eventually breaking it with the downward weight and pressure. The nearest river outlet flowing into the sea is the Quilca River, 50 miles E.S.E. from Pescadores Point. The currents on this part of the coast are variable and influenced by the wind, sometimes attaining a rate of from 1 to 1¼ knots per hour. This cable was again interrupted near the same position on the 23rd of March, just 20 days after the last repair. In my opinion, the break in the South American cable in March, 1895, may have been caused in this manner. Capt. Morton.

Mr. H. D. WILKINSON [*communicated*]: Mr. Benest has given us in his interesting paper a great deal of information on these remarkable depressions in the ocean bed near Cape Verde. The evidence so carefully collected by depth soundings and samples of bottom seems conclusively to point to an individual and distinct origin in the case of each of the two furrows where the breaks occurred. The deposits are different in each, also one depression is at 350 fathoms higher level than the other, and they are at a considerable distance apart—about 17 nautical miles. Similarly, the lowest depth in the gully formation near the shore is, again, at a different level, namely, about 700 fathoms higher, and at a distance of about 35 nautical miles from the intermediate furrow. We may therefore consider them as three distinct causes of disturbance. The shape of the contour lines appears to indicate that the disturbing cause in each case is an eruption through the earth's crust at the sea bottom, and might be either volcanic or the forcing open of a submarine river outlet. The fact of the lower level of the furrows being maintained in a Mr. Wilkinson.

Mr. Wilkinson.

direction away from the shore, while rising ground exists all round them on the shore side, points unmistakably to the disturbing force acting in a direction away from the shore. That is to say, the eruptions would not be vertical, but would shoot out of the earth's crust in a nearly horizontal direction, and in a direction *away from* the shore. The bulk of the evidence seems to me, therefore, to strongly support the view put forward by Mr. Benest, namely, that the disturbing causes are eruptions of *débris* resulting from fresh-water streams forcing their way through the earth's crust. The mountains inland forming the watershed for the great Senegal and Gambier Rivers would supply plenty of head to cause such eruptions were the positions of bibulous land and rocks favourable.

Speaking of *débris* affecting a cable, it has been my experience that this, when discharged with force, has a most destructive effect. I was engaged at one time in repairs to the old Rangoon cable, in a locality where faults and fractures frequently occurred. This was near the Krishna Shoal, about 60 miles from the mouth of the Rangoon River, and in a region near where a large arm of the Irawaddy River debouches into the sea. The river carried down all kinds of *débris*, which was washed against the cable with destructive effect.

The information Mr. Benest has conveyed to us in his paper will be of great utility in future surveys, and comes at an opportune moment. We are warned not to conclude always that, because we get a few soundings of soft mud and ooze in one locality, that locality is quite satisfactory. Especially when nearing a coast line, sufficient soundings must be taken to satisfactorily demonstrate the presence or absence of such depressions as referred to, and nothing shows this better than plotting the contour lines of the region. So far as the St. Louis-Fernando cable is concerned, its present diverted course lies clear of these destructive localities, and one cannot conceive its future safety endangered from these causes, except if, from any alteration in the force or volume of the eruptions, the ledge should give way.

Mr. Benest mentions that the cable relaid during the first repair was laid with 28½ per cent. of slack. Does not this large

amount of slack cable make it an easier matter for a submarine eruptive force to "screw" it or put a U bend in it, thus increasing the chances of fracture? Mr. Wilkinson.

I would also be glad if Mr. Benest would tell us the type of cable used in this portion of the St. Louis-Fernando cable. From the strain at rest, and the fact that it parted at $7\frac{3}{4}$ tons strain, the type used would appear to be the heavy deep-sea type. If so, a rather heavier type would, I think, be preferable to put in when repairing in these waters.

Aside from the main issue of the paper, Mr. Benest has given us many interesting side-lights on the conduct of operations at sea, such as the cut-in for loop test to localise the fault on the beach at St. Louis, and the temporary utilisation of the St. Louis-Yof Bay section to restore communication on the South American cable, the existing land line taking the coast cable traffic meanwhile. He has also put before us much information on the types of cable used, and the technical details connected with the laying of this cable, which cannot fail to be of the highest interest to members of this Institution.

I wish to add my thanks and congratulations to Mr. Benest for his valuable paper, which to me is one of very great interest, and to add that I heartily share the view held by many members of this Institution that we are greatly indebted to the Silvertown Company for the uniformly ready way in which they are willing to place their information at the disposal of this Institution.

The PRESIDENT: Mr. Benest's paper is one especially interesting to myself, as I happen to have been associated for many years with both of the cables on the West Coast of Africa to which he has been referring. I know too well the heavy expense to which we are occasionally put in consequence of interruptions, some of which are no doubt indirectly due to our imperfect knowledge of the bottom of the ocean and the physical geography of the sea. With reference to submarine fresh-water streams, I may mention that those of which I have personal knowledge are close to the island of Bahrein, on the western side of the Persian Gulf. There is, however, I believe, no doubt that the water which supplies these springs comes from the highlands of Persia on the other side of The President.

The President. the Gulf, some hundreds of miles away. Now, if you have in Persia a case where presumably a considerable stream of fresh water travels a great distance beneath the ocean bed before finding its way to the surface, it is only reasonable to suppose that the same conditions obtain in other parts of the world. The existence of these subterranean streams making their way to the level of the ocean bed may account for many of the irregularities of the bottom.

I am of opinion that many breaks in cables are caused by submarine landslips on a large scale, such landslips or falling in of submarine cliffs being caused by the scouring effects of ocean currents or streams of fresh water forcing their way in localities where the sea depth increases rapidly, say from 500 to 1,000 fathoms; under such circumstances we may expect that occasionally there will be enormous displacements of hard mud, causing sudden alterations in the depth. If the cable is in the neighbourhood of these displacements, it is likely to be fractured by the irresistible pressure of the moving mass, or left suspended for a considerable distance. The outer serving on one side of the cable is said to have been removed. This appearance is consistent with the theory that the cable while being hauled in was pressing against the edge or the face of a steep submarine bank; on the other hand, the resistance experienced might have been due to the fact that the cable had sunk a short distance in the soft ooze of which the bottom was composed. On one occasion in the Persian Gulf nearly three miles of new cable had to be abandoned; this must have been caused by an extensive submarine landslip, as the cable was quite new and had only been down a short time. I should not be inclined to attach too much importance to the presence of *débris* in the neighbourhood of the vessel; floating objects are much more likely to have been carried out to sea by surface currents in the ordinary way, than to have found their way through any subterranean channel.

With regard to Mr. Preece's remarks about soundings, we may take it for granted that every cable engineer recognises the importance of acquiring a general knowledge of the route over which it is intended to lay a cable; more than this can hardly be expected.

The President.

There must always be a slight element of risk, but even with the imperfect knowledge we possess we have done pretty well in the past, and no doubt shall do equally well in the future. We know that the ground off the mouths of large rivers is dangerous, and when possible we avoid it, or take special precautions. Variations in depth which on a chart look formidable do not appear so serious when the contour of the ocean bed comes to be plotted out. I am informed that previous to the laying of 1865 and 1866 Atlantic cables only about 30 deep-sea soundings were considered necessary. A glance at the more recent charts of the Pacific Ocean will show the immense amount of work which has already been done to ascertain the suitability of the bottom for the proposed cable; speaking generally, I see nothing that should lead us to anticipate that more trouble would be experienced than is the case with any other of our present long ocean cables. Although I do not think we have sufficient evidence to establish the fact that submarine rivers exist capable of conveying objects of considerable size, it is, on the other hand, equally clear that an enormous quantity of fresh water finds its way underground to the ocean.

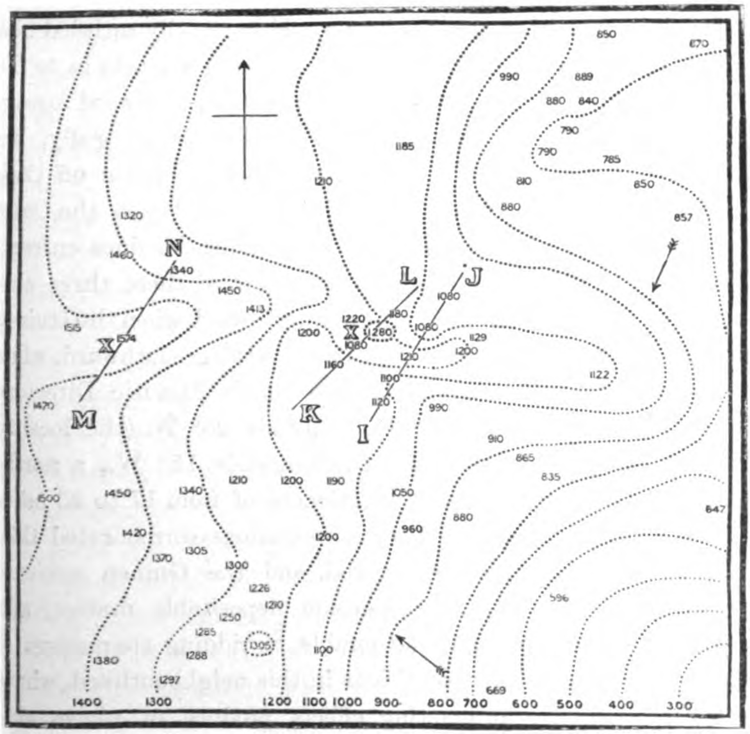
You have on the Gold Coast, about longitude 4°, a gully which has been referred to as the "Bottomless Pit," but there is no surface river emptying itself into the sea, nor is there any record that such a surface river ever existed. Nevertheless, you have the gully, and I think it may be accounted for if we assume that fresh water is constantly disturbing the bed of the ocean, the mud or ooze being carried gradually out to sea. If such conditions exist off the mouth of the Congo, they no doubt materially assist the scour of the river in forming a deep and well-defined gully, extending out to sea for a very considerable distance. It has been remarked in the course of the discussion that the telegraph cable is a most valuable agent to assist us in improving our knowledge of the physical geography of the sea. It is to be hoped cable engineers, when in the course of their work they notice any phenomena, or become possessed of information likely to assist the cause of science, will take advantage of their opportunity and record the particulars for the benefit of those interested.

The President. I have only now to ask you to pass with acclamation a hearty vote of thanks to Mr. Benest for his paper. Unfortunately, time will not permit of his replying to your criticisms this evening. I must ask him, therefore, to kindly submit in writing any remarks he may wish to make.

The resolution was carried with acclamation.

Mr. Benest. Mr. H. BENEST, in reply, said: By way of prefacing my reply, which I am sure I shall not be able to get through this evening, I think some further explanation is due to the meeting as to the reason for adopting what may be considered an unusual expedient in carrying out repairs to a cable. If you will glance at the chart, you will see that this cable between Yof Bay and St. Louis runs parallel to the coast. There is also a land line, so that the cable could very well be dispensed with for a time. The opportunity was therefore seized to join through this important Atlantic cable at the earliest possible moment, and the result was that communication was established in five days from the time the ship arrived on the ground, in spite of the 48 hours or more of bad weather. If the repair had been carried out in the orthodox manner, no doubt these five days would have been extended to at least three weeks, if not longer; so that the proceeding adopted was manifestly a great gain. As to my reply, I should just like to go through one or two things now, because I have here annexed a map prepared, together with three sections of the bottom (Plate 4). This map shows the vicinity of the breaks, and is an enlargement of a portion of Chart 2, Plate 3, showing the contour lines off Cape Verde, where the operations were carried out during the repair to the cable. Between the first diversion to the westward, and that latest diversion to the eastward, lies the ground on which all this trouble occurred. There are three lines drawn on the annexed map— I to J, K to L, and M to N. There are three sections corresponding—I to J, K to L, M to N (Plate 4). These represent to natural scale the state of the bottom, parallel with the line of cable. I want to make it perfectly clear that there can be no question of the suspension of the cable between any points on the bottom there. The dark shading I have put to represent what

may have been a subsidence; to what cause that subsidence is due I cannot pretend positively to assert, but that such Mr Beneest.



subsidence may have occurred I think extremely probable. At any rate, I think it will be clear to all the telegraph engineers and captains present, that there could be no possible question of the suspension of the cable between any points thereabout. The contour lines on Chart 2 I should like you to look at carefully, because to my mind they convey a great deal. Looking at Chart 1, with the soundings upon it, one might think that the bottom there is pretty level generally, with gentle undulations; but when the contour lines are drawn in, it presents a totally different aspect. It shows a series of mud terraces taking the form of an elongated amphitheatre, at the bottom of which, in a sort of arena, this manifestation of natural forces takes place against a product of mechanical skill in the shape of a telegraph cable, in which the

Mr. Benest. cable comes off very much the worse. That is as far as I can go in the description of the chart, the contour lines, and these sections. Probably, had Admiral Wharton been here to-night, the chart I have now been able to show would have fully satisfied him in one point that he raised. Mr. Buchanan's remarks as to the indraught of sea water as under-current, and the outward stream precipitating the silt on each side of the Congo gully, are suggestive to my mind of a somewhat similar condition off Cape Verde at the point of meeting or divergence between the Coast current, the northern equatorial current, and the Guinea current. From latitude 10° N. to 17° N. the resultant of these three currents is variable, at times setting to the northward, when the Guinea current is strongest, but more frequently setting southward, when the coast current is strongest. The North Atlantic Directory states that from latitude 15° N., longitude 20° W. (the locality of the breaks), to latitude 5° N., longitude 15° W., a north-westerly set at times has been experienced of from 17 to 23 miles daily; during the operations our observations corroborated this. The coast, the northern equatorial, and the Guinea currents having each doubtless in suspension depositable matter, and only waiting for conditions favourable to ridding themselves of their burthen, find these conditions in this neighbourhood, where their varying and counteracting effects produce in places still water, which enables gravitation to assert itself and deposits result. Such a deposit of sediment imbedding a cable, and by spreading or sliding over ground, may throw sufficient strain upon it to break the cable. The remarkable difference, mentioned in the paper, between the muds on different lines of soundings affords evidence in this direction. These masses or ridges of mud, whatever form they may take, could be broken down and dispersed by the back-wash of great waves during the prevalence of heavy surf, and the effects so destructive to the cable might be brought about by the power of the coast current and this transmitted wave-force. Enormous deposits must be brought down the Senegal River, navigable for 700 miles, and I believe that nearly the whole of the bottom in this vicinity consists of terriginous material from this source; but this does not necessarily disprove the existence of

other outlets below sea level on the coast near to Cape Verde, nor of artesian outbursts capable of disturbing the sea bed and causing subsidences. Mr. Benest.

In continuing my reply to Admiral Wharton's remarks, having by charts and sections shown that the cable could not have been broken by suspension, I now give figures to prove that the impressions on the cable, such as its flattened and twisted condition, core squeezed out and cut, &c., could not have been caused by simply hauling it up, as the gallant Admiral appeared to think.

The weight of the cable in sea water is 1·12 tons per N.M.; the depth at the 1895 position was 1,574 fathoms; so that the weight of cable from ship to ground would be 1¾ tons only. The breaking stress of the cable, without the outer cords, was, when new, 9⅝ tons, and the length of itself it could support in the sea before breaking = 8½ N.M.

The average strain while picking up had been from 3½ to 4 tons when the cable was imbedded in mud. It sustained no injury from this, nor at a strain of 5½ tons, beyond becoming a bit "screwy," when breaking out the ends on the 1893 repair. On the 1895 repair, one end of the cable could not be extricated at all; and the other side was finally recovered after considerable time and trouble, with a strain often touching 8 tons. This would never happen in pulling a cable out of a plastic mud. The mangled condition of the end itself serves to prove the existence of hard material in great mass at the locality of the 1895 rupture.

Mr. Bright could not understand why the fact of the indicated strain falling after the cable was at the bows to 6 tons, and then to 5 tons, should prove that sufficient slack had been originally laid.

A cable is held, when down, by its own weight plus the friction of its bed, and there is, on raising it from the ground, a pull at the bottom tending to draw the cable towards the bight until the slack is exhausted. With practically 10 per cent. slack (9·9 recorded), and the cable lifted from 1,250 fathoms (depth recorded in this case), the tension on the bight would be over 5½

Mr. Beneat. tons, and the length of cable off the ground slightly over 7 nautical miles. The weight of this cable in sea water is 1·12 tons, which, multiplied by 7 (length in nautical miles off the ground), = 7·84 tons, representing strain on the grapnel rope. The strain recorded on the dynamometer, in this case, was 7 tons, and strain on cable was approximately estimated at 5 tons; thus proving, from what has been above stated, that sufficient slack had been laid originally along this portion of the route.

Mr. Bright criticises the speed for the ship whilst paying out cable. As I shall refer to this later on, it need only be remarked now that the rate was not considered high by the engineer representing Messrs. Clark, Forde, & Taylor—Mr. R. E. Peake—who accompanied the expedition in the interests of the South American Cable Company, and followed every detail of the work in the thoroughgoing manner for which he is distinguished.

Mr. Bright raised a question about the small retarding force required in paying out, and as to brake power. The calculated retarding strain for the paying out of the South American cable over the deepest water (2,800 fathoms round figures), for speed of ship = 7 knots per hour, and to lay 10 per cent. of slack, was 44 cwt. The calculated retarding strain for 2,500 fathoms (taking this as the average depth over the deeper portion of the route), at same speed of ship and percentage of slack, was 39 cwt.; but the retarding strain actually applied throughout was less than the calculated.

Comparing this South American cable deep-sea type with the Anglo-American Company's latest cable, laid in 1894 (two years after the South American), in its mechanical detail, we find the weight in sea water 22·4 cwt. South American, as against 22·75 cwt. Anglo-American; the specific gravity 2·07 South American, as against 2·3 Anglo-American; and the B.S. 10$\frac{3}{8}$ tons South American, as against 8·2 tons Anglo-American. A less average amount of slack was laid on the Anglo—viz., 9 per cent.—and the retarding strain applied while paying out at speed of ship 7 knots per hour (same rate as in paying out the South American) was between 17 and 18 cwt. per 1,000 fathoms of depth, or = nearly 44 cwt. for an average depth of 2,500 fathoms.

In depth of 2,800 fathoms the retarding strain would have been nearly 49 cwt. for this latest type of Atlantic cable, which is modelled in mechanical construction on the South American cable type. Mr. Benest.

The absorption of energy by the brake in paying out the Anglo-American cable of 1894 at speed of 7 knots per hour was, it appears, equivalent to over 100 horse-power. The absorption of energy by the brake in paying out the South American cable at nearly the same rate of speed was about 85 horse-power; so that it may be considered a perfectly fair assertion that the light deep-sea of the South American cable requires small retarding force in paying out.

Paying-out gear is designed to meet requirements for various types of cable. Brake power is provided with a margin in excess, for use in cases of emergency; for example, if compelled to stop and pick up in great depths, a holding power of 3 to 4 tons instead of 1½ tons would be required.

Mr. Preece appeared to think that high temperature is an indication of ocean current. This may be so to a slight degree, but the case he cites is not quite apposite. It is true that the mean temperature is something like 35° Fahr. in over 2,000 fathoms depth; and this I mentioned in the paper, where I stated that a mean depth being taken as 2,142 fathoms, the mean bottom temperature is 35° Fahr. The mean depth over that portion which Mr. Preece refers to as having a temperature of 49·9° Fahr. is only 565 fathoms. Doubtless he omitted to notice this fact, and will now understand that the temperature at the bottom here is not in excess of normal.

Regarding the island of Fernando Noronha, the cable is landed there, and there is a station; but cable is joined through and messages are sent between Pernambuco and St. Louis direct, Fernando Noronha station simply cutting in to forward local traffic for a certain time daily.

"*Knot.*"—This is really the oldest and most correct term to apply for distance run at sea, the term *naut* being simply an abbreviation of nautical mile, and of quite recent adoption. The "knot" is an actual measure derived from proper dimensions

Mr. Benest. Old nautical authorities, such as Raper and Norie, have established its right to be thus considered, as much as electrical authorities have done so in regard to units of measurement of electrical resistances.

Heaving the log is by no means an obsolete method of finding the ship's rate through the water. The log-reel and sand-glass are included in the equipment of all ships and steamers. The log-line, as every sailor knows, is divided into a number of lengths; each length bears the same proportion to a nautical mile as the number of seconds run by the sand-glass does to an hour of time. These lengths are marked by knots of yarn tucked through the strands of log-line, necessary marks being added for each nautical mile up to the number required to suit the maximum speed of the vessel; so that the knot has a distinct meaning as applied to the velocity of a ship, or of a cable paying out, the terms "knot" and "nautical mile" being synonymous.

I notice, by the way, that Mr. Preece uses the term "knot" himself in the corrected proof of his remarks as published in the technical journals.

As to Mr. Preece's objections to paying out the cable at speed mentioned, I would say that, apart from the decrease in the retarding strain, the advantages of paying out a cable at a moderate instead of a low rate of speed are obvious, the horse-power absorbed by the brake being considerably reduced, and the cable laid in less time; for example, if the speed of the ship had been kept at the old regulation rate of 5 knots per hour while paying out cable, the retarding strain would have been increased by between 7 and 8 cwt.—an increase in the absorption of energy by the brake of between 5 and 6 horse-power.

In laying, more or less, across a current too, it is manifestly a necessity to pay out at as high a rate of speed as safety in the tank will allow, in order to reduce the waste of cable that would otherwise ensue through the ship being set away from the proper direction.

With regard to Captain Goodsall's criticism, it is surprising to hear an assertion made in public in these days that waves have no effect beyond 20 fathoms depth; but astonishment is turned

to amusement when Captain Goodsall—who, so to speak, scuttles Mr. Benest. the ship of his argument and sends her to the bottom at once—goes on to say that during one of his experiences in the Indian Ocean (presumably in fairly deep water, as the average depth is given, on best authority, at 2½ miles) he found that in the case of a cable with conductor broken and sheathing intact, the communication was interrupted during stormy weather through the cable being moved or swayed about on the ground, and that continuity was restored on the weather becoming calm, and thereby allowing of the broken conductor making contact through absence of movement on the bottom.

As to the climatic conditions, the reason why I deemed this worthy of remark was because I could not account for the confused state of the sea surface in absence of sufficient wind, the greenish tinge of the water, and other peculiar conditions, otherwise than by ascribing these to some action going on below the sea surface. I did not state in the paper that the disturbed state of the sea was the *cause* of the breaks, and it is incomprehensible to me how Captain Goodsall could so interpret my words.

In regard to hollows between higher levels on the bottom, these may well exist without any possibility of a cable being suspended across; a glance at the numerous diagrams of sections of the bottom illustrating the paper should make this perfectly clear. A hollow between a higher level may be considered a correct description for the shallow depressions shown. For sharper declivities I should use other terms.

If all breaks that occur to cables in 1,000 fathoms, more or less, are due to suspension, as Captain Goodsall declares is his opinion, then it is a wonder they do not take place more frequently. I know of one case to the contrary, reported in the *Electrician* of January 24th, 1896, where the late Sir J. Pender, presiding at a meeting of the Direct United States Cable Company, referred to that company's cable having been suspended for 22 years. Notwithstanding this, it had continued to work without interruption; as it was in deep, still water, no harm resulted.

It is remarkable that the speaker who followed Captain Goodsall should—quite unwittingly too—have fairly contradicted

Mr. Benest. that gentleman's beliefs, &c., by the simple relation of absolute facts in connection with the repair to a cable as carried out by the narrator, Captain Lugar, off the coast of Peru, in the neighbourhood of a gully where a submarine river outlet has been proved to exist.

There are points in common in the repairs referred to by Captain Lugar and those I have been describing. This testimony is therefore very valuable, considering that the cables he adverted to and those forming the subject of my remarks are situated so far apart, yet at localities in proximity to land, and where the physical conditions obtaining on the sea bottom appear to be alike.

For a piece of cable inserted in a repair in over 1,000 fathoms depth, on a comparatively level bottom, and with an abundance of slack, to have been fractured inside of five days is, to me, overwhelming evidence that there is movement of some kind on the sea bottom, not only due to current action, but of a character violently destructive, and which follows a well-defined path in a certain direction from the coast, whereon, and beyond the mountain ranges inland, a chain of lakes exist, with their only outlet through a hole in the mountain side.

In reply to Professor Milne, I can only give the day on which the cable broke down on the second occasion, viz., the 10th of March, 1895. The hour was not reported, owing, no doubt, to the Cadiz-Tenerife cable being also interrupted at that time.

Captain Wilson-Barker's idea that hot springs might exist at the bottom of the sea appears to support rather than to oppose my suggestion of underground rivers with submarine outlets; because fresh water from an inland source, impregnated or not with chemical or mineral matter, may either percolate through porous strata, or may flow through subterranean channels and break ground under the sea. I would point out, in reference to this, that no sign of erosion due to chemical action was found on the cable.

Mr. Donovan spoke of decomposing animal or vegetable matter giving off sulphuretted hydrogen being the cause of mischief to cables. Injury from this cause is frequently found to have occurred in depths of from 300 to 400 fathoms, and under.

Vegetable life is of course confined to shallow depths, and in

cases where cables were laid through algæ their lives have generally been of short duration, partly through these being found principally in rough ground, but no doubt partly due to the exhalation of oxygen by these submarine growths; this oxygen increasing the usual amount of free oxygen to be found in sea water, and causing a rapid decomposition of the iron sheathing. Mr. Benest.

It could not be said that the causes suggested in reference to the breaks off Cape Verde would be applicable to such a case as is cited by Mr. Donovan in one of his West Indian experiences, in which the probable cause of injury to the cable may have been suspension between irregularities upon an uneven and ragged bottom, with extremely slow motion imparted to the cable by the action of under-currents.

Mr. Buchanan, in his account of oceanic shoals discovered in the "Dacia" in October, 1883, tells us that "between Grand "Canary and Tenerife, in over 1,000 fathoms depth, the tidal "current reaches to the very bottom, which shows scouring action "by the absence of all deposit, the bottom being rock or coral, "often coated with black oxide of manganese."

Captain Thomson spoke of the possibility of a cable, already weakened, being broken by the vibration of a heavy surf acting upon it for several hours continuously. This is a suggestion in which I fully concur; such vibrations would, no doubt, put the *coup de grâce* on a cable enfeebled by some other destructive agency.

Mr. Gray, in the course of his remarks, adduced strong evidence of the existence of subterranean lakes and caves, and gave cogent reasons why these water conduits having their outlets under sea level may be the cause of many cable interruptions that occur during rainy seasons, and especially in the month of March. I am not quite in accord with Mr. Gray in his general conclusion that March is a rainy month "in the tropics, as elsewhere." Captain Lugar told us that March and April coincide with the time of the heaviest of the rainy season in the Cordilleras and Amatape Ranges, situate between 4° and 5° S. latitude in Northern Peru. This region may be considered inter-tropical, and the rainy season, according to Dampier, obtains all the year round; and it rains abundantly, there being little difference in the

Mr. Benest. wet and dry seasons, only in the dry the rains are less frequent and more moderate than in the wet season, for then it pours as out of a sieve.

The belt of calms, commonly termed "doldrums," separating the two trade wind zones follows the sun in his annual course. This belt has a mean average breadth, according to Maury, around the globe of about 6° of latitude. The limits generally pass from one extreme to the other in about three months. The whole system of wind and calm belts moves northward from the latter part of May till August.

In the sub-tropical regions of Africa the rainy seasons are governed by this movement, and in Senegal the rains do not arrive until June, and cease about October.

When the sun is making north declination, we may therefore say generally that rains fall heavily over those regions included in the passage of the calm belt following.

The configuration of land surface of continents will, however, considerably modify or alter these conditions.

Mr. Rymer-Jones has added a little more to the detail of that part of the paper which refers to the break of 1895.

I cannot, however, agree with him in the conclusion he arrives at as to the cause of the break. I do not attribute the twisting or untwisting of the cable to its having been carried downwards on an inclined plane. The sections of the bottom have shown clearly that inclined planes sufficiently pronounced to lead to this result are not present. Of course subsidences might cause such inclines locally. It may not be generally realised that a cable pinned on the ground, and with a shifting body tugging at it in a more or less vertical direction, is somewhat analogous to a stone at the end of a string held in the hand; the stone in this case being the ship, which, in her endeavours to extricate the cable, turns round and round on the surface above, and is moved ahead and astern, the cable being meanwhile tautened and slackened alternately, to assist it in clearing rocks or obstructions on the bottom. When it is remembered that two and a half hours were spent in getting in the last mile, it can be readily understood that the ship had turned round probably a few dozen times, in this case opening, or "bird-caging," the sheathing wires at the bottom.

Mr. Beneat.

Under such conditions, which would allow of the core protruding, a sudden relief consequent on a withdrawal of the cable from the *débris* covering it would cause the hard steel sheathing wires to close like a vice upon the protruding core and sever it in the manner seen.

The fact that the angle of the cut where the core is divided corresponds with the angle of lay of the sheathing wires affords the strongest proof that a sudden closing of the outer armour on a portion of the core protruding was the means of severing it.

Mr. Wilkinson remarked upon the large percentage of slack laid during the first repair, and asked whether this would not cause trouble in the cable on the bottom in case of eruptive force. I should imagine that in such an event, whether there were much or little slack, the result would be the same. If a U bend were suddenly converted into a curl or "half-crown," it would, upon a strain coming upon it, be drawn into a hard kink, which would of course increase the chances of fracture.

The type of cable used in the repair was the light deep-sea.

The reason that it parted at 7¾ tons strain was that the cable had been weakened at the "nip" between the bow sheaves and the dynamometer by frequent heaving in and slacking out, in order to clear it from the ground. I cannot agree to Mr. Wilkinson's suggestion that a heavier type of cable should have been used during the repairs, principally on account of the depth of water.

Having answered as far as I am able the different points raised by the various speakers, it is evident that to say any more upon the subject of the cause of these two breaks would be to go over the same ground again. The discussion, to me, has been highly interesting, and valuable in many respects in throwing some light upon causes of cable ruptures generally; but I must leave to others more competent to judge, the task of pronouncing upon the extent to which we have elucidated the origin of the trouble leading to these two particular breaks.

My thanks are cordially tendered to those gentlemen who have favoured me by taking part in the discussion, which has, upon the whole, been favourable; and I think it is fortunate that

Mr. Benest. the paper has had the advantage of being criticised by so many of those best qualified to pronounce upon it.

I must, however, express regret that, after every endeavour was made to invite criticism, some who have for years been connected with work of the kind treated of did not respond to the courteous invitations conveyed to them by our genial Secretary, and attend the reading of the paper, or the discussion; but I must conclude, I suppose, from their absence, that they probably had no opinions to offer, or that they acquiesced in the general ideas set forth.

It is to be hoped, at all events, attention having been directed to this interesting question, of such importance to cable companies, as pointed out by Mr. Gray in the course of his remarks on the paper, that cable engineers, and captains, in charge of repairing operations in different parts of the world, will evince a burning desire to prove, or disprove, the suggestions I have put forward, and that they will communicate the results of their observations to the Institution, for the benefit of all concerned.

The PRESIDENT: I have to announce that the scrutineers report that the following candidates have been duly elected:—

Foreign Member:

B. L. Vogels-Dolhain.

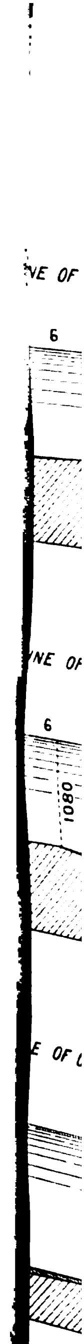
Associates:

Charles Frederick Clapham.	Thomas Henry Lloyd.
Charles Philip C. Cummins.	John Francis McMahon.
John George Griffin.	Hans Jacob Pheodor Norballe.
John Louis Hermessen.	Frank C. Porte.
Albert Higgins.	George Ward.
Bertram Hopkinson.	Henry Edward Allan Wiggett.

Students:

Arthur B. H. Cope.	Wilfrid John Murphy.
George William Harris.	Mark Packer.
Charles Jones Lockyer.	John Price Trotman.

The meeting then adjourned.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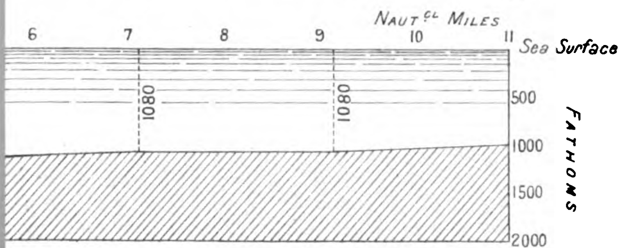
6
1080

Plate 4.

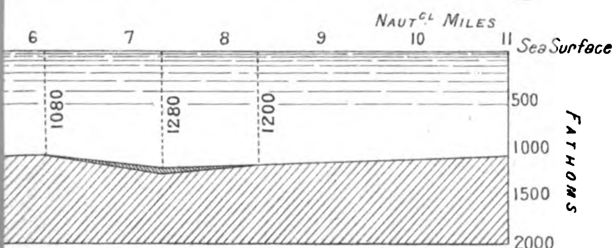
NE OF CABLE CLOSE TO BREAK OF 1893.

J



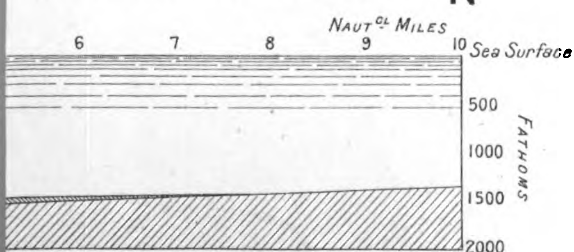
INE OF CABLE CLOSE TO BREAK OF 1893.

L



E OF CABLE CLOSE TO BREAK OF 1895.

N



ABSTRACTS.

M. G. BIGOURDAU—ON THE COMPARISON OF THE DURATION OF OSCILLATION OF TWO PENDULUMS REGULATED APPROXIMATELY TO THE SAME PERIOD.

(*Comptes Rendus, Vol.* 124, *No.* 6, 1897, *p.* 279.)

The author refers to the method described by M. Lippmann (*C. R.*, vol. cxxiv., p. 125), which necessitates the use of electrical apparatus. This in some cases is not altogether convenient. The method described by the author possesses greater accuracy than is necessary in determining the force of gravity. This method depends also on the illumination of the pendulum for a very short period, under which condition it appears stationary. In practice, a determination of the force of gravity would not take longer than two or three days, but this would not be sufficient time for a clock to get into regular working order.

Chiefly for this reason, and also for convenience, the author makes use of a simple chronometer, which produces the necessary flash for making the observation.

For this purpose, the balance-wheel carries parallel to its plane, a very light disc, in which a slot about 1 mm. wide has been made. The case of the chronometer has a hole in it, in a line with the above slot, when the balance-wheel is in its stationary position; consequently, at each oscillation, the slot passes before the opening in the case, and if a lamp be placed at the back, a flash will be seen at each oscillation; and this flash illuminates the index of the pendulum. In order to make this flash as intense as possible, a system of lenses is employed.

With a slot 1 mm. wide the flash lasts about 1-400 second; and it can be easily observed in daylight. The author states that the flash might be reduced to 1-1,000 second. This system has been practically applied since last May.

CH. HENRY—ON A NEW PROCESS OF ELECTRIFICATION.

(*Comptes Rendus, Vol.* 124, *No.* 6, 1897, *p.* 279.)

M. d'Arsonval has shown that electrification by alternating currents, the curve of which follows the sine law, presents great advantages over the ordinary system of electrification by means of the induction coil; at the same frequency the former have greater internal effects, without causing as much contraction or pain.

The author was of opinion that, according to Fourier's and Helmholtz's theorem on sound harmonics, it would be possible to transform a succession of musical sounds into alternating currents, to which the nerves and muscles of the system would respond.

The apparatus employed in these experiments consisted of a Gülcher thermo-electric pile, giving an E.M.F. of 4 volts after several minutes' heating; the source of sound consisting of a musical box. In order to ascertain the physiological

influences of rhythm and of time, these are compared with the physiological effects which are produced when the subject is electrified with alternating currents of the same mean frequency. For this purpose the author employed a siren, placed in front of the microphone, the source of sound being out of hearing.

On the resonator of the musical box is placed a four-carbon Hughes microphone, connected on one side to the positive pole of the pile, and on the other side to one of the terminals of the primary circuit of the small coil of a D'Arsonval-Bert telephone, the other terminal of the same primary circuit being connected to the negative pole of the pile. The two terminals of the secondary circuit of the coil are connected to the organism by the wires and the exciters employed in electro-therapeutics. The vibrations of the microphone act as interruptor to the coil.

A series of shocks are felt in the muscles representing the transformed musical phrases, with all their variations. In a future communication the author hopes to resume the results of an electrometric and physiological investigation in connection with this new method of electrification.

AUG. CHARPENTIER.—ON IRRADIATION COLOURS IN LUMINOUS EXCITATIONS OF SHORT DURATION.

(*Comptes Rendus, Vol.* 124, *No.* 6, *p.* 305, *February*, 1897.)

About a year ago the author published results, under the title of "Irradiation "Ondulatoire," showing that, once a luminous impression has been produced at a given point in the retina, it is propagated from point to point in all directions.

The author has observed this phenomenon not only in the case of moving luminous objects, but also with fixed luminous excitations. To these observations was also added an important characteristic, viz., that the luminous trails observed in these experiments, show colours in the order of the spectrum, commencing with the red side. Since the above publication, the author has continued his researches on this phenomenon, and on its relation to the successive colorations which the luminous object itself presents from the moment of its action on the retina. The publication of certain phenomena observed by Mr. Shelford Bidwell (10th and 17th December, 1896) on the Benham top, led the author to somewhat prematurely publish his own results, showing the connection between the subjective colours so much talked about, and the laws already established by the author's previous researches. If a small luminous object be displaced on a black ground, under the conditions previously mentioned (February, 1896), the circular region is seen to be surrounded by light trails, their size and their distance from the object depending on their velocity on the retina.

At first these trails appear to have nothing special, but after a little attention, if the illumination is sufficiently strong, and especially if the eye is fixed on a point fairly near the passage of the object, these trails are seen to be coloured. Red is the initial colour forming the front part of the wave; the other colours follow in succession This experiment is fairly delicate, on account of the particular difficulty in observing objects in motion. Much simpler, however, is the observation of colours emitted by a fixed luminous object, when it is made to produce a short excitation on the retina.

The coloured zones which are observed are of a complex nature, several phenomena being involved in their production. The phenomenon under consideration becomes more marked by keeping the eyes steady, and by varying the time of illumination. This is effected by means of a system of rotating discs.

It is, however, simpler, and often sufficient for the purpose of producing short excitations, to displace a large opaque screen before the eye, in which a fairly narrow slit several millimetres wide has been made. By thus watching a candle against a dark ground, its appearance changes according to the duration of excitation—the duration of the passage of the slit before the eye. The above phenomena are analagous when other sources of light are employed—Auer flame, gas flame, electric, incandescent, or arc lamps. The author has noticed, with regard to the halo, that an increase in the luminous excitation is equivalent to an increase in the duration; also that an increase in the dimensions of the source, or its being brought nearer, causes the coloured zone round the flame to be increased.

JEAN and LOUIS LECARME—APPARATUS FOR REGISTERING THE VELOCITY OF PENDULUM MOVEMENTS.

(*Comptes Rendus, Vol.* 124, *No.* 7, *February*, 1897, *p.* 336.)

In July, 1896, the authors published a method for directly recording the resultant motion due to the combination of a certain number of pendulums.

The following apparatus was employed for recording the velocity at any point along its path:—

To the end of a beam, balanced by means of a counterweight, is fixed a steel pointer carrying a style, to which a vibratory motion is imparted by means of a special electro-magnet. The velocity of the beam can be altered by the displacement of the counterweight, thus modifying the moment of inertia of the system.

The amplitude of vibration of the pointer is regulated by altering the duration of contact by means of a screw. Under normal conditions the number of vibrations may between 50 and 500 per second, which may be measured either by a graphical method or by the pitch of the sound produced. Under working conditions, the impacts of the point on the glass produce a dotted line. The latter is only regular so long as the weight of the system on the point, which is reduced to several decigrammes, remains constant. By measuring on the dotted curve, the distance between the points, a series of numbers are obtained which are proportional to the velocity at each instant. These curves are traced on slightly smoked glass, or, better still, on polished steel.

This method enables a more complete study to be made of pendulum motions.

A. CHARPENTIER—THE CHANGE OF COLOUR OF QUICKLY CHANGING LIGHTS IN TERMS OF THEIR DURATION.

(*Comptes Rendus, Vol.* 124, *No.* 7, *p.* 356.)

The author's results published in February last, showed, with regard to the excitation on the retina of a succession of irradiation colours around the image of

a flame, that the phenomenon became complicated by the changes of colour of the ground on which the object is defined. The author discovered the law that, all things being equal, the less refrangible colours are the ones to appear first. The illuminating flame mentioned in the previous paper will appear reddish, and with longer exposures will tend towards yellow, green, &c.

These changes of colour are observed especially in the neighbourhood of the smallest perceptible exposures. The effects vary through wide limits, and depend on the intensity of the light and on its retinal value. They are generally of an order below 1-100 second. The author draws attention to the fact that the law of these phenomena is exactly opposite to that of Purkinge, relative to the influence of light intensity on colour effects. These two influences may counterbalance one another to some degree, but it is clear that in the neighbourhood of the shortest perceptible exposure the factor of time will alone remain in play. White light becomes, indeed, of a reddish colour under these conditions.

A strong example of this phenomenon was given by the following experiment, suggested by Exner:—If at a small distance from the flame of a candle or lamp be placed a large sheet of ground glass in a vertical position, this will show a series of concentric zones of gradually decreasing intensity. Then, if a screen or rotating disc in which a narrow slit has been made, is displaced before the eye, these zones will appear to be coloured in the order of the spectrum. The red colours will appear at the periphery—since the luminous and effective intensities are least at this place—and the spectrum colours will follow towards the centre.

M. VASCHY—A STUDY OF VARIATIONS OF ENERGY.

(*Comptes Rendus, Vol.* 124, *No.* 6, *February*, 1897. *p.* 284.)

When any forces, such as a weight, pressure of a fluid, electric forces, &c., are applied to an elastic body which they deform, and these forces are balanced by elastic forces, then the work performed in deforming the elastic body is transformed into elastic energy, distributed through the body; there being no other variation of energy. If, however, the work done by the forces is negative, then the elastic energy, w, may become *nil*, and then positive, but never negative, $w = 0$ corresponding to the absence of deformations and of elastic forces. In an electric or magnetic field of constant potential, a simple displacement of the bodies without deformation of the bodies submitted to the electric or magnetic forces gives rise to positive or negative work, which constitutes a loss or gain of energy of the field, represented by the formula, $\delta\tau + \delta w = 0$.

This, however, is not the case when the whole field ceases to be of uniform potential (see author's previous work, 1st February, 1897). The author remarks that it is sometimes objected that a variation of heat taking place in the phenomenon is neglected, when calculating the variation of electric energy, δw, by means of the above formula (applicable to any form of energy). A body compressed by a force undergoes a rise in temperature without variation of heat, given by the well-known formula,

$$d\,Q = 0 = c\,d\,t + l\,d\,v.$$

It is certain that this rise of temperature may produce a more or less slow transfer of heat from the body to the surrounding medium. This, however, is a secondary phenomenon, due to the conductivity of the body, and without influence on the electric or magnetic elastic phenomenon which is produced. The same holds good for all secondary effects, notably in the spontaneous transformation of electric energy into heat (rapid in lead, and much slower in the metals said to be perfectly elastic).

P. JOUBIN—ON THE DIMENSIONS OF ELECTRIC AND MAGNETIC QUANTITIES.

(*Journal de Physique, February*, 1897, *3rd Series, Vol.* 6, *p.* 57.)

The author has recently shown (*Jour. de Phys.*, 3rd series, 1896, vol. v., p. 390) that the dimensions of electric and magnetic quantities could be completely determined, as a function of the fundamental units of rational mechanics, on the condition of admitting a postulate. The author proves that the formulæ of the dimensions arrived at by this means, are exact, without making any restrictions.

He assumes that A is a value depending on several variables, $x, y, z \ldots\ldots$, and supposes that the form of the function which connects A to these variables is known and can be expressed in the form, $A = C\,\phi\,(x, y, z \ldots\ldots)$, C being an absolute constant, since it contains none of the variables, and the dimensions of A are, owing to the necessary homogeneity of the formula, the same as those of the function ϕ. The author applies this principle to several examples: the Hall phenomenon—the Piézo electricity of quartz—rotatory magnetic power—conductivity.

M. G. WEISS—EXPERIMENTS ON TWO PHENOMENA PRODUCED BY THE PASSAGE OF A CONTINUOUS CURRENT THROUGH ORGANIC TISSUES.

(*Journal de Physique, 2nd Series, February*, 1897, *p.* 72.)

Experiments performed by the author on frogs and guinea pigs, show that continuous currents of sufficient strength passing through the muscles for a sufficiently long time produce very marked lesions in these muscles. These are visible through the microscope, and lead to a progressive atrophy. Clinical facts observed in the human system are explained by the author's experiments, which show that these effects are produced only by unidirectional currents, and never by alternating currents. The passage of continuous currents through the muscles is accompanied by polarisation phenomena, as was shown by the author several years ago. These experiments were, however, not suitable for measuring polarisation, the measurement necessitating two readings:—

1. The first operation produced a polarisation of the electrodes and of the tissues through which the current passed.
2. And, secondly, by dipping the electrodes into salt water, the polarisation of the electrodes alone was obtained.

The polarisation of these tissues was then given by the difference between the two readings.

The author has now adopted Chaperon's modification of the above method, which only necessitates taking one reading. The organism under experiment dips into two vessels containing liquid, through which current is passed by means of tin plates. The reading is taken by first charging a condenser, and then discharging through a galvanometer and reading the deflection.

This method was found convenient, and enabled the author to make a series of investigations.

He found, when operating on a frog with a current of increasing strength, that the polarisation increases with the current, and attains a maximum value of about one-fifth volt.

Experiments on the human body showed that with the same current it is polarised less than the frog, but that the maximum polarisation was much higher with the latter. The explanation given for this, is that with equal currents the polarisation of the muscles is proportional to their length and inversely to their section.

The author compares a muscle to a mass of particles which polarise, thus acting as small accumulators and forming a series of batteries.

The following experiments were then made to show that at the surface of separation of two mediums, out of contact with the electrodes, there may take place chemical decompositions, explaining some of the above facts. In the first case gelatine was run into a U tube, and when this was set, the remainder of the tube was filled up with salt gelatine. The ends of the tube were then placed into two water vessels, by which the current is sent through the gelatine. When the current is established the two gelatines are seen to become liquid at their surface of contact. This phenomenon is due to the liberation of acids and bases. A further experiment consisted in filling a tube with white gelatine in the centre of which was a ring of gelatine coloured with aniline. When a current was sent through the tube the colouring matter was seen to be displaced under the action of the current.

This effect is produced with very weak currents, and a displacement of 6 to 7 cm. per day has been observed in the case of methylene blue, in a tube of 1·5 cm. square section, with a current of 1-25th milliampere.

All colours are not displaced in the same direction. The direction of displacement is connected with other properties. Experiments were made on a series of so-called basic colours, and also on a series of acid colours. It was found that all basic colours were displaced from the positive pole to the negative pole, and all acid colours from the negative pole to the positive pole. The author suggests that these phenomena are caused either by a transfer of colouring matter (salt), or by a series of successive decompositions.

If the latter hypothesis were correct, it would constitute the first experimental demonstration of Grotthus's hypothesis.

A. MOUTIER—THE TRAMWAYS OF BUDA-PESTH.

(*L'Éclairage Électrique, Vol.* 10, *No.* 7, *p.* 299, *February*, 1897.)

Both horse traction and electric traction are used for tramways in Buda-Pesth.

The electric tramways are worked by a mixed system of underground conduit and overhead trolley.

This city has also an underground electric railway. In the heart of the city some of the streets have no drains. In such streets no electric traction is employed, owing to the risk of flooding the underground railway, and also because the trolley system is not tolerated in the city itself. The underground electric railway has a double track and a normal gauge. The greatest gradient is 20 mm. per metre, and the smaller radius is 40 metres.

The total length of the line is 3·8 kilometres, along which there are eleven stations, nine of which are underground, while the last two are in the open air, in the woods.

The foundations and walls of the underground railway are built exclusively of concrete, the roof being constructed of iron and concrete.

The available height of the tunnel is 2·75 metres. This had to be made as small as possible, in order to avoid the drains.

The ironwork of the roof of the tunnel is supported by iron pillars. A portion of the tunnel was tested, 28 days after construction, in the following manner:—

1. A load of 5,000 kilogrammes, distributed over an area of 150 × 150 millimetres, was applied to the middle of the arch of the tunnel. No deflection was recorded on the Vernier apparatus.
2. A gradually applied load of 14,400 kilogrammes per square cm. was applied between two transverse girders. This gave rise to a deflection of 5 mm. in the centre of the girders, and to a lateral displacement of 8 mm.

The removal of 150,000 cubic metres of earth was performed by means of electric machinery supplied by Messrs. Siemens & Halske, which firm also provided the electric concrete mixing machinery. The construction of the tunnel involved the use of more than 3,000 tons of iron.

The rails used are 115 mm. high and 9 metres long, weighing 24 kilogrammes per metre run.

The stations are arranged in the same manner as on the London Metropolitan Railway. The platforms on each side are 3 to 8 metres wide and 24 to 32 metres long. The charge is twopence for any distance. Automatic ticket distributors are being used.

The whole of the electrical work of the railway was carried out by Messrs. Siemens & Halske. Two naked conductors carried on insulators are fixed to the roof of the tunnel, above each track, and to which contact is made by means of runners fixed to the roof of the car; the latter being only 15 cm. from the roof of the tunnel. In certain places the overhead cables are replaced by bars of copper. The motors work at a pressure of 300 volts. Special circuits are run for lighting purposes. The automatic block system of signalling is employed, but in case of a failure the telephone can be used. The cars are so designed, as to stand only 0·50 metre above the rails, which leaves a free height of 2·085 metres inside the carriage.

These cars are about 11 metres long, with motors and bogies at either end. The driver always occupies the front part of the train, which necessitates the use of two completely equipped cabins.

The motors have each an output of 50 H.P., and are capable of driving the car at 20 to 25 kilometres per hour. Each car can accommodate 28 seated and 18 standing passengers.

The rolling stock consists of 20 cars, of which 14 are in use simultaneously between the hours of 6 a.m. and 1 a.m. on the following day.

This line cost 7,300,000 francs, or about 2,000,000 francs per kilometre.

M. GUGGENHEIMER.—ON THE INFLUENCE OF THE RÖNTGEN RAYS ON THE EXPLOSIVE DISTANCE OF THE ELECTRIC SPARK.

(*Comptes Rendus, Vol.* 124, *No.* 7, *p.* 359, *February*, 1897.)

The author refers to the discovery, first made by Hertz, that if a spark is made to pass between the two balls of a micrometer, then the explosive distance for the same difference of potential is greater if the micrometer is under the influence of a source of light emitting ultra-violet radiations, than if it were in the dark.

He observed especially, that the increase in the explosive distance was very considerable if the light were supplied by another discharge spark, that the effect decreased with distance, and that this effect was due to radiations having a smaller wave-length than the last parts of the ultra-violet spectrum.

The author suggests that it is probably M. Eilhard Wiedemann's "Entladungs-"strahlen" which produced the effects observed by Hertz. The author felt that it would be an interesting contribution to the theory of the Röntgen rays, to obtain the same effects with the x rays as with the ultra-violet rays; and he was not aware at the time that M. Swyngedauw was working in the same direction.

The experiment consisted in causing sparks to pass between the two balls of a micrometer of about 4 mm. diameter. The small Ruhmkorff coil was so adjusted that the maximum explosive distance was about 2 mm. (without the action of the x rays). By causing sparks to pass at different distances from the Crookes tube, an increase in the explosive distance was observed.

The results led to the following conclusions:—

1. With equal distances, and with equal differences of potential, the increase in the explosive distance of the passive spark depends on the intensity of the x rays falling upon the micrometer.
2. With an equal difference of potential (of the micrometer), and with an equal intensity of the x rays, the increase in the explosive distance of the passive spark depends on the distance between the micrometer and the surface of emission of the tube.
3. The interposing of a fluorescent screen of platinocyanide of barium, of a strip of glass or of quartz, did not sensibly alter the action of these radiations.

The author considers that these phenomena are caused by variations in the constitution of the dielectric forming the surrounding medium of the micrometer. He states, in support of this opinion, that Hertz showed that the sensitiveness of a passive spark depends largely on the dielectric constant of the medium in which the spark takes place.

DÉSIRÉ KORDA—THE COMBINED USE OF ACCUMULATORS AND OF THE TROLLEY.

(*L'Éclairage Électrique, Vol.* 10, *No.* 6, *p.* 274.)

The above system has been employed in several large towns, and has recently been employed in France. Owing to improvements in the construction of accumulators, these have found greater favour for traction purposes, and have proved specially useful in improving the load-factor of central stations. This system was first applied to the Hirelanden electric tramway of Zurich three years ago, and has since then worked very satisfactorily.

On account of the uneven nature of the track, the current in the line varies between 0 and 210 amperes, but the output of the dynamo remains almost constant at 90 amperes and 550 volts, owing to the use of accumulators working in parallel with the generators at the station. The battery consists of 270 cells, which are connected directly to the dynamo circuit, and only require recharging for an hour during the day.

Other towns in Germany, notably Remscheid, have followed on the lines of the Zurich installation. The station contains four Thomson-Houston overcompounded dynamos of 100 kilowatts each. On account of the varying levels of the track, the current varied from 0 to 450 amperes in the station, when 12 cars were in motion, the mean reading of the registering apparatus being barely 150 amperes. A battery of accumulators was then installed by the Tudor Co. of Hagen. It consists of 250 cells of 650 ampere-hours capacity, with a normal discharge current of 215 amperes, but which can be pushed up to 240 amperes.

Although the dynamos are working non-compounded, the pressure does not vary by more than 10 to 15 volts above or below 500 volts. The current varies between 115 and 150 amperes—a mean of 132 amperes for the 12 cars in motion.

At some times of the day accumulators only are used. The charging is performed by means of a booster.

Owing to recent developments in the manufacture of accumulators, these are now being placed on the cars themselves, to be used instead of the trolley in such streets where trolley wires are prohibited.

Accumulators have also proved to be a help in diminishing the fluctuations of load on the dynamos.

The Tudor Co. of Hagen was the first to employ this system at Hanover and at Dresden. The ordinary cars, which are fitted with a trolley and 500-volt motors, carry a battery of some 200 cells of sufficient capacity to drive the motors in places where there is no trolley. When the trolley wire is in use, the accumulators are then charged at a high rate. The two motors are of 250 volts each, and are connected in parallel for working on the trolley, and in series for working from the accumulators.

The cells are connected in three parallel sets for charging, and act to some degree as rheostats for the motors. At the time of discharge they are all connected in series.

Another example of this system of traction exists in the Chicago North Side Railway Company, where, however, the charging of the accumulators is not

carried out on the cars themselves. At the "mixed" points a special car removes the accumulators and draws them back for charging purposes, and another car returns with a charged battery. For every 25 cars in operation 40 batteries are disposed of.

The advantage of this system is that the batteries need not be charged at so great a rate, and also that the cars when working by the trolley do not have to draw the dead weight of the accumulators.

The above systems are only applicable to towns where there are strong gradients.

With regard to the cost of such a system, it is less than with accumulators alone, so long as the trolley portion of the line is not too small, for in such a case the charging of the accumulators would necessitate too many stoppages at the "mixed" points.

A. MOUTIER—THE BUDA-PESTH TRAMWAYS: THE UNDERGROUND CONDUIT AND OVERHEAD TROLLEY SYSTEMS.

(*L'Éclairage Électrique, Vol.* 10, *No.* 12, *p.* 529, *March*, 1897.)

Four lines on the underground conduit system were installed in 1889 in Buda-Pesth by the firm of Siemens & Halske. In this system the current is carried by a conductor placed in an underground conduit extending along the track below one of the rails. The rail above the conduit consists of two independent portions forming a slot 33 mm, wide. The conduit itself is 28 cm. wide and 33 cm. high. It is built of concrete, strengthened at intervals with iron frames which serve to keep the double rail in position, and also to support the insulators on which the conductors are carried.

The conduits are always placed above the level of the drains, with which they are in communication.

The other rail may be either of the Phœnix, Vignole, or Marsillon type, but at Buda-Pesth the Haarmann rail was used. Current is conveyed to and from the motors by means of two bent metal strips placed between the double rail and rubbing along the conductors.

The motors on the present cars are direct coupled to the axle. The generating station supplies current both to the tramways and to the underground electric railway. The motors work at a pressure of 300 volts. The great disadvantage of this system is due to the difficult question of drainage. The conduits become flooded during heavy rains.

Experiments have been carried out in Paris to test the behaviour of the double rail under heavy traffic. The tests were not satisfactory; the double rail did not maintain its proper position.

The first two lines working in Buda-Pesth on the above system were opened in 1889, and are respectively 2·5 and 3·5 metres long; the two other lines were opened in 1890 and 1891. Another line which is in hand will be opened this year.

There are two lines of tramways in Buda-Pesth working on the overhead trolley system. One of them is 10 kilometres long, and works on the Siemens system. The second overhead line is not controlled by the General Tramways

Company of Buda-Pesth, the concession having been granted to the firm of Ganz & Co., of Buda-Pesth. The total length of this line is 12·8 kilometres, and it is used for goods as well as passenger traffic. The normal gauge of track is used, the rails weighing 20 kilogrammes per metre run. They rest on 16 × 22 cm. sleepers, and are placed 0·8 metre apart.

The overhead trolley wire is 7 mm. diameter, and is divided into five independent sections, supplied by a feeder extending the whole length of the line.

The motors work at 440 volts.

The rolling stock consists of 24 cars, each fitted with two 15-H.P. motors; 18 cars without motors, 10 goods trucks, 2 electric locomotives capable of drawing 60 to 70 tonnes, and a steam locomotive capable of drawing 100 tonnes.

A six-minute service is maintained.

The central station of the General Tramways Company contains 13 boilers, of 200 square metres heating surface.

There are two 250-H.P. engines, and two 600-H.P. engines, driving four direct-coupled dynamos. Three other engines, one of 600 H.P. and two others of 250 H.P., are used as reserve.

The engines are compound condensing, of the Lang type.

The central station of the Ganz Tramway Company contains five vertical Nicholson steam engines of 150 nominal horse-power. Babcock-Wilcox boilers are used.

The dynamos are of 100 kilowatts capacity, direct coupled to each engine. A battery of accumulators of 250 ampere-hours capacity is connected in parallel to the dynamos, to diminish fluctuations in the load.

Tables are given showing the cost of working these systems.

ELECTRIC STEERING GEAR OF THE UNION ELECTRICITÄTS-GESELLSCHAFT, BERLIN.

(*Elektrotechnische Zeitschrift*, 1897, *No.* 5, *p.* 66.)

This type of electric steering gear has been installed on one of the ships of the German Navy. It comprises a steel shaft mounted in five bearings on a base, and having at one end a worm which engages with two worm-wheels on the ends of two vertical shafts. To each of these shafts is keyed a drum from which hawsers pass over guide-pulleys to the rudder.

On the worm-shaft is also mounted differential driving gear, enclosed in a casing. In connection with this driving gear work two multipolar motors having their armatures turning in opposite directions, in such a manner that, when they are rotating at equal velocities, they have no influence on the worm-shaft, which passes through the centre of their hollow shafts. When the relative velocities of the motors is altered, the planet-wheels of the differential gear move, and the worm-shaft is set in rotation in one or the other direction, according to which of the motors is moving the faster.

The motors can have their armatures coupled either in series or in parallel. When they are in series the velocities of the motors can be varied by inserting a

resistance in parallel with the armature. Then, assuming constant excitation of the magnets, the shunted armature will act as a generator and generate current, and will thus be braked. The velocity of this armature will therefore decrease; this decrease is, however, dependent upon the mechanical resistance of the rudder, since, if this were infinitely small, the armature of the braked motor would immediately come to rest, whilst the armature of the running motor would run at its maximum velocity. If the resistance of the rudder be greater than the maximum turning moment of the armature which is running as a dynamo, or if the rudder-shaft be purposely secured in a fixed position, no difference in the velocities of the motors can take place.

A second arrangement consists in running the two armatures in parallel with one another and connecting up the field coils in series. The regulation of the apparatus is then effected by putting a resistance in parallel with the field coils of the motor the velocity of which it is desired to increase; this also causes a stronger current to pass through the field coils of the other motor, whose velocity is consequently decreased. This arrangement has been found to work very satisfactorily in practice, since, even when the helm was put suddenly from port to starboard on the high seas, there was neither mechanical shock to the gearing nor fluctuation in the light when the apparatus was worked from the dynamo feeding the lighting circuits of the ship. In connection with this apparatus a device is provided for locking the rudder in any position. The mean current required for steering is only 1 ampere, although each of the motors is capable of working up to 50 H.P.

G. QUINKE—ON ROTATIONS IN CONSTANT ELECTRIC FIELDS.

(*Wiedemann's Annalen, Vol.* 59, 1896, *p.* 417; *Elektrotechnische Zeitschrift*, 1897, *No.* 6, *p.* 82.)

The author obtained a constant field of electric force by connecting the vertically arranged plates of a condenser either with the coatings of a battery of eight flint-glass Leyden jars, or with the poles of an accumulator consisting of 400-1,200 test-tube cells. If rods, plates, balls, or cylinders of insulating material be hung in an electric field of this kind by a silk thread, they begin to rotate as soon as the condenser plates are immersed in an insulating fluid. The insulating materials comprised quartz, calcspar, arragonite, mica, sulphur, crown and flint glass, shellac, &c. A ball of arragonite, for example, rotated for 12 hours with apparently constant velocity.

The insulating fluids experimented upon were ether, bisulphide of carbon, mixtures of ether and bisulphide of carbon and of oil of turpentine and bisulphide of carbon, benzol, oil of turpentine, petroleum or rape oil. For the same material the mean velocity increases with the electric force. In the various liquids the mean velocity in ether was least for small potential differences, and greatest for high potential differences. With pure ether, bisulphide of carbon, and benzol the turning moment was approximately proportional to the potential difference; with the mixture of bisulphide of carbon and oil of turpentine very much greater. In fluids of higher viscosity, such as oil of turpentine, petroleum, and rape oil, the

rotation of the balls only took place at high potential differences. With rape oil the angular velocity soon reached a constant value (between 10,000 and 14,000 V). Hollow balls and cylinders, instead of rotating about axes normal to the lines of force, rotated about axes parallel thereto as soon as the field became sufficiently strong.

The author ascribes the phenomena to the action of the electric force on the layer of air which envelops the bodies while in the insulating fluid. A thin layer of any other fluid, whose dielectric constant, K_0, is different from the dielectric constants. K and K_1, of the fluid and the solid body, will produce the same effects.

F. KOHLRAUSCH—ON PLATINISED ELECTRODES AND THE MEASUREMENT OF RESISTANCE.

(*Wiedemann's Annalen*, 1897, *No.* 2, *p* 315.)

The author employed, for platinising electrodes, a solution consisting of 1 part of platinic chloride and 0·008 lead acetate in 30 of water. This solution is advantageous, not only for the reliability with which a dull, dark deposit of platinum black can be obtained, but also for the fact that electrodes so prepared gave an especially sharply defined sound-minimum.

By employing these electrodes the electrode surface can be reduced to below half a square centimetre. When platinising, a current-density of 0·03 ampere per square centimetre was employed for one and a half minutes. The result was improved by continuing the deposition with a current which was regulated by means of a resistance so that gas was freely evolved at the cathode, but only slightly at the anode; the two electrodes were made alternately the anode and cathode, each electrode being finally made the cathode for a full quarter of an hour. After this treatment, resistances down to 20 ohms can be measured to 1 in 2,000 between electrodes of half a square centimetre area with a workable sound-minimum.

In making a cell with fixed electrodes in which the leading-in wires are fixed into the glass wall of the cell, there is a risk of cracking the glass if such wires be bent. In order to obviate this, the wires are surrounded by a roll of wax-resin cement, or by a sheet-metal cap cemented on to the glass.

Various forms of vessel for containing the electrolyte whose resistance is to be measured are described and shown, together with a special arrangement of Wheatstone's bridge. Methods are also given for calibrating the tube.

In some cases it was found that the platinised surface could not be wetted by the solution whose resistance was to be measured. This difficulty was overcome by first moistening it with a drop of alcohol and then bringing it into the solution.

M. TROTTER—AN ELECTRIC BATTERY WITH AN ORANGE.

(*Beiblätter*, 1897, *No.* 2, *p*. 136.)

It was found by the author that a steel knife and a silver fork stuck into an orange gave a fairly strong current.

G. MEYER and K. KLEIN—THE DEPOLARISATION OF PLATINUM AND MERCURY ELECTRODES.

(*Beiblätter*, 1897, *No.* 2, *p.* 137.)

The authors found—

1. The size of the polarised electrode, so long as it remains small relatively to the other electrode, has no influence on the depolarisation ;

2. The rate of depolarisation decreases as the time of polarisation increases ;

3. When the time of polarisation is constant, the rate of depolarisation increases as the temperature rises ;

4. If a salt be dissolved in the electrolyte such that its acid corresponds with that of the electrolyte, and the base of which is the metal of the electrodes, the rate of depolarisation is increased at the cathode ;

5 The rate of depolarisation of Pt electrodes, with a limitation to be mentioned below, is less than that of Hg electrodes in the same electrolytes ;

6. In solutions of $Na\,O\,H$, $Na_2\,C\,O_3$, $Na\,Br$, $Na\,I$, mercury electrodes have approximately equal rates of depolarisation for polarisation at the cathode as platinum electrodes in solutions of $Na\,O\,H$ and $Na_2\,C\,O_3$;

7. In all the cases investigated the polarisation at the anode disappeared more slowly than that at the cathode.

— HASTINGS—A NEW METHOD OF READING OFF THE DEFLECTIONS OF A GALVANOMETER NEEDLE.

(*Beiblätter*, 1897, *No.* 2, *p.* 138.)

The reflecting mirror is perforated at the centre, or has the silvering removed at that place. Behind the mirror is arranged a mark, and in front a telescope and scale, the displacement of the image of the scale relatively to the image of the mark being observed.

G. GRANQUIST—ON THE MEASUREMENT OF COEFFICIENTS OF INDUCTION BY MEANS OF THE VIBRATION-GALVANOMETER.

(*Beiblätter*, 1897, *No.* 2, *p.* 139.)

The instrument employed consisted of a coil of wire having in the centre a magnet secured to a stretched wire and provided with a mirror. The magnet and torsion wire are arranged at right angles to one another, and both lie in the plane of the winding of the coil. When an alternating current passes through the coil, the wire is set in torsional vibration. The amplitude reaches its maximum value when the number of oscillations per second of the wire is equal to the frequency of the current. This condition can be attained by adjusting the free length of the wire or altering the frequency. An illuminated slit is visible, by means of the silvering on the galvanometer mirror, in a telescope provided with an eye-piece micrometer, and the amplitude is measured by the elongation of the image of the slit.

The differential vibration-galvanometer of the author has two coils, with their windings at right angles to one another. When the currents in the two coils have the same phase, by rotating the pair of coils about an axis coincident with the wire, the amplitude of oscillation of the wire can be made zero. The angle through which the pair of coils is rotated can be read off on a graduated circle, this angle giving the ratio between the intensities of the currents.

For measuring the phase difference between two alternating currents, each is sent through one of the coils, and the amplitude measured both before and after reversal of the current in one of the coils. If an alternating current be divided between the two coils, the relation between the coefficients of self-induction of the two branch circuits can be measured by making the amplitude zero, partly by introducing an inductionless resistance in one branch, and partly by rotating the pair of coils. By determining the frequency and the corresponding number of oscillations of the wire, the absolute value of the coefficient of induction can be found

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BENOIST and HURMUZESCU—ON THE DISCHARGE OF BODIES BY "x" RAYS.

(*Beiblätter*, 1897, *No.* 2, *p.* 155.)

The authors have established the following law, viz.:—The dispersion of electricity by x rays is proportional to the square root of the density of the gas in which the body is immersed, and is, moreover dependent on the nature of the metal on which the x rays fall. This appears to differ from the result obtained by Perrin, the conditions of the experiments being certainly essentially different. The authors, however, show that this is not the case. Their law is of the nature of a convection law: everything takes place as if the gas condensed on the electrified surface were driven off at a velocity such as is given by Graham's law; that is to say, that the velocity of the mass is exactly proportional to the square root of the density, μ. Consequently, if M is the quantity of gas driven off, μ the density, E the quantity of electricity set free, $M = K \sqrt{\mu}$, $E = K' \sqrt{\mu}$, and $E/M = K''$: that is to say, the quantity of electricity set free is a constant for unit mass, which is Perrin's law.

A. C. CREHORE and G. O. SQUIER—DISCUSSION OF THE CURRENTS IN THE BRANCHES OF WHEATSTONE'S BRIDGE, WHERE EACH BRANCH CONTAINS RESISTANCE AND INDUCTANCE AND THERE IS AN HARMONIC IMPRESSED ELECTRO-MOTIVE FORCE.

(*Philosophical Magazine*, *March*, 1897, *p.* 161.)

The authors describe graphical methods of solving this problem in its various cases. Methods are given for determining the currents in the branches and in the main line, the potential differences between various points and other quantities; the E.M.F., inductances, and resistances of the several circuits being given.

The condition for zero current in galvanometer, as shown by the diagrams, is

$$\frac{R_1{}^2 + L_1{}^2\,\omega^2}{R_3{}^2 + L_3{}^2\,\omega^2} = \frac{R_2{}^2 + L_2{}^2\,\omega^2}{R_4{}^2 + L_4{}^2\,\omega^2};$$

or, denoting the impedances of the branches by J_1, J_2, J_3, and J_4, respectively, and remembering that $J_1 = \sqrt{R_1{}^2 + L_1{}^2\,\omega^2}$; J_2 = &c., we have—

$$\frac{J_1}{J_3} = \frac{J_2}{J_4}$$

The ratio of the squares of the currents $I_{1,3}$, $I_{2,4}$, in the two branches when there is zero current in the galvanometer is

$$\frac{I_{1,3}^2}{I_{2,4}^2} = \frac{R_2{}^2 + L_2{}^2\,\omega^2}{R_1{}^2 + L_1{}^2\,\omega^2} = \frac{R_4{}^2 + L_4{}^2\,\omega^2}{R_3{}^2 + L_3{}^2\,\omega^2} = \frac{R_2\,R_4 + L_2\,L_4\,\omega^2}{R_1\,R_3 + L_1\,L_3\,\omega^2} = \frac{J_2{}^2}{J_1{}^2} = \frac{J_4{}^2}{J_3{}^2}.$$

Methods are also given for determining how a variation of the inductance in one arm of the bridge affected the inductance of a second arm, other constants remaining unchanged, while the galvanometer continually indicated zero. The methods given are illustrated by an example in which actual values, obtained in experiments, are employed; the inductances being varied by altering the position of iron cores inserted in resistance coils forming the arms of the bridge.

P. ZEEMAN—ON THE INFLUENCE OF MAGNETISM ON THE NATURE OF THE LIGHT EMITTED BY A SUBSTANCE.

(*Philosophical Magazine, March*, 1897, *p.* 226.)

The author examined the spectra from sodium and lithium flames, to observe the effects thereon of a magnetic field. The effect on an open sodium flame was to widen the two D lines. The experiment was also tried with the absorption lines produced by passing the light from an arc lamp through sodium vapour contained in strongly heated porcelain tubes; means being adopted for ensuring that the density of the sodium vapour should remain the same at different heights in the tubes, and thus make the absorption lines of equal breadth from the top to the bottom. When a magnetic field was produced in the tube the absorption lines immediately widened along their whole length.

The author considers that the action of magnetism on the free vibrations of the atoms is to superpose other vibrations of changed period. The approximate value of this magnetic change of the period was determined as follows, viz.:— The widening of the sodium lines to both sides amounted to about 1-40th of the distance between the said lines, the intensity of the magnetic field being about 10^4 C.G.S. units; hence there follows a positive and negative magnetic change of 1-40,000th of the period.

On discussing the phenomena observed in the light of Professor Lorentz's theory of electricity with Professor Lorentz, that gentleman stated that the edges of the lines of the spectrum should be circularly polarised. This, on experiment, was found to be actually the case.

O. M. CORBINO—ON THE VARIATION OF THE DIELECTRIC CONSTANTS OF AN INSULATOR BY THE EFFECT OF TENSION THEREON.

(*Beiblätter*, 1897, *No.* 3, *p.* 238.)

The author formed a condenser from a glass plate 150 mm. broad and 3·1 mm. thick, on both sides of which brass plates were cemented by means of paraffin. One of these sheets and one plate of an adjustable air-condenser were each connected to one pair of quadrants of an electrometer; the other plates being connected together and to one pole of a Ruhmkorff coil having a tuning-fork interruptor, the other pole of this coil, as well as the needle of the electrometer, being connected to earth. By hanging weights on the glass plate a deflection of the electrometer needle was produced, and then the adjustment of one plate of the air-condenser necessary to produce the same deflection of the electrometer needle measured. The author found that the dielectric constant of glass was decreased by a pull in a direction normal to the electric field, and that this decrease was proportional to the weight producing the tension.

ST. LINDECK—ON A COMPARISON OF THE B.A. STANDARD RESISTANCES WITH THOSE OF THE PHYSIKALISCH-TECHNISCHEN REICHSANSTALT.

(*Beiblätter*, 1897, *No.* 3, *p.* 240.)

The author compared the standard resistances of the Physikalisch-Technischen Reichsanstalt with those of the British Association at the Cavendish Laboratory, Cambridge, and found that

1 legal ohm = 1·01063 B.A. unit ;
1 international ohm = 1·01349 B.A. unit.

Glazebrook (Report of the Electrical Standards Committee) found that one legal ohm is equal to 1·01061 B.A. unit (mean value).

D. NEGREANU—A NEW METHOD OF MEASURING THE E.M.F. OF CELLS.

(*Beiblätter*, 1897, *No.* 3, *p.* 243.)

The current from a standard cell, the internal resistance of which is ρ, passes through a variable resistance r as far as the point a, where the circuit branches; the first branch contains a resistance r'', the other a galvanometer, a variable resistance r', and a cell connected up in opposition to the standard cell and having an E.M.F. $= e'$ and internal resistance ρ'. The resistances r and r' are adjusted until the deflection of the galvanometer needle is not altered by opening and closing the first branch. The ratio of the E.M.F.'s is then given by the ratio of the two resistances.

D. TOMMASI—AN ELECTROLYTIC PROCESS OF DESILVERISING LEAD.

(*Beiblätter*, 1897, *No.* 3, *p.* 247.)

The electrolyte consists of a solution of the double salt of lead acetate and sodium or potassium acetate. Between the anodes of the lead to be desilverised is moved the cathode of iron, aluminium, bronze, or copper. In the electrolysis no peroxide of lead is thrown down; the resistance of the bath is very small. All the lead migrates to the cathode; the silver falls undissolved to the bottom, together with any antimony or arsenic that may be present, and which is separated from the silver by smelting the mixture with saltpetre.

K. WILKENS—METHOD OF CALIBRATING MEASURING INSTRUMENTS FOR ALTERNATING AND POLYPHASE CURRENTS EMPLOYED BY THE ALLGEMEINEN ELECTRICITÄTS-GESELLSCHAFT.

(*Beiblätter*, 1897, *No.* 3, *p.* 248.)

The calibration of wattmeters, electricity meters, and phase meters is difficult when employing a special transformer with suitable transformation ratio for the reduction of the energy expended in each circuit, so as to enable standard instruments to be used for single-phase measurements of current and potential. In order to be able to use the single-phase values so obtained, without correction for the difference of phase, the angle of phase difference is regulated by means of a phase regulator which enables the phase of the "pressure" current to be altered as desired relatively to that of the main current. A laminated iron core is provided with a ring winding to which a drehstrom is supplied at fixed points 120° apart. Into the inner opening of the ring an iron core is introduced from below. The separate coils or turns of the ring are connected to a series of contacts arranged concentrically with the ring, and over which slide two springs. The phase of the current taken off by the springs can be altered as desired relatively to any one of the currents of the drehstrom.

CLASSIFIED LIST OF ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM

Appearing in some of the principal Technical Journals during the Months of FEBRUARY and MARCH, 1897.

S. denotes a series of articles. I. denotes fully illustrated.

ELECTRIC LIGHT AND POWER.

ANON.—The Electric Steering Gear of the A. E. Gesellschaft of Berlin.—*E. T. Z.*, No. 5, Feb., 1897, p. 66 (I.).

C. E. GUILLAUME—The Effect of High Pressures on Lowering the Penetrative Power of Rays emitted from the Positive Carbon in the Voltaic Arc.—*Beibl.*, vol. 21, part 2, 1897, p. 142.

Dr. MARKS—On the Electric Arc in an Enclosed Globe.—*Bull. Soc. Int.*, vol. 14, No. 135, Feb., 1897, p. 97 (I.).

L. COLIN—On Electric Heating and Cooking.—*Ibid.*, p. 134 (I.).

M. F. BONFAUTE—On the Electric Wiring of Private Houses.—*Ibid.*, p. 158 (I.).

G. PELLISSIER—On the Electric Lighting of the Avenue de l'Opera.—*Ecl. El.*, vol. 10, Feb., 1897, No. 6, p. 247 (I.).

A. BLONDEL—The Luminous Efficiency of the Electric Arc.—*Ibid.*, No. 7, p. 289 (S. I.).

L. B. MARKS—The Electric Light in an Enclosed Globe.—*Ibid.*, p. 313 (S. I.).

A. BOCHET—On Electric Installations.—*Ibid.*, p. 385 (S. I.).

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JOURNAL

OF THE

Institution of Electrical Engineers.

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The Three Hundredth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, April 8th, 1897—Sir Henry Mance, C.I.E., President, in the Chair.

The minutes of the Ordinary General Meeting held on March 25th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Students to that of Associates—

George F. L. Alexander.
John C. M. Matthews.
W. F. Stuart-Menteth.
Charles F. Wilkin.
Norman J. Wilson.

Mr. F. V. Andersen and Mr. C. E. Grove were appointed scrutineers of the ballot for new members.

The Secretary announced that donations to the Library had been received since the previous meeting from Mr. C. H. W. Biggs and Mr. R. Kaye Gray, Members, and Mr. E. H. Crapper, Associate, to whom the thanks of the meeting were unanimously accorded.

The PRESIDENT: I regret that I have a brief, but sad, announcement to make to you. You will all remember that at a very recent meeting we elected Dr. Von Stephan an Honorary Member of this Institution, and it was only at the last meeting that his letter thanking us for that honour was read. I regret to have received a letter—and I daresay many of you saw the announcement in the *Times* this morning—informing me that Dr. Von Stephan is dead. I had not the honour of his personal acquaintance, but we all of us knew him by repute, and we cannot but feel that in his death his country has sustained a great loss. I am sure you would wish us to adopt the usual plan in these cases, and to send a letter of condolence to Frau Von Stephan.

The proposal was unanimously agreed to.

SOME RECENT DEVELOPMENTS IN ELECTRIC TRACTION APPLIANCES.

By A. K. BAYLOR.

GENERATORS.

Mr. Baylor. In comparing a modern traction generating plant with one of seven or eight years ago, the most striking differences are found in the average size of the units in proportion to the station output, and the absence of belting or other medium of transmission between engines and generators. This may be largely accounted for by the change in public sentiment regarding electric traction during that period, and the greater confidence that investors have now in such enterprises. When electric traction first began to gain a foothold it was looked upon, like all innovations, as experimental and of doubtful value. As a natural consequence, all investment in plant was minimised. Small generating units were used, as the number of cars to be run was small. For the sake of economy in first cost, high-speed dynamos and slow-speed engines were generally employed, which necessitated the use of belts or ropes as a means of transmission. Now the conditions have changed, and in the design of power plants for traction work engineers are free to consider only what is best and most efficient in apparatus. Instead, therefore, of stations

Mr. Baylor.

subdivided into small units, and filled with belts, ropes, counter-shafting, and clutches, we have in a modern plant a minimum number of separate units, and, with rare exception, the engine shaft carries the dynamo armature and is rigidly connected to it.

The first result of the call for increased sizes and greater mechanical efficiency was the introduction of multipolar instead of bipolar generators. Up to 1891 over nine-tenths of the traction dynamos in service were bipolar; to-day fully nine-tenths are multipolar.

Multipolar generators possess decided advantages over those of the bipolar type; they permit of better distribution of the magnetic material, and are lighter in weight, requiring less floor space for a given output, which is of especial importance when the output is considerable. Belted machines of the four-pole type were made up to 500 kilowatts, and this is still the largest standard unit of that class, for the reason that satisfactory commutation cannot be economically secured in four-pole traction machines of a greater output. The violent fluctuations of load, incident to such work, necessitate a fixed point of commutation, and experience has shown that the economical limit when this condition is fulfilled is reached at 100 to 125 kilowatts per pole on a 500-volt generator. Therefore the number of poles and the speed must vary with the output, the poles increasing in number, and the speed decreasing, as the capacity of the machine increases; so that slow speeds have become standard for all large traction generators, entirely apart from any consideration of the prime mover.

A very important step in the development of this class of machinery was the adoption of steel instead of iron for the field frames and pole cores. By its use the magnetic properties of wrought iron are obtainable, together with compact and complex forms of construction not possible with wrought iron. Machines with steel fields require proportionately less magnetising force, and are of better efficiency in consequence, and a saving in copper in the exciting coils is effected owing to the smaller cross sections of poles for a given effect. Steel machines are lighter also, and

Mr. Baylor.

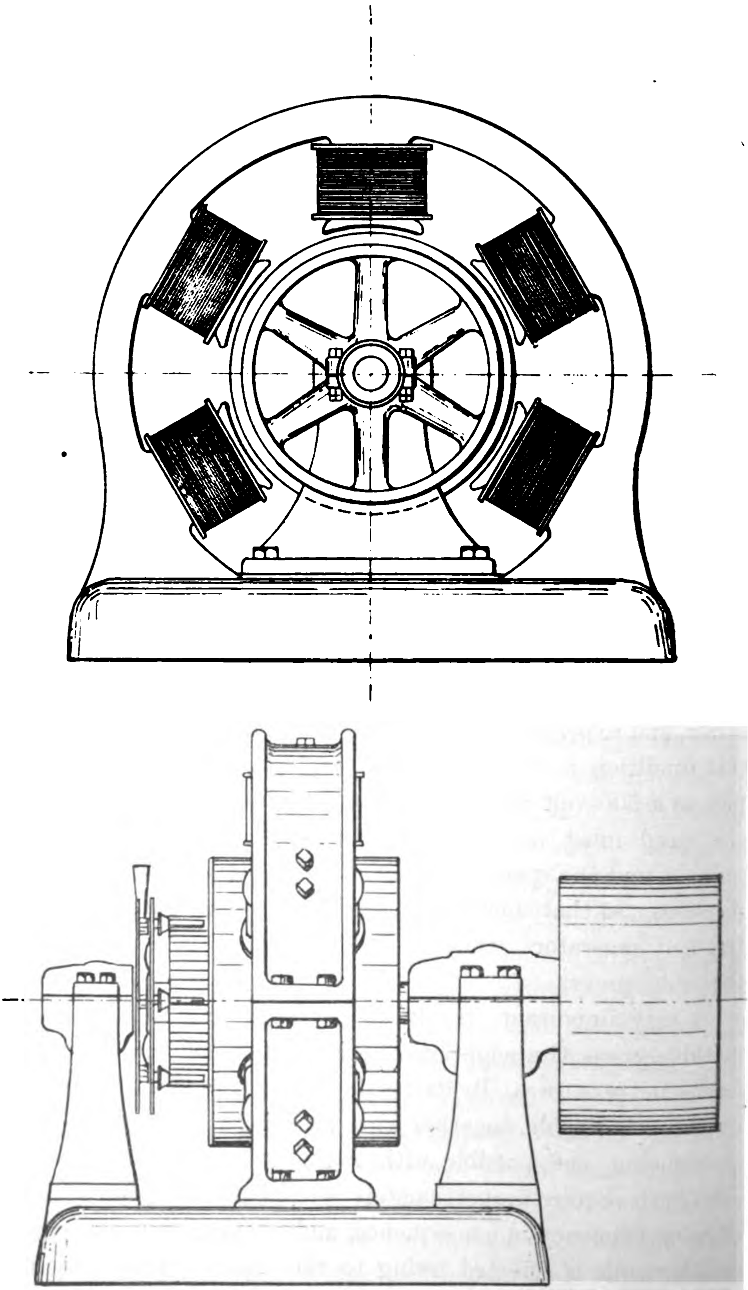


Fig. 1.—Typical Multipolar Traction Generator for Belt Connection.

the consideration of weight becomes of great importance when Mr. Baylor. the unit is a very large one. This question is not so practically serious in small units, but, generally speaking, the lighter a machine is for a given output the better.

Direct coupling was at first adopted only in the larger units, but this construction is now practically standard for all sizes. The severity of traction work has led many engineers to protest against the rigid connection of engine shaft and armature, and various arrangements of spring cushions have been devised to absorb the strains. These devices have been found useless and unnecessary, as with a properly proportioned fly-wheel the inter-connection of engine and dynamo presents no difficulty, such a fly-wheel being amply adequate to prevent racing on an open circuit or undue slackening of speed on a sudden overload.

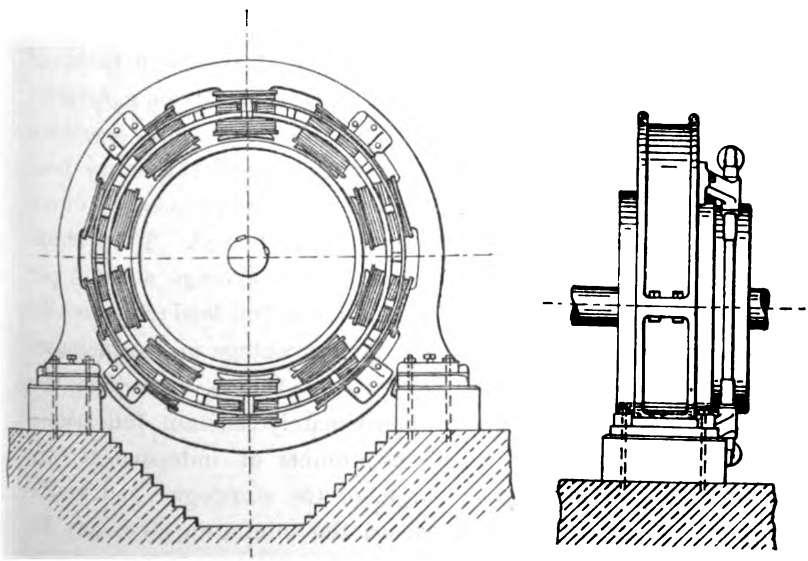


FIG. 2.—Typical Multipolar Traction Generator for Direct Connection.

The relative proportions of wheels vary with different engine builders, but average practice has shown the advisability of a wheel of such dimensions that it will carry the full load of the dynamo for about three revolutions with a variation of not more than 5 per cent. in speed.

Mr. Baylor. The method of connection between shaft and armature is different with large and small machines. In the former the armature spider is merely keyed to the shaft, but with large machines it is usual, in addition to keying, to bolt the spider to the arms of the fly-wheel at a diameter as far from the shaft as possible, in order to relieve the strain on key and shaft.

As a precaution against bursting, the built-up type of fly-wheel has been used in some few cases for direct-coupled traction units. Fig. 3 shows the detail of such a wheel. As will be seen, it is made up of segments bolted and riveted together. This construction of course permits of a relatively greater peripheral speed than is safe with the ordinary cast-iron wheel, but the cost is increased about 25 per cent.; and with a proper safety check upon the engine for cutting off steam at any predetermined increase of speed, danger of accident with cast wheels may be amply guarded against.

There are many points of commercial advantage in favour of direct-connected, as compared with belt- or rope-driven, generators that appeal to those interested in investment and operating expenses. Although not altogether new, some of these may bear repeating. The efficiency of a given plant is increased by direct coupling, and its operating expenses are decreased. The friction losses due to the use of belts or ropes will average about 5 per cent. of the indicated power of the engine at full load; and, as this loss is a practically fixed quantity, the percentage of loss increases as the percentage of load decreases. Unless, therefore, the running units can be kept at approximately constant full load—which is possible only when the number of independent cars or trains in service is very large, or the stoppages and starts infrequent—these losses will exceed the minimum, and may be taken as about 8 per cent. in average practice. In operation, a substantial economy is gained also by the reduced number of bearings and wearing parts, by a saving in maintenance of belts or ropes (to say nothing of their first cost), and the possibility in general of running with a smaller staff for a given plant output.

As affecting the total cost of plant, the reduced floor space occupied by direct-coupled machinery becomes a factor of much

Mr. Baylor.

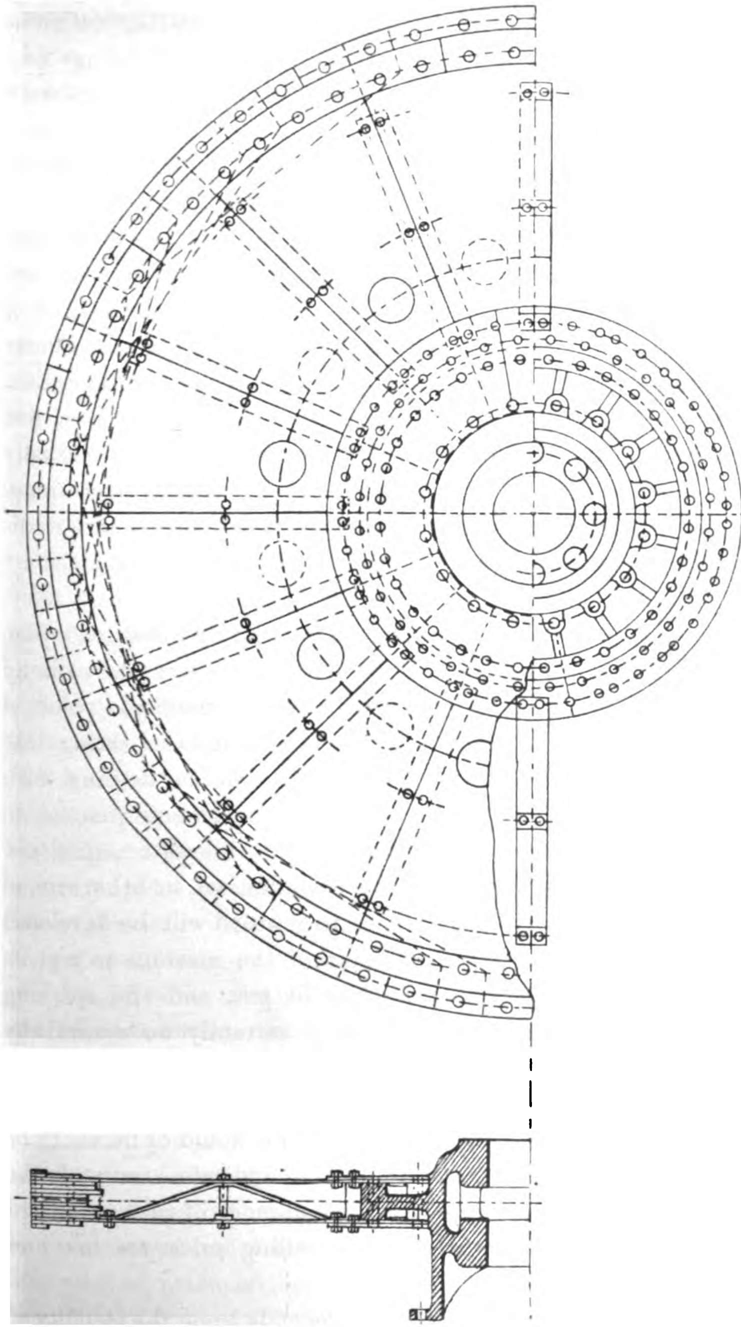


Fig. 8.

Mr. Baylor. importance when it is desirable to locate the generating station in a district where land values are high. Again, the buildings may be smaller, and therefore less expensive, and something may be saved in foundations.

There are undoubtedly some engineers who still consider the direct coupling of traction units unsafe practice, but the 300,000 horse-power (or thereabouts) of such machinery now in regular and successful operation ought to remove their doubts.

In the selection of generators the efficiency and the cost of maintenance should be taken as the measures of superiority and value. First cost should be a secondary consideration, unless the machines are intended for exhibition purposes and not for service. Unfortunately, this order of things is usually reversed, and the selling price too often settles everything, without regard to the expense for fuel, maintenance, and repair that the use of poorly constructed (and naturally cheap) machines may lead to.

In this connection it is to be borne in mind that, as a rule, dynamo ratings are arbitrary. With engines where the cylinder dimensions, steam pressures, speed, and economical point of cut-off are given, tests will establish accurately the rating that may be assigned to a given machine; but in dealing with generators and motors it is, unfortunately, common practice to give only the rating in horse-power or kilowatt capacity—a figure usually dependent on the conservatism, or otherwise, of individual engineers. The temperature which will be developed in the conductors or any other part of the machine in a given number of hours' running at a given output, and the sparking limit, are too often omitted. There is certainly no reason why dynamos should not be as specifically rated as the engines which are to drive them. A standard should be adopted covering sparking and temperature limits, so that all dynamos would of necessity be rated on the same basis, and the rating indicate accurately the capacity of a machine for service. The absence of such a standard puts a premium upon over-rating, as selling prices are based on rated output.

In the economy of a plant much depends upon the stability of

Mr. Baylor.

the generator armatures, and the development of this feature of modern machines has been most interesting. Compared with the drum windings common in the early bipolar types, or the ordinary ring winding used formerly in the multipolars, the present standard armature construction is extremely simple and durable. Properly speaking, it is not wound, but built. The conductors do not consist of wire, but solid bars formed to shape before they are put in position.

The greatest cause of burn-outs and grounds in the old wire-wound armatures is the shifting of the windings, due to twisting and centrifugal strains—a trouble especially common on smooth cores. It was difficult to provide proper radiation for the inner coils on such armatures (more particularly those of the drum type) and the indiscriminate crossing at the ends of conductors of considerable potential difference was another source of trouble.

Breakdowns from these causes are now pretty much done away with. Shifting of the conductors is impossible, as they are embedded in slots in the core and bound firmly in place; and in the latest construction all the end connections or wires outside the slots, instead of being bent down against the core, are extended straight out parallel with the shaft, and supported by cylindrical extensions from the spider at a diameter equal to a circle cutting the base of the core slots. When the winding has been properly disposed and covered with insulation, it is bound in place across the core face and out over the cylindrical end supports. The leads to the commutator are taken off at the extreme edge, leaving the conductors proper a compact cylinder like the rim of a pulley wheel, and nearly as substantial, presenting on the outside a smooth surface upon which dust and grit cannot collect, and protected fully on the inside by the core and the spider extensions.

The task of replacing a damaged coil in such an armature is comparatively simple, and rupture of the insulation seldom occurs, as the coverings have little tendency to deteriorate when the conductors cannot shift or vibrate, and when wires of great difference of potential are kept well apart. Ventilating ducts through the core reduce the heating, and the lateral extension of

Mr. Baylor. the connections as described also provides an excellent means of radiation. This arrangement of conductors also reduces the length of wire at the ends to a minimum, effecting an economy in copper and an increase in efficiency.

A practical advantage of much importance gained by this method of armature construction is that it effectually guards against damage due to careless workmanship. In constructing an ordinary wire-wound armature, a careless winder may so abrade the covering of the wire that its insulation will be ruined. With the construction described, on the contrary, each coil or section is made up, formed, and insulated separately, and carefully tested for any imperfection. The separate sections being shaped by die before being placed on the core, assembly is a simple matter, and is attended with little or no danger to the insulation.

In commutators the chief development has been towards better mechanical construction. On the best machines the commutators are built in sections, so that separate parts may be removed and renewed without disturbing the balance of the commutator—a matter of great convenience on a large machine.

Switch-Boards.

In modern practice the panel system is almost universally used in dealing with the switching and controlling gear of the station.

The panels are usually slabs of polished slate or marble (wood being no longer sanctioned), upon which the necessary instruments are mounted so that each panel, whether for the control of a generator or a line feeder section, is complete in itself. By following fixed dimensions, and a regular arrangement of instruments and switches, any number of these panels may be mounted side by side, forming when interconnected a complete and simple switch-board. This practice is largely the result of the constant additions that have been necessary in almost all electric traction plants, and the desirability of increasing the capacity of a station by the addition of new machines and controlling gear without disturbing the instruments already installed, preserving at the

same time the appearance and uniformity of the switch-board as a whole. Mr. Baylor.

In the principle of switch-board arrangement there has been little or no change, except as to the main switches.

Formerly it was the usual practice to mount the positive, negative, and neutral switches on a common yoke, and throw them in and out of circuit simultaneously by a single handle. It was then necessary, in putting a generator on the line in parallel with other machines, already running, to build up its field with the shunt excitation only, to a point something below the station voltage, so that when the positive, negative, and equalising switches were closed the additional field due to the series coil coming into action would bring the machine up to station pressure. It required some care on the part of the operator to determine how far to carry the shunt excitation so as not to fall below or exceed the station potential when the main switches were closed. Even with the greatest care there was always the danger of a fluctuation in the voltage of the running machines at the instant of closing. To prevent any tendency to motoring or flashing at the brushes, when throwing machines into parallel, the main switches were separated, so that each could be closed or opened independently, and this arrangement is now followed in practice. With separate switches it is possible to run the generator up to full voltage before throwing it into circuit by building up the shunt field and then closing the positive and equalising switches, thus putting the series coil into parallel with the series coils of the running machines. In order to connect with the line it is only necessary to close the negative switch, and the load will divide smoothly, the voltage being already balanced.

A further practical advantage is gained by this arrangement in that less time is required to build up, due to separate excitation through the series coil. It also prevents the fields coming up at opposite polarity to those on the machines in operation.

Fig. 4 shows a composite board of generator and feeder panels, and the general arrangement of instruments followed in practice.

Mr. Baylor. This board also includes a "station" or "total current" panel fitted with high-reading ammeter and wattmeter.

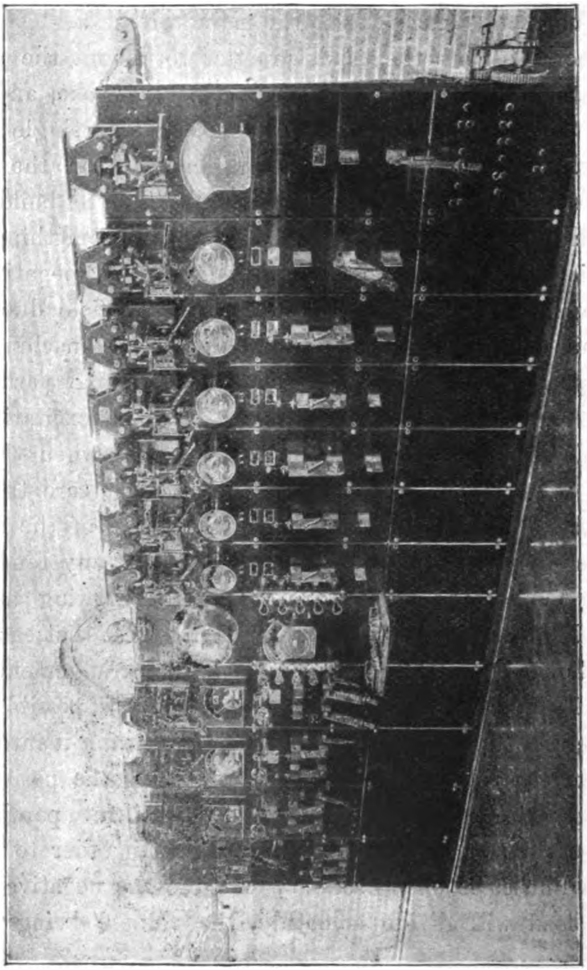


Fig. 4.

It is usual on any extended system of electric traction to divide the line into insulated sections fed by separate feeders. Where it is practicable, the separate feeders are carried back from each individual section to the station, so that any part of the line may be thrown in or out of circuit at the main switch-

board. The extent to which this arrangement may be carried is illustrated by Fig. 5, which shows the feeder switch-board of one of the large American systems. Mr. Baylc

Circuit-Breakers.

In early practice a fusible cut-out was thought sufficient to protect the dynamo from any excessive draughts of current, but it was found that a fuse small enough to properly protect the machine was subject to such frequent displacement as to be a cause of inconvenience. Automatic circuit-breakers were therefore adopted, mounted upon the switch-board of each generator; and separate feeder switch circuits, when they were used, were fitted with fuses. This arrangement is sometimes followed still, but the free use of automatic circuit-breakers in central stations is becoming more and more universal, and now, as a rule, they are placed on all the separate feeder circuits as well as the central station mains. Fig. 6 shows diagrammatically a device largely used for this purpose, and Fig. 7 its electrical circuits. When closed as shown in the diagram, the current flows up through the tripping coil, "T," to the stud "M," crossing by the yoke, "Y," to stud "M 1," and thence to ground. When the current has reached a predetermined volume, which is limited by the tension spring, "S," above the tripping coil "T," the armature, "A," is

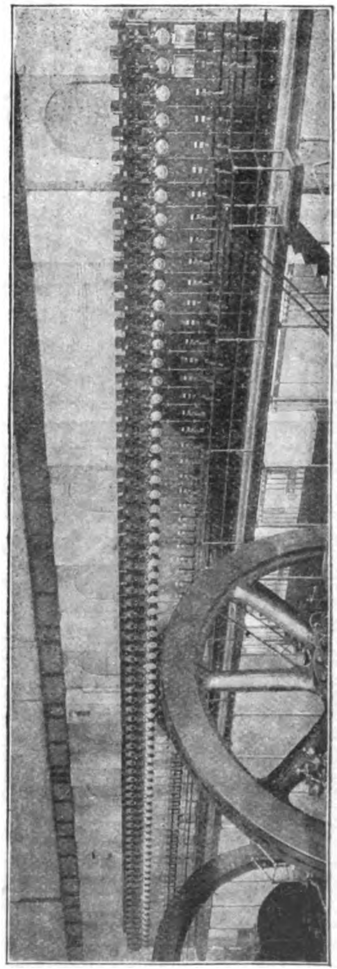


Fig. 5.

Mr. Baylor. depressed, lifting the latch, "L." A spring then draws the yoke "Y" away from the studs "M" and "M 1," when the whole of

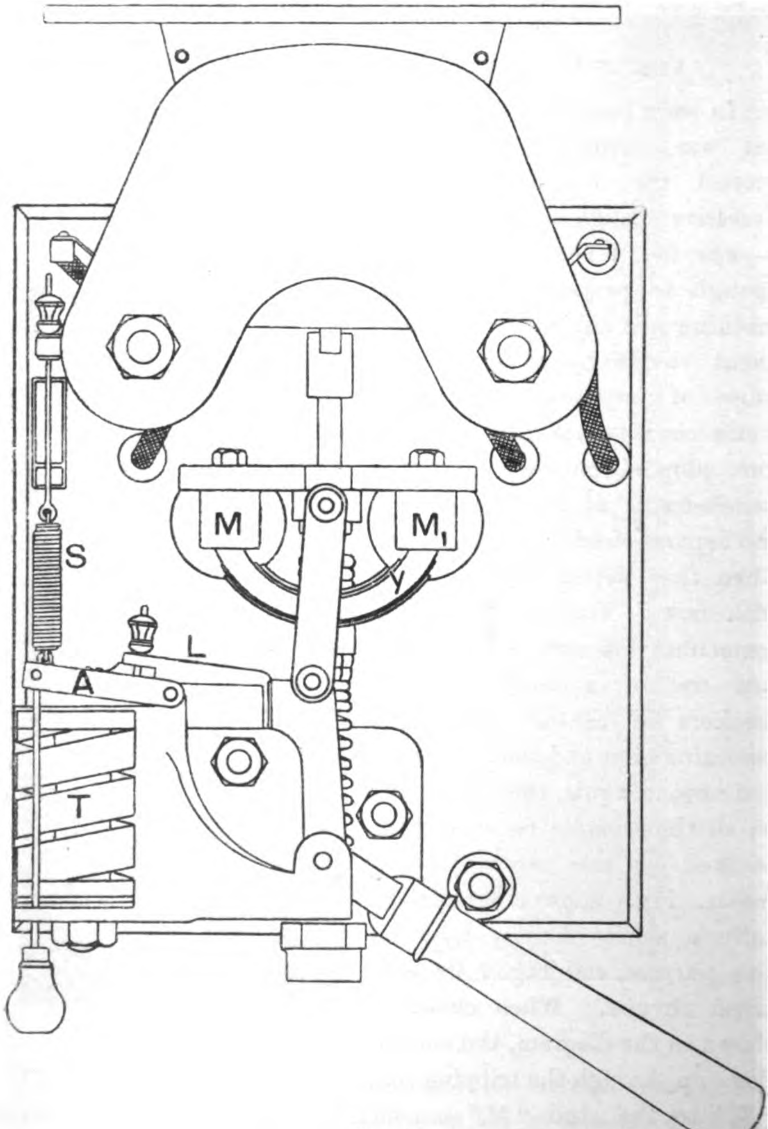


Fig. 6.

the current is thrown through upper coils "B" and "B 1" and contacts "C" and "C 1."

The wire in the coils is small, as the current is only flowing through them for an instant. As the yoke "Y" is drawn away from the main circuit studs, the plug, "P," which is attached to it, is withdrawn from between contacts "C" and "C 1," when the whole current is broken between these contacts, which are surrounded by an arc chamber and are directly under the influence of the magnetic field created by coils "B" and "B 1." Mr. Baylor.

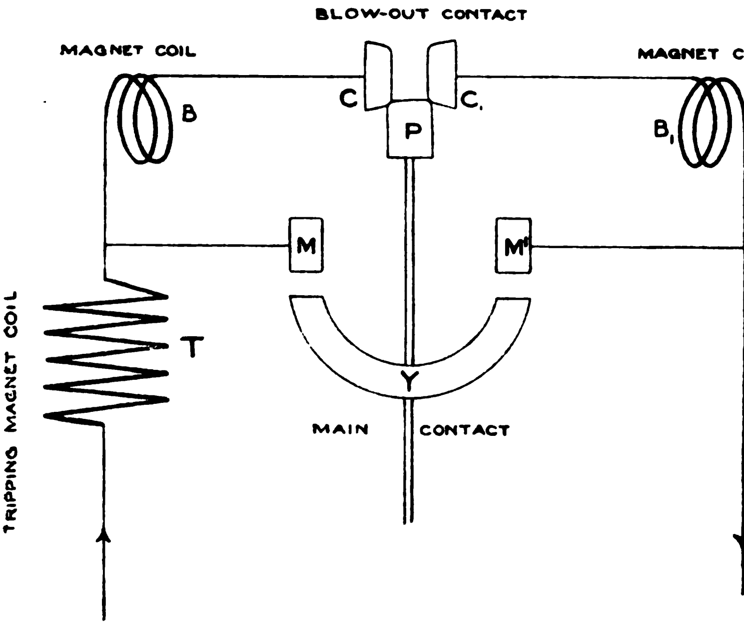


Fig. 7.

The practice has recently been adopted of using an automatic cut-out switch, constructed upon these principles, on motor cars, in place of the metal fuse. When a fuse is used it must be placed in such a position that it will not cause damage in blowing, so that, as a rule, it is more or less inaccessible, causing a delay in replacement. This car cut-out is combined with the main circuit switch, and, like the station circuit-breaker, is set to open at a given current by means of a spring. Such a cut-out is a surer protection to the motors than a fuse, because it acts instantaneously; while the fuse, even if of small capacity, requires

Mr. Baylor. an appreciable time to melt, so that in cases of excessive load or short-circuit the motor windings may be damaged before the circuit is broken and the current cut off.

OVERHEAD LINE.

As a means of conveying current to the car, the overhead trolley system must still be considered the standard for street surface lines, and where maximum efficiency is to be combined with minimum cost for installation, there is little probability of its being superseded. Aside from the objection that may be raised to overhead trolley construction on the score of appearance, everything is in its favour, and there is no doubt that most of the prejudice against it is based on ignorance as to the character of a well-constructed line. There are certain districts, of course, where an overhead trolley wire could never be sanctioned, but for general purposes it has much to recommend it. Its general popularity in America has come gradually. The idea that overhead wires can be erected promiscuously in American cities is distinctly erroneous, and the opposition met with when the first trolley systems were installed there was, in every way, as vigorous as it has been in England or on the Continent.

Probably nothing connected with electrical construction or its application has been so fiercely fought over as the details of the overhead line. The most modern form of such construction is essentially the same as that first generally introduced, and it is based upon the under-running wheel contact. It is the special form of overhead switches and "frogs," so called, to properly guide the under-running trolley wheel on branching circuits, that has been the basis of such severe litigation in America. The various forms of under-running bar trolleys that have been adopted to obviate the necessity of any switching devices overhead have never found favour in general practice, more particularly on account of the excessive wear upon the trolley wire.

It is difficult to so arrange a long bar of small diameter that it will roll freely even with ball bearings, and lubrication of a bar that does not revolve is still more difficult.

In dealing with high-speed service with the ordinary overhead

trolley construction, some difficulty was at first encountered in Mr. Baylor. the method of supporting the wire. The form of support ordinarily used, such as shown in Fig. 8, by partially surrounding

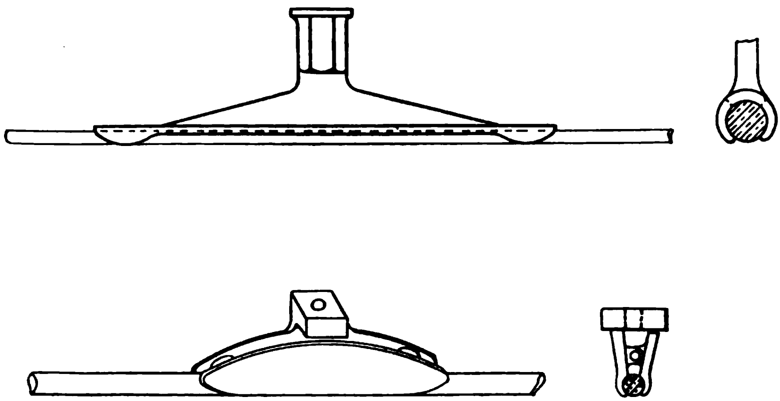


FIG. 8.

the wire, destroys the smoothness of the surface against which the wheel makes contact. Even at limited speeds of 8 to 12 miles an hour the trolley wheel strikes a perceptible blow on passing such a support, but when the speed is increased to 20 and 30 miles an hour or more, as it often is on inter-urban service, this blow throws the wheel away from the wire, and causes serious arcing.

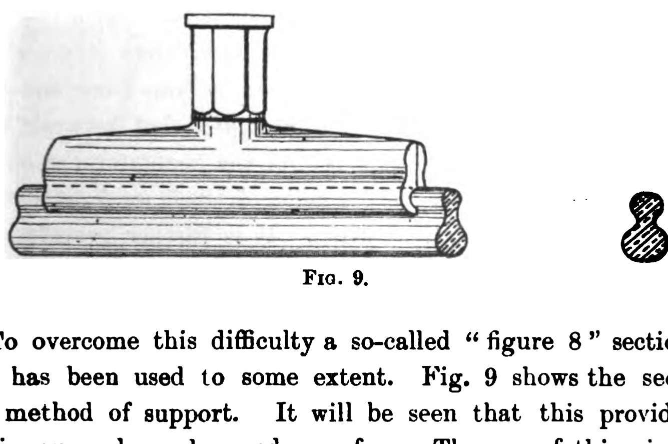


FIG. 9.

To overcome this difficulty a so-called "figure 8" section of wire has been used to some extent. Fig. 9 shows the section and method of support. It will be seen that this provides a continuous and regular under surface. The use of this wire has

Mr. Baylor. been limited, however, on account of its tendency to kink badly in handling. The form of support shown in Fig. 10 provides a

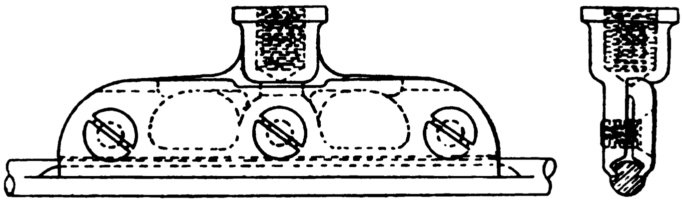


FIG. 10.

perfectly smooth run-way for the wheel, and permits the use of standard round-section wire. In putting up a line with this method, the wire is milled at the points of support, as the work proceeds, by a special portable tool.

GROUND RETURNS.

On a single-trolley line the question of track bonding is one of vital importance. The resistance of the rail joints must, of course, be kept at a minimum to secure best efficiency; but this is by no means the most serious consideration in applying electric traction to English conditions.

Under the Board of Trade regulations governing electric traction systems it is specified that the negative pole of the generators shall be connected with the rails or return circuit, also with two earth connections having such contact with the earth that a difference of potential of not more than 4 volts shall cause a current of 2 amperes at least to flow from one earth contact to the other. It is also provided that between these earth contacts and the negative side of the generator the current shall not exceed 2 amperes per mile of track, or 5 per cent. of total current output of the station. It is further provided that the drop in voltage over the track or uninsulated earth return shall not exceed 7 volts. These requirements mean practically that all the current shall return by the rails and none by the earth, and they limit the distance to which current may be supplied direct to the lines from any one point. In an average case this limit is reached at three to four miles from the generating station,

and farther away than this it becomes a question of installing Mr. Baylor. another plant or transmitting power to distant parts of the line

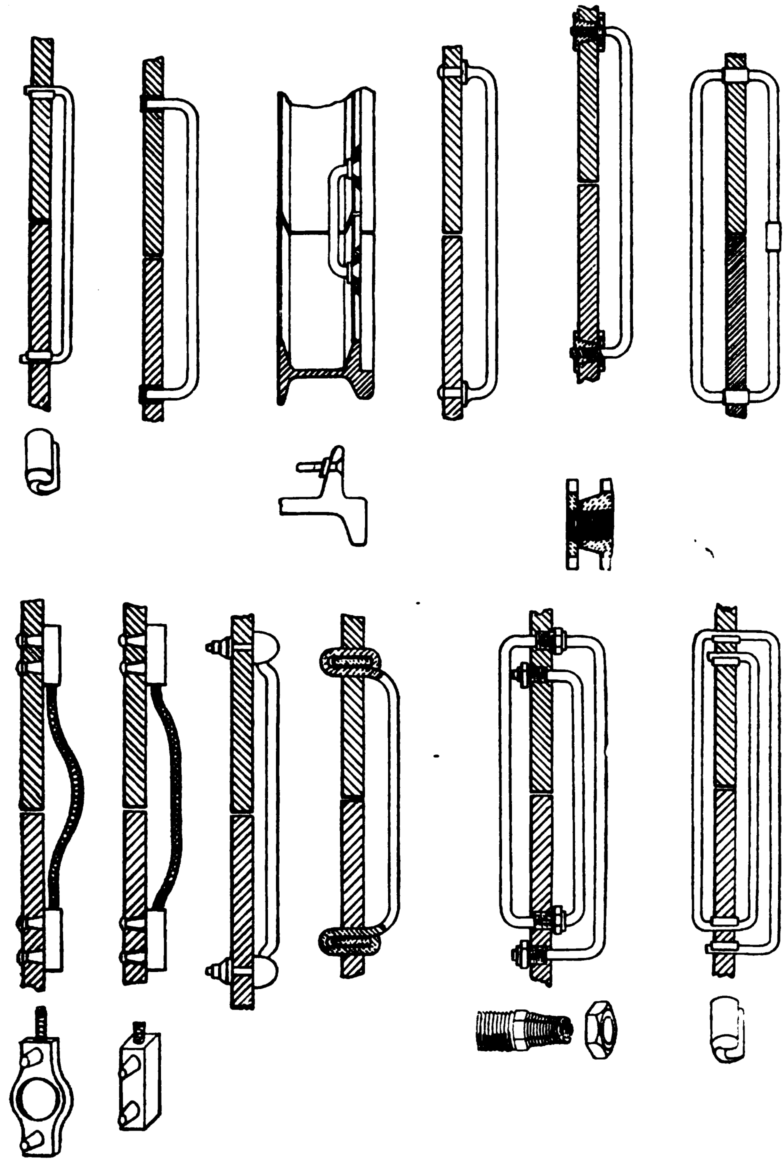


Fig. 11.

through sub-stations. Thus on an extended system in England

Mr. Baylor. it is, as a rule, the conductivity of the earth circuit that governs the design of the plant. It determines the necessary number of points of distribution, and the choice between direct- and alternating-current machinery in the generating station.

Fig. 11 shows a few of the various bonding devices that have been adopted from time to time, and which provide ample conductivity when first installed, but very few of them possess the virtue of maintaining their contact in service. The bonding problem is a mechanical one. A serious difficulty in the development of electric traction has been to find means of rigidly supporting the rail ends, and the weakness of all metal bonds making contact by means of studs through the web of the rail is their liability of being worked loose by the up and down motion of the rails at the joint. The best way of making contact by means of a stud through the web is by spreading the metal in the stud out against the rail. With this arrangement the hole in the rail may be made purposely irregular, and the copper in spreading will fill

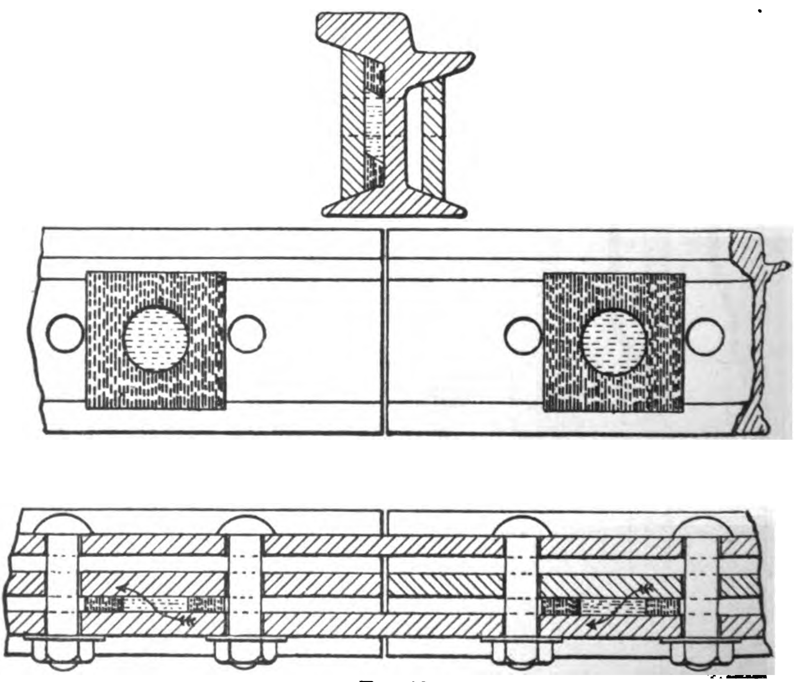


Fig. 12.

every crevice, increasing the maximum area of contact, and at the same time providing good mechanical means of preventing the stud from twisting or working loose. With weak rail joints, however, no bond of the stud type can be relied upon to maintain permanent contact in service, no matter how firmly it may be set to begin with. Various arrangements of spring washers and gravity nuts have been tried to provide adjustment and absorb vibration, but without success. Mr. Baylo

A so-called plastic bond has been recently introduced which has shown good results in test. It provides a means of utilising the conductivity of the fish-plates, no other solid conductors being used. Fig. 12 shows the method usually followed in applying this bond. A section of elastic cork, of convenient shape, with a hole in the centre, is placed vertically against the side of the rail; the hole is then filled with a plastic amalgam, and the fish-plates bolted in place. The cork washer yields to the pressure of the fish-plates, allowing the amalgam to spread somewhat on the surface of the rail web and the inner side of the fish-plate, thus establishing the electrical connection. It is claimed for this material that it maintains its plastic condition and provides a permanent amalgamation of the metal surfaces. If this is so, it is clear that any ordinary vibration and motion at the joint will not affect the contact. Fig. 13 shows

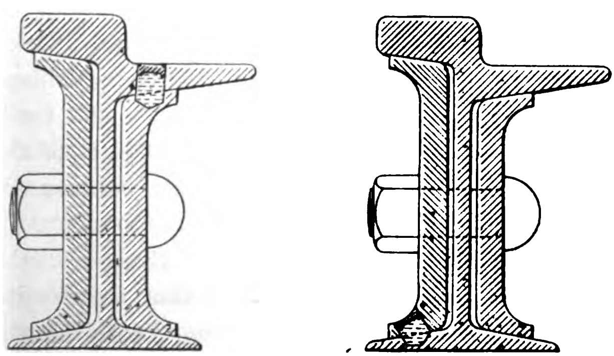


Fig. 13.

another means of making contact. A hole is drilled down from the top of the rail into the fish-plate, or from the base of the

Mr. Baylor fish-plate into the base of the rail, the cavity being in either case filled with plastic material and covered with a small soft iron plug.

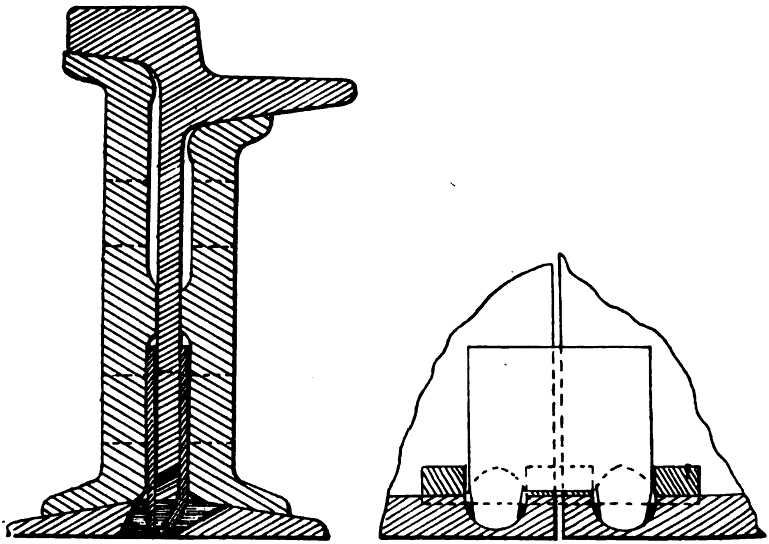


FIG. 14.

Fig. 14 shows still another method, which has somewhat the characteristics of a mercury cup connection. Holes are drilled into the abutting rails near the ends at the angle formed by the base of the web. These cavities are filled with the plastic amalgam, and receive the ends of a copper yoke, which spans the joint. No other means of contact is provided, and the yoke is held upright in position by the fish-plate. A joint of this kind will, it seems, allow a great deal of motion between the rails without disturbing the connection.

It must be remembered, however, that there are other considerations beside bonding which demand that movement at the rail joint should be prevented. With loose joints the whole car equipment is strained in service, and the cost of maintenance on the car body, the truck, and the motors is seriously increased.

In this connection the experiments with electrical and cast welded rail joints are worthy of attention. The idea of forming

by these methods a continuous rail arose first from mechanical considerations and the great difficulty of maintaining rigid joints. During the past two or three years a great deal of experience has been gained with solid joints under various climatic conditions, and the results indicate that this practice will have an important bearing on permanent way construction, especially for electric traction when the rails form the return circuit. Mr. Baylor.

In making cast welded joints the rail ends are first cleaned, and then a mould is clamped around them and filled with molten iron, which forms, when it has set, a strong mechanical bond which has been found in service to withstand admirably great extremes in temperature without fracture. It often occurs, however, that the casting makes no union with the rail, and when this is the case it has little value as an electrical connection. When the joints are made with the rails at a comparatively low temperature (as they usually are, in order to secure the greatest possible contraction before joining, and, therefore, less liability of breakage in cold weather), the iron poured into the mould is apt to be chilled against the surface of the rail, preventing adhesion. Furthermore, the rail may expand slightly under the heat, contracting again when the mass has set, leaving the casting practically a shell around the joint, although supporting it rigidly. Mechanically, this may be considered an advantage, as it allows slight contraction and expansion with changes of temperature, without undue strain; and in some cases the rails are painted with a compound before the iron is poured around the joint, for the purpose of preventing any union of the metals. When the rail ends are left perfectly clean it frequently happens that a good union is effected, especially at the base of the rail, where the casting is thickest, and in such cases the joint has a very low electrical resistance. Until the process can be made to ensure adhesion of the metals, however, cast joints should, as a rule, be supplemented by the usual bond connections. When this is done no saving is effected in the first cost of bonding, but the solid joint will materially decrease the expense of maintaining the connections, doing away, as it does, with vibration and movement at the rail ends. When cast joints are used frequent

Baylor. cross-bonding may be sufficient guard against bad electrical contact if the ground return is not worked to its utmost capacity, and in such cases the cost of bonding is reduced.

There are several long sections of line in America with cast welded joints that are being operated without auxiliary bonding, and with apparently perfect satisfaction and freedom from electrolysis; but it is doubtful whether some of these sections would come within the existing Board of Trade limitations in England as to drop in the ground return.

Electric welding of the joints, on the other hand, secures perfect electric continuity, and reduces the joint resistance to that of any other part of the rail. Electric rail welding was formerly accomplished by placing two U-shaped yokes against the rail, one on each side, forming a link around the joint, and the weld proper was made between the ends of these yokes and the webs, 3 or 4 inches back from the rail ends. It has been found difficult, however, in practice to secure the requisite mechanical strength by this means; for although perfect union was made with the web, the weld, in cooling, introduced very severe strains on the surrounding metal, and frequently after short service the links broke loose, taking out a disc of each web still in perfect union with the yokes. The process was consequently abandoned, and rails are now welded by abutting the ends together and welding them under pressure. This gives greater mechanical strength, which is the principal consideration, and as such a joint has the full conductivity of the rail no auxiliary bonding is necessary. If these welds prove in practice to have the requisite mechanical strength, they will of course be better than cast joints for electric traction.

There seems to be no doubt that the principle of a continuous rail is mechanically sound, and that, when joints are made with the rails at a low average temperature, they are fully capable of withstanding strains of service and temperature, especially in climates with moderate extremes of heat and cold.

Three-Wire System.

In dealing with long lines extending beyond the economical

limits of 500-volt direct-current transmission, the three-wire system has been proposed, and applied experimentally to a limited extent. Mr. Baylor.

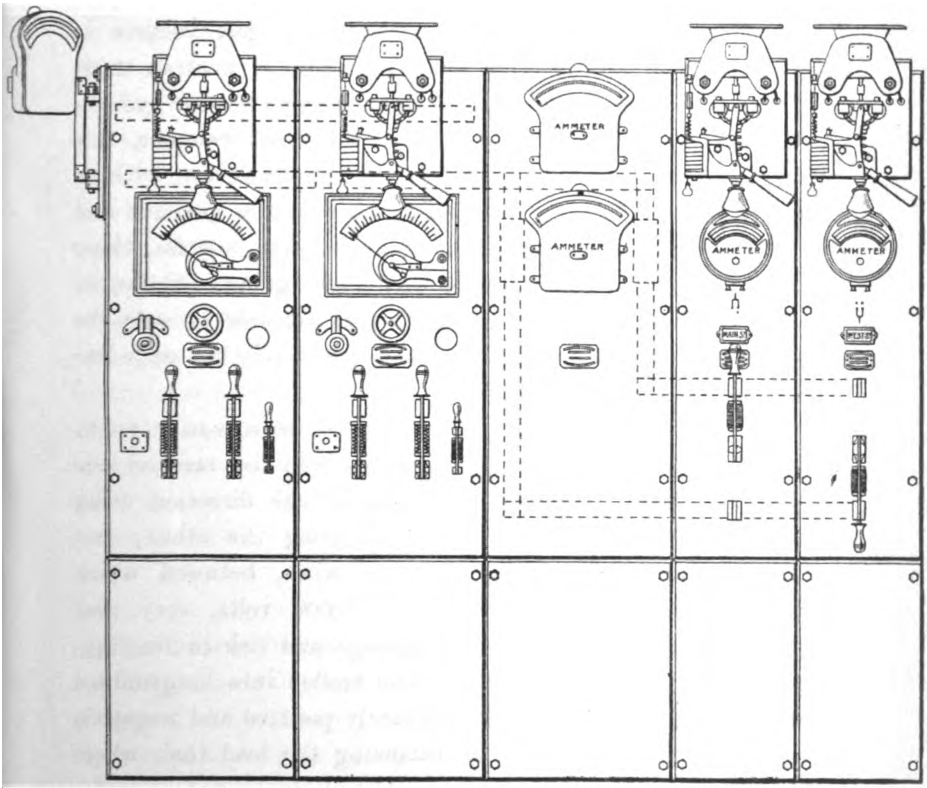


FIG. 15.

Fig. 15 shows a specially arranged switch-board for the three-wire system, with the feeder switches arranged so that they may be thrown from one side to the other as desired, for balancing purposes. In using this system it is of course necessary to divide the station into units of two generators each. If, however, it is desirable on light loads to run only one machine, all the feeders may be thrown on one side, and the line run as an ordinary 500-volt two-wire system.

This plan cannot be recommended in laying out a new plant for high-tension distribution, and is by no means as flexible as

Mr. Baylor. alternating-current transmission through sub-stations either with static transformers or rotary converters.

If a perfect balance could be maintained, the track current would of course be reduced to zero, and the danger of electrolytic trouble would be entirely removed. But a perfect balance is practically out of the question, and the track, or neutral, must carry current and be bonded, as with the ordinary two-wire system.

Various arrangements have been tried to overcome this difficulty of balancing. On an ordinary double-track line, with an equal distribution of cars, one trolley wire may be positive and the other negative: the track in all cases being neutral, those cars going in one direction balance with those going in the other direction; and when the cars of each pair are side by side, the cross bonds between the rails represent practically the only line drop outside the trolley wires.

With a single line an equal division is not so easily made. The positive and negative trolley wires may be erected side by side over the track, the cars going in one direction using one trolley wire, and those returning using the other; but it is not desirable to bring the two wires, between which there is a difference of potential of 1,000 volts, very near together, on account of danger of leakage and risk to linemen. Another arrangement is to divide the trolley into longitudinal sections, making such sections alternately positive and negative. This gives a better division for balancing the load than where two trolley wires are used on a single track, as in case of a blockade the cars congested at a point are apt to be going in the same direction, and would therefore on the double trolley wire arrangement be on one side of the system, unless the trolleys on some of the cars were temporarily changed from one wire to the other. On double-track systems having branching single lines, the balance may be made by dividing the branches equally as to load between the positive and negative mains; but this has the objection of requiring the current to flow over a considerable length of track, with attendant danger of electrolysis.

Another means of dividing the load is to have every car balance itself, by running two trolley wires over each track, and

mounting two trolley poles on the car, with two separate circuits between the trolley and the ground. This arrangement, however, is only to be considered with a four-motor equipment, as otherwise series-parallel controllers could not be used. Furthermore, in practice it is inadvisable to introduce as high a difference of potential on the cars as 1,000 volts. Mr. Baylor.

To meet this question of balance, it was at first thought possible to follow the lines of lighting practice, whereby compensators were used to equalise the load on the mains; but this plan was found impracticable, as the sudden variations of load occurring on traction lines present an entirely different problem from that met with in lighting. Thus far the experiment of changing from the two-wire to the three-wire system, as tried in several instances on American roads, has been unsuccessful, and the three-wire system for traction work has been abandoned, on account of the practical difficulty in dealing with the question of balance; and this with no limit as to drop in the ground return, which, as has been pointed out, is the most serious consideration for English practice.

MOTORS.

The improvements in street car motors have been along lines parallel to those followed in the development of generators. The adoption of steel has made possible the construction of much lighter, much stronger, and much more compact motors. The two-pole types have become practically obsolete, and multipolars have taken their place. The open frames necessary when machines are built of forged pieces have given way to entirely closed frames, which go far toward increasing the life of the motor and reducing expense of maintenance. Formerly the interior of the motor was open to the attacks of dust and grit in dry weather, and mud and water in wet weather, which caused rapid wearing away of the commutator and brushes and serious deterioration of the insulation. With the modern construction the armature, commutator, and brushes are completely enclosed between two bowl-shaped castings, excluding all grit and moisture. In the older forms the gearing was also exposed, and naturally cut away rapidly in

Mr. Baylor. service. All gearing is now enclosed in oil-tight casing, which increases its life and adds to the efficiency of the machine. The modern steel motor is, for a given output, only about one-half as heavy as the old wrought-iron bipolar type. This is clearly of the greatest importance as affecting the total weight of the car, reducing wear and tear on the permanent way, and economising power.

The principle of armature construction already spoken of in connection with generators is now followed in all the best modern street car motors. Here it is of even greater importance than in the generator, as the motor armature is much more subject to overloads and consequent danger of burn-outs.

The use of alternating-current motors for traction work has made little headway. With the exception of the line at Lugano, Italy, practically nothing has been done in practice with such apparatus. In spite of the many apparent advantages of such a system, it does not adapt itself economically to traction work. When current has to be widely distributed, the use of three-phase motors will of course do away with the necessity of sub-stations, which is an advantage, and the absence of commutators is a large factor of economy. It is also a great convenience to be able at any given point on the line, where the conditions are severe, to place a special transformer delivering more than the normal voltage to the line, thus increasing the maximum torque of the motors for the time; or a special booster may be placed on the car and thrown into circuit whenever an increased torque is desirable. On the other hand, as the torque of this type of motor at a given voltage is limited, it is at a disadvantage for ordinary traction purposes as compared with a direct-current machine, having an indefinite maximum torque under similar conditions.

It is evident that for a given service a multiphase motor must be larger than a direct-current motor. When direct currents are used, the motor may be designed for more nearly the average work; and when severe conditions are met with, as in mounting a gradient or rounding a curve, the motor will exert sufficient torque to overcome the difficulty. A triphase equipment designed for the average work would be

stalled under similar conditions. Consequently three-phase traction motors must be designed for the maximum effort which they may have to exert. Another serious drawback in the triphase equipment, as compared with direct-current apparatus, is the fact that series-parallel control cannot be as economically employed. The use of two trolley poles is also a disadvantage, although not necessarily a serious one. Experience so far has gone to show that the use of three-phase motors for traction work must be confined to special conditions, and that for ordinary service direct-current motors are much better adapted. Mr. Baylor.

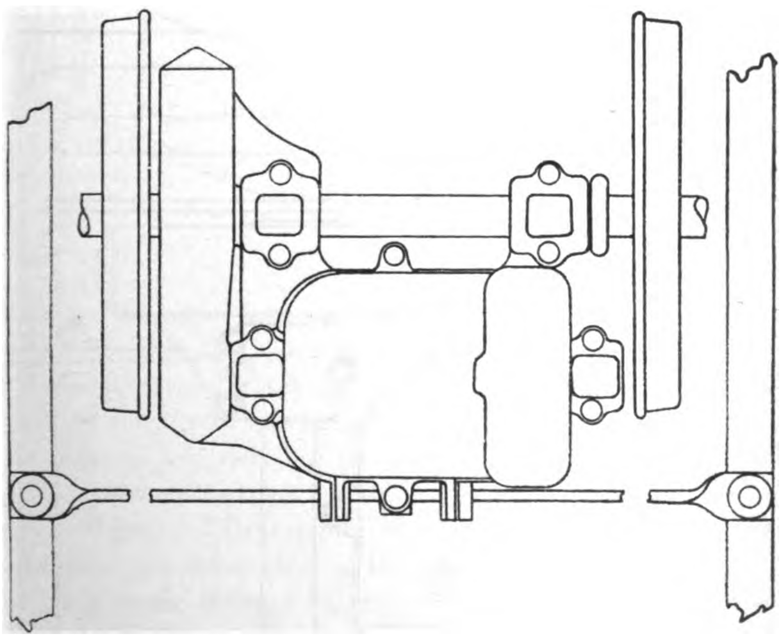


Fig. 16.

Figs. 16, 17, and 18 show various methods of supporting motors upon the axle of truck. The well-known nose suspension is the most commonly used, and requires no comment.

The so-called side-bar and yoke suspensions are intended to relieve the axle from the dead weight of the motor, which it has to bear with the nose suspension. As will be seen, the side-bar construction supports the mass of the motor at its centre line, so

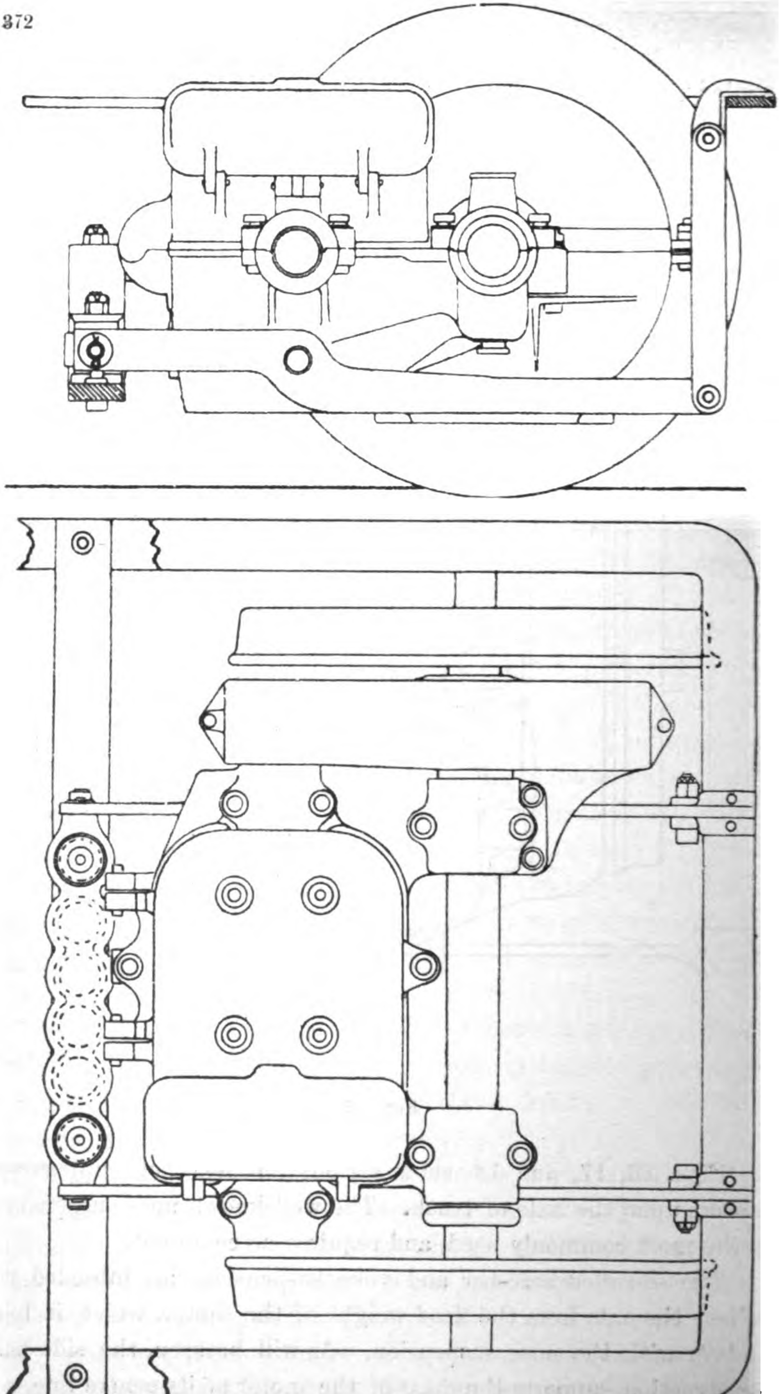


Fig. 17.

that the wear on the axle bearings is minimised. This suspension has the disadvantage that the side bars interfere with ready Mr. Baylor.

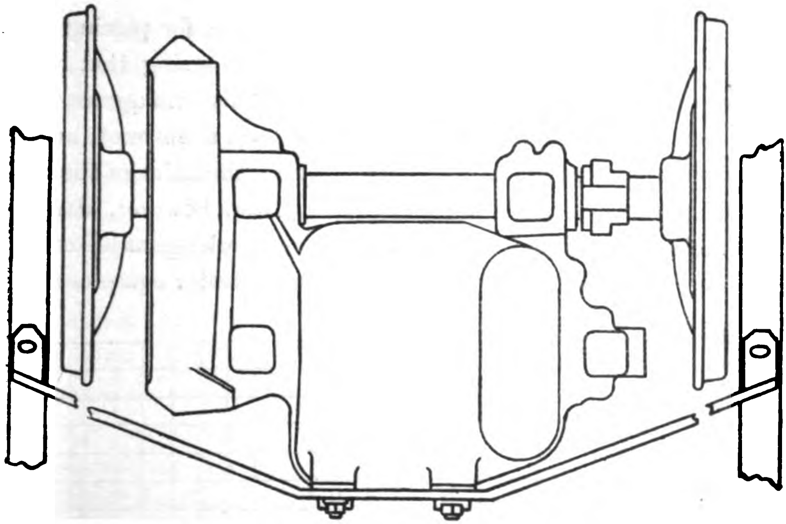


Fig. 18.

access to the motor from the side. The yoke suspension, however, is free from this objection, while still supporting the motor so as to relieve the axle from its weight. While supporting the motor in the centre has the effect of increasing the life of the axle bearings and relieving the axle, it does not eliminate the hammer blow on the track in passing over any irregularities, such as a bad joint. This hammer blow can only be prevented by supporting the motor clear of the axle either on supports from the truck frame between the axles with chain or other flexible connection to them, or by following such a plan as that adopted on the Baltimore locomotives, where the car axle passes through a sleeve to which it it is connected by some flexible coupling, so that the motor as a whole may vibrate up and down independently of the car axle.

Controllers.

The series-parallel method of control is now used exclusively in standard practice wherever the car equipment consists of two

Mr. Baylor. motors. The economical advantages of this method of control are of course obvious, and some of the very earliest experimental equipments embodied controllers of this type. At that time it was customary to use an independent switch for passing from series to parallel, on account of the severe arcing that would otherwise occur at the instant of change. This arrangement was so inconvenient in practice that the idea was abandoned, and for some years the motors were controlled by commutation of the fields or by the use of graduated resistance. Now, however, that the old arcing difficulties have been overcome, series-parallel control has supplanted all other methods for double-motor equipments.

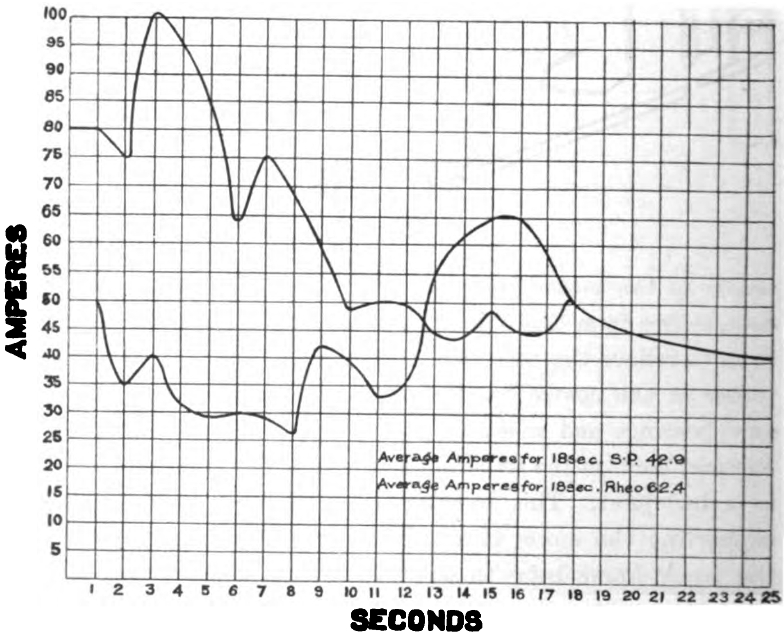


Fig. 19.

The superimposed curves shown in Fig. 19 represent the comparative power consumption in starting a double-motor equipment with rheostatic and series-parallel controllers.

It follows that the more congested the service, and the more frequently the car is stopped and started, the greater is the economy to be gained by the use of series-parallel controllers, and

high-speed lines, or lines upon which fewest stops are made, gain least in total power consumption by their use. As the maximum possible draft, however (that is, should every car start at the same instant), on the power house of any system using series-parallel control is approximately half as much as with rheostatic control, the maximum load is brought down nearer to the average load, with a corresponding saving in the necessary generators and engine power to be provided. Mr. Baylor.

When modern series-parallel controllers were first produced, many large American systems found it to their advantage to instal them on all their cars in service, and to throw away all other types of controller, the expense of the change being many times offset by the increased service of cars possible without additional power house capacity or line feeders. If on a system using this method of control one-half the station capacity should be disabled, all the cars could still be operated by being run in series, and therefore at half speed, which would inconvenience the public much less than taking half the cars out of service and running the remainder at full speed. This plan also has the commercial advantage that just as many passengers can be carried, although the car mileage for the time being is reduced.

The mechanical features of the modern controller have been carefully developed, and every precaution has been taken to prevent careless or unskilful motor men from injuring the apparatus. It is usual to so interlock the power and reversing cylinders by means of gearing that it is impossible to move the power handle unless the reversing switch is fully thrown either forward or backward, and impossible to move the reversing switch unless all power is cut off from the motors.

Electric Brakes.

In dealing with modern mechanical traction the question of proper brake facilities is of the utmost importance. As compared with the ordinary horse car, a mechanically propelled car is necessarily heavy. A large factor in this increased weight is the heavy truck frame necessary, and to this is added the weight of the driving equipment. Moreover, the size of car bodies has

Mr. Baylor. been increased, and their weight is proportionately greater, due to the heavy framing which is required throughout, in order to withstand the strains incident to mechanical traction service. The difficulty of braking such cars is increased in direct proportion to the increased weight, and in addition to this the average speed of an electric car is nearly, if not quite, twice that possible in ordinary horse service, which is a more serious consideration than the weight, as the difficulty of braking increases with the square of the speed. Under these conditions the best possible arrangements of hand brakes and levers impose an undue burden upon the motor man, and call for an amount of brute force inconsistent with perfect handling of the vehicle. The application, therefore, of power brakes to street cars is worthy of very serious consideration, as they enable the motor man to stop his car under all conditions without muscular exertion, leaving him free and on the alert for any emergency.

Air brakes on street cars have been fairly successful in so far as they permit of the application of powerful braking effect without taxing the driver; but upon electric cars, where the generating power of the motors may be availed of for braking, air brakes have little to recommend them. In the usual form of air brakes used in street cars, the air pressure is derived from eccentrics driven from the axle, so that pressure can only be generated while the car is running. It is undesirable to use a separate motor for operating the pump in ordinary practice, on account of cost, inefficiency, and the limited space available for additional apparatus outside of the necessary driving motors and other accessory equipment. It is difficult even to find room for air receivers on the average street car. When the pump is driven from the axle the receiver must of necessity be of ample dimensions, as repeated applications of the brake may follow one another at short intervals of space, the pressure absorbed in each application exceeding that generated between stoppages. A continuance of this process may so reduce the pressure that it will be inadequate for controlling the car in case of emergency. With a properly constructed electric brake, on the other hand, the frequency of stoppages is immaterial, as it is the

current from the motors, driven by the motion of the car itself, which furnishes the braking force. Moreover, the efficiency of mechanical friction brakes is greater at low speeds, and falls off rapidly as the speed increases; the electric brake, on the contrary, is most effective at high speeds, as the faster the motor armatures are revolving the more promptly will current be generated. Mr. Baylor.

The idea of converting traction motors into generators and utilising the current for braking purposes is an old one, and has been applied in practice to a limited extent for many years. Hitherto electric braking has been little used, although any ordinary motor equipment embodies a practical emergency brake which may be applied by short-circuiting the motors either directly or through resistance. This practice imposes such severe strains, both upon the windings and gearing, that it can only be used as an emergency measure, and is quite out of the question for regular braking. In order to apply this emergency brake when there are two motors on the car, it is only necessary to put them in parallel and throw the reversing switch as if to go backwards. The generated voltages will then oppose each other, and the weaker one will be overcome, resulting in a counter current, through the corresponding motor reversing its field. The motors now being in series instead of in parallel, with nothing but the motor windings in circuit, a powerful current is generated, which increases until the momentum of the car has been checked. It is, of course, clear that whenever a traction motor is to be thus used as a generator it must be reversed, otherwise the first effect of generation would be to oppose the residual field and reduce it to zero. It sometimes happens that the voltages of two motors in parallel so nearly balance each other that the difference in potential is insufficient to quickly build up the fields, and this insufficiency may be aggravated by resistance at the brushes or some other part of the circuit. Under such conditions the car would run some distance before the generation of a current strong enough to act as a brake. To avoid this possibility "emergency switch" connections are now so made that the motors are independently short-circuited, and the full potential of each motor is brought at once into play. The effect of thus turning the motors into generators

Mr. Baylor is practically instantaneous, and the electrical and mechanical strains are excessive. It is clear that the magnetic drag upon

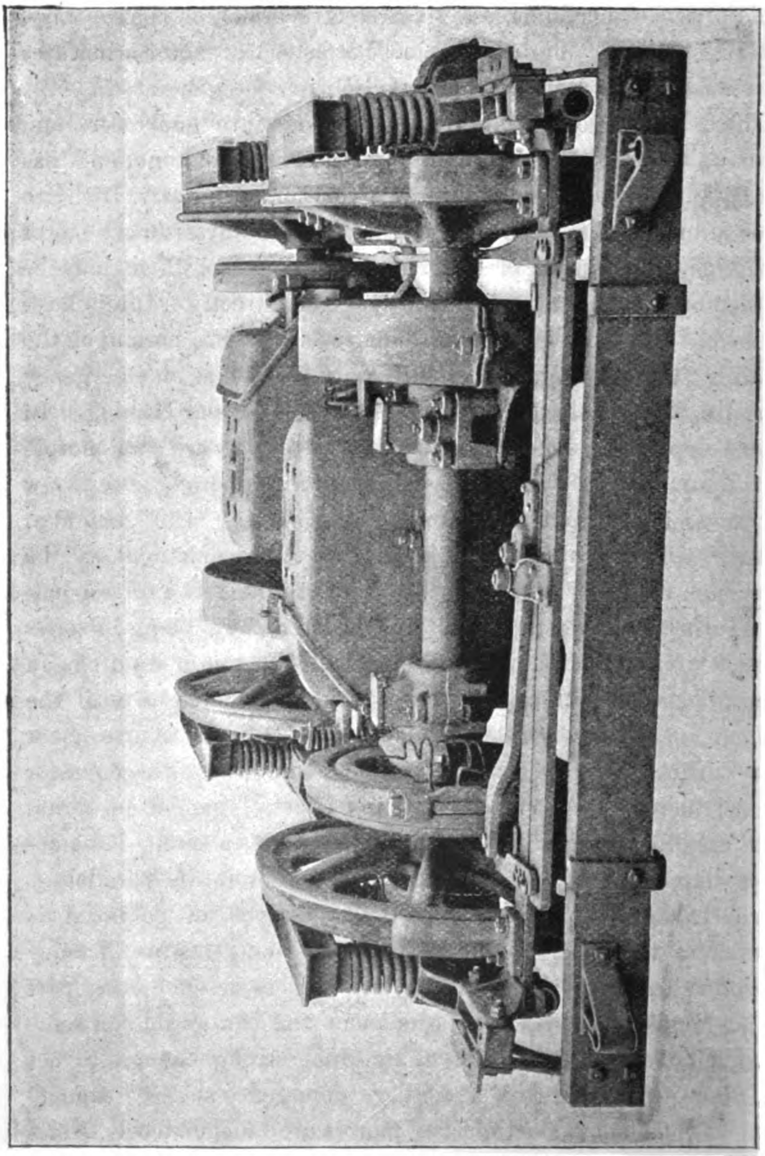


FIG. 20.

the armatures due to their generator action furnishes the entire

retarding force, and the braking effect is delivered to the wheels Mr. Baylor. through the gearing. Gear wheels or pinions have been repeatedly stripped or broken in this way, and frequent use of this method of stopping is a serious tax upon the insulation of the motor windings. However, to avoid collision or running down a pedestrian such a switch is effective, and therefore valuable, and at such times the breaking of a gear or the burning out of a motor is not to be considered. The strain upon the motor may be reduced by introducing resistance in circuit; but if this resistance is sufficient to afford full protection to the motors it will be impossible to stop quickly, and therefore the value of the arrangement as an emergency brake is destroyed.

The latest form of electric brake is based upon an entirely different principle of action, and permits of the car being controlled at all times by the motors in stopping as well as running. The arrangement consists, as shown in Fig. 20, of a circular iron plate made fast to the axle and turning with it, and an electro-magnet also, in the form of a disc, which is mounted so that it cannot revolve. The magnet disc is usually mounted (as shown

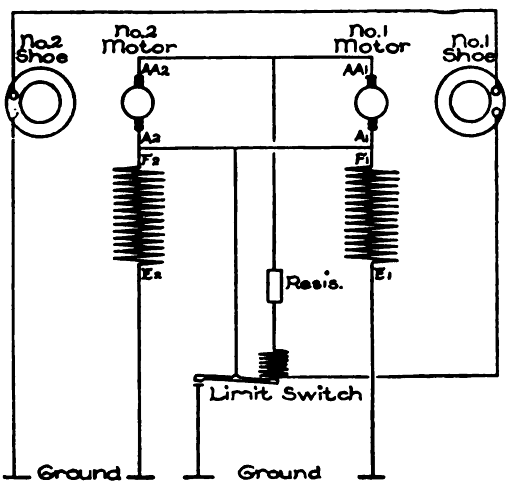


Fig. 21.

in the cut) upon the motor frame. In the form shown, one of the axle bearing caps is cast with projecting lugs to support it.

Mr. Baylor With this form of brake all forces tending to urge the car ahead are opposed directly and simultaneously, reducing internal strains to a minimum, and bringing the vehicle to rest in the least possible time. Fig. 21 shows the circuits for a two-motor equipment. The current from the motors is thrown by graduated steps through the magnet discs, bringing them into close contact with the axle discs, the resulting friction retarding the latter, and with them the car wheels, while at the same time the generator action going on in the motors tends to bring the armatures to rest, thus increasing the braking effect, and relieving the gearing from strain.

In addition to these forces, there is another retarding action, due to the eddy-currents set up in the axle plate as it revolves in the field of force surrounding the magnet. From the curves shown in Figs. 22 and 23, it will be seen that this Foucault current effect is an item of considerable importance in the efficiency of the brake. Fig. 22 shows the static pull, in pounds,

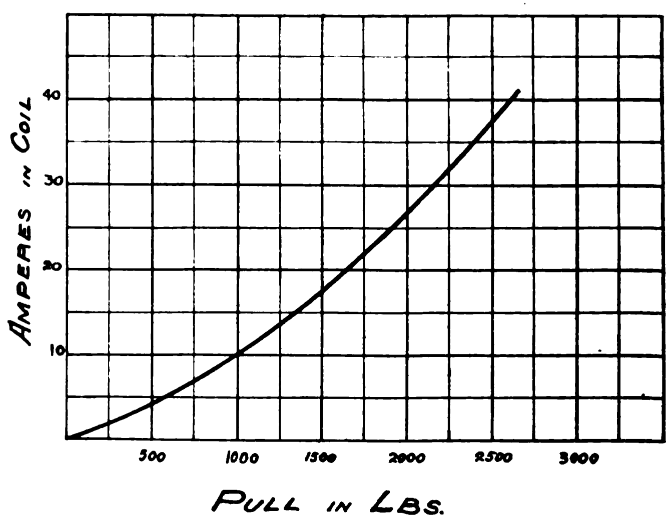


Fig. 22.

on the magnet disc with a given excitation in the coils, with the car at a standstill. In Fig. 23 the curves represent running tests. The upper set of lines show the actual coefficient of retarding effect for different currents in the brake circuit, and the lower set

of lines represent the coefficient of retarding effect for corresponding mechanical pressures, all readings being taken, as nearly Mr. Baylor.

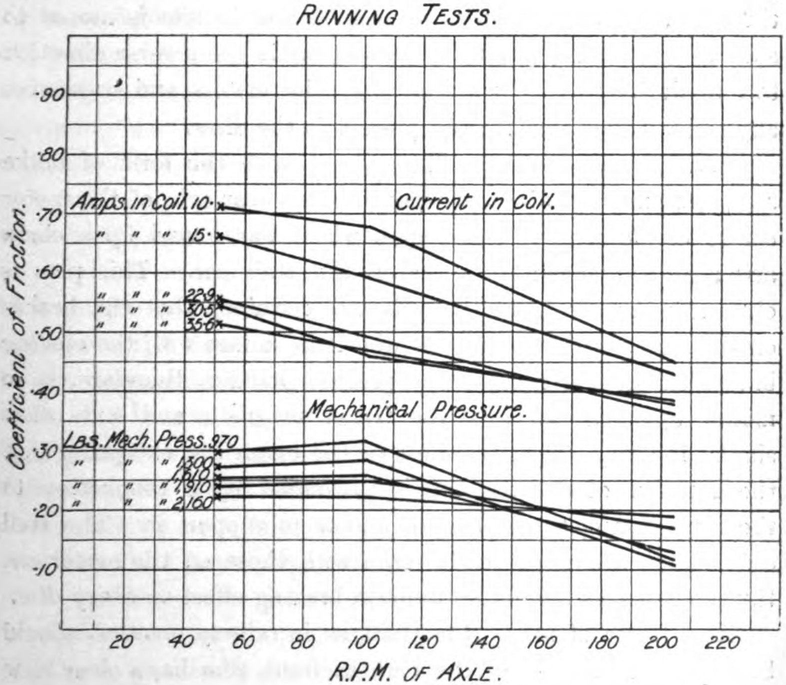


Fig. 23.

as possible, under similar conditions. It will be seen from these curves that the actual efficiency of the electric brake at a given current is about twice what it would be if acted upon by a direct mechanical pressure corresponding to the static pull with such a current. This additional retarding effect is due to eddy-currents and magnetic strains set up in the shoe and disc, and is, of course, secured without wear on the parts.

To prevent excessive wearing, hard metals must be used for the discs, and some difficulty was at first encountered through residual magnetism, and the tendency of the discs to cling together after the car had come to rest, requiring an excess of power in starting again. It is, of course, impracticable to use soft iron, as the wear on the friction surfaces would be excessive; but

Mr. Baylor. the difficulty has now been obviated by connecting the brake circuit, through a resistance, in shunt with the first running position of the controller, by which means a weak current is sent through the brake magnets as soon as the handle is moved to start the car. The shunted current flows in the reverse direction to that delivered to the magnets by the motors, and overcomes any residual field that may be present in the discs.

Formerly, in dealing with trail cars, with this form of brake a separate magnet disc was placed on the rear axle of the motor car in the form of a drum, upon which was wound up a chain actuating a mechanical brake on the trail car. This plan is obviously imperfect, as there is no certainty that the brakes on the rear car will retard the wheels in unison with the electric brakes on the motor car. The present practice, therefore, is to mount upon the axles of the trailer magnetic and axle discs similar in every respect to those on the motor car, excepting that the rotation of the magnet is prevented by a connection to the truck frame, there being no motor to support it. The trail car magnets are connected in series with those on the motor car, thus ensuring an absolutely uniform braking effect on every disc.

As a factor of safety, the train, as in railway practice, should be under full control of the man in front, who has a clear view before him, and who must in all cases of emergency rely upon himself only to avoid accident. With a proper system of signalling, an independent brakesman on the trailer, if skilful, may under normal conditions stop his car in approximate unison with the motor; but he is of absolutely no assistance to the motor man in cases of sudden emergency, and the added inertia of his car at such times reduces the efficiency of the brakes upon the motor. As a factor of economy in power consumption also the motor man should control the train, as much current is wasted through too early application of the trailer brakes imposing a drag upon the motor.

An electric brake has an advantage over any simple friction device when the car must be stopped within a minimum distance. It is well known that a better braking effect can be attained by retarding the wheels up to a limit just below the slipping point,

than by passing this point and skidding the wheels; and not only does skidding prevent effective braking, but it usually means flattened wheels—a result both troublesome and expensive. Under the most favourable conditions it requires skill and precaution with ordinary friction brakes, whether operated by air or by hand, to bring a car to rest without skidding the wheels, and in emergency the first impulse of the motor man is to set his brakes hard, which will, as a rule, cause skidding. Thus at a time when a quick stop is most necessary there is least liability of its being effected with mechanical friction brakes. With an electric brake this tendency to skid the wheels is prevented automatically, for the armature is connected through the gears and axles to the wheels, and the latter cannot stop turning without also stopping the armature, and with it the current from which brake action is derived. When the wheels begin to turn again, they are again retarded until the car has been brought to rest. Mr. Bayler.

It should be noted that this action is essentially automatic, and requires no skill and experience on the part of the motor man, being, in fact, quite beyond his control; therefore with such a braking device greater speeds are possible with a given degree of safety, and the personal factor of the motor man in this particular is practically eliminated.

It has been found in practice that even with an electric brake a slight flat wheel might occur if the full current from the motor were free to flow through the magnet discs. This is due to the fact that the first surge of current would stop the wheels; and, although they would begin to turn again, an appreciable time would be required to overcome the inertia of the armatures and the sticking of the discs, and during this interval there would be danger of skidding. Moreover, the first flow of current from the motors under these conditions is far in excess of that necessary to stop the wheels, and the surplus only heats the motors, imposing a strain on the insulation of the windings while doing no useful work. To overcome this difficulty, an arrangement known as the "limit switch" has been provided, which consists of a small coil of wire in series with the brake discs. When energised, this coil lifts an armature, making connections which cut out the fields of

Mr. Baylor. both motors, thereby stopping the current. The armature of the limit switch is controlled by an adjustable tension spring, which is set to allow for the current being cut off at a point which prevents the wheels from actually stopping. This limit varies with different weights of car and different conditions of track, but with the ordinary electric car and average track conditions it is between 30 and 35 amperes; and when this is divided between two motors there is, of course, no danger of heating the conductors or destroying the insulation.

It should be noted that but for the residual magnetism left in the discs, and their consequent tendency to stick together until demagnetised by the line current, an electric brake would not hold a car absolutely still on a gradient. It is evident that, when the car has been stopped and the current from the motors ceases, the wheels, but for this sticking, would begin to turn again, as already described. As a matter of fact, on heavy grades this sticking is insufficient to hold the car, and unless the auxiliary hand brakes are applied the car will start again by gravity. As soon as it does this, however, the generating action begins again in the motors, and the effect in practice is that the car will move down the grade at a snail's pace, the current from the motors all but balancing its tendency to move.

The electric brake connections in the controller are usually placed upon the reverse side of the power cylinder, only one handle being used, which is turned one way to supply power and the other way to apply the brake, the same resistance with different connections serving with both circuits. It is possible to continue the shaft of the controller down through the platform, connecting it with the ordinary brake rigging and peripheral brake shoes (which should always be placed upon the car as an auxiliary). When arranged in this way the controller handle, after passing through the various braking steps, may be turned farther, setting the brake shoes, which will require but little pressure, the car being practically at a standstill. This seems inadvisable, however, as the brake shoes would in all probability be applied at every stop, and unnecessary binding of the wheels and possible flattening would ensue. It may be noted, however, that with an

electric brake one flat spot on a wheel has no tendency to produce another, as is the case with peripheral friction brakes that tend to grip the wheel where flattened, causing skidding that would not normally occur. Mr. Baylor.

The saving by the use of electric brakes in wear and tear on wheels and replacement of brake shoes is a considerable item of economy in ordinary service. The wear on wheels due to the grinding action of peripheral brake shoes where frequent stoppages are necessary amounts to more than that due to rolling friction on the tracks, and the brake shoe has to bear its share of the effects of this grinding action.

A source of considerable waste in power consumption on individual cars with ordinary equipment is the tendency of motor men to put on brakes before power is completely cut off, and to throw on power before the brakes are fully released. With an electric brake operating from the reverse side of the controller cylinder this is impossible, as the handle must pass through zero, or "off" position, in going from power to brake, and in going from brake to power.

Various forms of so-called electric brakes have been devised whereby the ordinary rigging and shoes are employed with some form of electro-magnet to take the place of the usual handle in applying power to the chain. Such brakes may be operated by current from the overhead line or from batteries on the car. The brake deriving its power from the line is unreliable, as the trolley may leave the wire at a critical moment, rendering the brakes inoperative, and storage batteries necessitate the addition to the car equipment of heavy and expensive apparatus. In common with air brakes, such devices are open to the criticism that in providing an auxiliary it is necessary either to duplicate the rigging and shoes—which is impracticable in the space, already overcrowded—or to rely upon a reserve source of power only, trusting to one set of shoes and connecting mechanism. With air brakes, too, they must be classed as uneconomical for electric cars, since such cars must of necessity embody in the motors an ample and flexible braking power, whether it is availed of or not, which runs to waste when any other force whatsoever is used

Mr. Baylor. in braking. We might as logically run our electric cars by horses, with the motors dragging upon the axles, as to use some other and less effective means of braking than that obtainable from the revolving armatures while their inertia urges the car ahead.

Mr. Dawson. Mr. PHILIP DAWSON: I must congratulate Mr. Baylor on the excellent paper given to-night, and the material exhibited. With regard to the three-wire system, I have seen most of the prominent electric tramway systems of America and Europe; and when I first visited the States some years ago, I thought this system might be capable of great extension. I have since found that, although tried in one or two instances, it has been abandoned, the practical difficulties in its application not having been yet surmounted. A much better system is that of using high-tension alternating current, with sub-stations where the high-tension alternating current is transformed into continuous current at working voltage. This system, as you all probably know, has been very successfully applied in Dublin; and its economical results are quite as good as, if not better than, those claimed for the three-wire system. On the Continent, while the application of the three-wire system has been watched with great interest, it has never been employed. There they have used alternating currents, especially polyphase currents, and particularly in power transmission they are very much ahead of us. As to three-phase motors, the only car-motors now operated by three-phase current are to be found at Lugano. I saw them last year, and the one great difficulty encountered was the tremendous induction caused in the telephone lines. It upset the entire telephone system, and I doubt if the difficulty has yet been overcome. As regards the efficiency of electric brakes, I had a little time ago a very good opportunity of testing the emergency type exhibited by Mr. Baylor. A child ran very close in front of a motor car, which was coming down an incline of about 1 in 18, with a trailer car attached. The ordinary brake could not have been applied in time, but with the emergency brake the car was stopped within half a length. That certainly saved the child's life. On the Continent electric brakes are much used. In fact, most of

the roads in Germany and Switzerland are now adopting that type of controller of which it forms a part. Mr. Dawson.

Mr. J. S. RAWORTH: I think we must all congratulate Mr. Baylor on his excellent choice of a subject, which has brought together, I think, the best meeting that we have had this session. At the same time I cannot help feeling that to some extent Mr. Baylor has suffered in the nature of his audience. He would, I am sure, have been better pleased if he had had before him a much larger and more representative section of the great British public; for I feel in reading this paper that there is a very considerable portion of it which we can scarcely consider as being addressed to this Institution. I refer to those pages which explain to us in considerable detail the great advantages to be derived from direct coupling. We have on my right a gentleman (Mr. Crompton) who, if he be not the father of direct coupling has at least addressed this audience on many occasions, many years ago, and has expounded those advantages in such cogent terms, that one cannot now find an engineer in this country who is prepared to do any other than direct-couple his dynamo to his engine. Mr. Raworth.

Although I am sorry we have not had the great British public with us to hear those excellent reasons which Mr. Baylor has advanced in favour of direct coupling, yet I am glad we have had the paper in such detail; because, when it is printed in the Proceedings of this Institution, we shall have a definite and lasting record that the practice of the United States was so many years in arrear of our English practice. I think this fact is indisputably proved, because we find, amongst other things which are recommended to our notice, the multipolar dynamo in place of the bipolar. Now we have gone through the multipolar dynamo many years ago; we have made it a success in England, and I have no doubt it would have been a leading feature of English practice to-day if we had had any demand for dynamos of such size that the advantages of multipolar dynamos could have asserted themselves above the bipolar. But there are certain advantages in the bipolar dynamo which we in this country have looked upon as paramount, and to which we have

Mr. Raworth. subordinated many other considerations. The feature I particularly wish to draw attention to, is that with the bipolar dynamo, constructed upon the lines which Mr. Crompton and Messrs. Siemens have advocated, we have obtained a sparkless commutator, which assumes a dark blue polish like a looking-glass, and which never gives the user the slightest trouble. When you turn from the bipolar dynamo to the multipolar with a cog-wheel armature, you have to give up that very desirable feature and put up with more or less sparking, which Mr. Baylor has alluded to. But I should like to mention this fact: that a multipolar dynamo will very shortly be one of the ordinary products of England—not in the form of a dynamo which sparks, but in the form of a dynamo which will give those excellent results which you have been in the habit of obtaining, and which you have a just and perfect right to expect. Those results have been made possible by the work of Mr. W. B. Sayers, who has shown us how to produce a dynamo with a very short air gap in which the sparking could be controlled, and with which a commutator could be worked with practically the same results as with the Crompton or Siemens two-pole dynamo. That work is being continued by Mr. Mordey, and I believe the improvement will shortly be applicable to all the dynamos of commerce.

I now come to the next point of Mr. Baylor's paper, in which he informs us that steel is much superior to cast iron for dynamo-making purposes. Well, we all know that!

He goes on later to criticise the practice of the United States. I do not think he means his remarks to apply to our practice, because he says that it is their habit in the States to specify an engine so closely that when they get the engine it will do the work desired of it. When they specify the size of cylinders, steam pressure, and the cut-off, the result is exactly what they wish; but they do not get the dynamo they require when they order it, because either they object, or they forget, or they decline to put in what we look upon as a fundamental principle of the specification, viz., a temperature limit—I will not say a sparking limit, because we do not allow it at all. Now these matters, in the view of some people, may not

apply to such an occult subject as electric traction. I have seen certain specifications that have been issued in this country for electric traction work, from which were omitted those all-important clauses which we have come to consider essential features of English practice: they did not specify any condition as to sparking; they did not specify any temperature limit. They *did* specify that the dynamo should give from 500 to 550 volts, but whether the 550 volts were to occur at the maximum load or at minimum load was not stated. Nevertheless, they went one step further, and specified that the dynamo should have cast-iron magnets. I tried hard to get to know the reason for putting in that particular clause as to cast-iron magnets, and ultimately I found that the real object was that the said dynamo should be made by the General Electric Company of Schenectady. I do not desire to carry the criticism of the paper any further, because at this late hour it would be unamiable on my part to do so; I much prefer to back up Mr. Baylor in what I take to be his dissatisfaction with the methods in vogue in the United States, and in the great admiration for our methods which he has probably acquired since he came to this country. Mr. Raworth.

Mr. E. W. Rice, jun.: I did not come here this evening with the expectation of addressing this meeting, but rather for the pleasure of listening to the paper and seeing such a representative body as the Institution of Electrical Engineers of England, of which I have the honour to be a member. I have listened to the paper with a great deal of interest, although, naturally, many of the matters are quite familiar to me. I certainly agree with Mr. Raworth in his praise of the excellent and early work accomplished by England and by the Continent in direct-coupled apparatus. At the same time, some of the earliest efforts in direct-coupled apparatus existed in the old Pearl Street station in New York—the famous Edison "Jumbo" dynamos, some of which I recently saw operating in Milan, Italy, having been in continuous operation for a period of something like 12 years. Of course these machines are only interesting historically, and would hardly be taken to-day as a model for construction. While it is quite true that the English and Continental engineers led the American Mr. Rice.

Mr. Rice. engineers by many years in the general introduction and application of direct-coupled dynamos, I think that, if the previous speaker could have the pleasure of inspecting some of the latest engineering work in the United States, he would see a great many very large and excellent direct-coupled plants, many of which I do not think can be equalled in the world. In one of the stations of the West End Street Railway Company of Boston, for example, he would find eight machines of 1,500-kilowatt capacity, each direct coupled to an engine of about 2,000 H.P., running at a speed of 75 revolutions. These machines will run to a load at least 50 per cent. above that of their normal capacity without any sparking, and with a commutator which I can assure you has this beautiful glossy appearance which you so highly and justly prize The apparatus in this station, one of the largest which is in existence in the United States, can be duplicated in Brooklyn, New York, Chicago, Philadelphia, and many other places. Of course these stations are used for electric traction; in fact, the particular station I mentioned has a capacity of at least 20,000 H.P. I may say that to-day the direct-coupled dynamo is the standard in the States for all machines for lighting or for railway, or for any other work, even in small sizes—wherever the customer will pay for them.

I do not suppose that the previous speaker intended that his remarks as to the alleged specification in which cast iron was demanded, and heating and sparking conditions omitted, so as to enable a certain company's apparatus to be used, should be taken seriously. If he did, and in case the American company of the name mentioned was intended, I would say, speaking as one familiar with—in fact, responsible for—the practice of its engineers, that while soft steel and other excellent magnetic materials are extensively used in the making of that company's apparatus, the properties of cast iron, both mechanical and magnetic, are not unknown, and cast iron is therefore used without hesitation where the engineering conditions demand it. It is also true, while many firms show such confidence in that company's product as to order its apparatus without heating and sparking specifications, that such requirements are not entirely unknown in American practice.

I was much interested in Mr. Baylor's remark in reference to the application of the three-wire system to electric traction. The extent of the application of electric motors for traction purposes in the United States is undoubtedly familiar to you all, and does not need any description from me; and in this increased application we have come to a point where long distances are to be covered. Starting from Boston, it is possible to ride in an electric car for at least 50 miles in almost any direction, except out to sea. This is true of almost all our large centres—New York, Philadelphia, Cleveland, Buffalo, and so on. In covering such large distances—20 to 50 miles—there are two methods which have grown up. One, due to the gradual extension of such lines, is using a number of separate power stations. This has been the natural evolution in consequence of the fact that the railways frequently connect the large centres with outlying suburban districts, or, it may be, several cities with a large number of inhabitants. In such cases, each of these cities, having its large central station, supplies the current to the cars within a radius of from five to eight miles. However, the cost of the feeders and copper required in such large distances as are now under consideration has brought forward other systems, and among the systems tried has been the one mentioned by Mr. Baylor—that of the three-wire system. In several instances, to my knowledge, this system has been proposed and actually employed—in Portland, Oregon, in Bangor, Maine, in Milwaukee, Wis., and other places—and after a careful trial was abandoned, chiefly on account of the difficulty of balancing the current on the two sides of the system. This is reasonable when one considers the variation in the amount of energy required by the different parts of an electric traction system. Therefore, in searching for a practicable method, the generation of electricity by means of multiphase generators in a large central station, its transmission to sub-stations, at which sub-stations alternating-current motors of multiphase type were used to drive direct-current generators, which generators were attached to the local 500-volt circuit, have been employed. Such a system, however, has been found to be somewhat inefficient as compared with the Mr. Rice

Mr. Rice. more modern method, and one now frequently employed, and which will undoubtedly be extended in future. This system is a modification of that I have just mentioned, and consists, briefly, in the generation of the electricity by means of two-phase or three-phase—generally three-phase—currents in a large central power station, and its transmission over aerial or underground lines at high pressure—2,000 to 5,000 volts—to sub-stations located at different points along the line. At these sub-stations the high-pressure alternating current is reduced in pressure and converted into 500-volt continuous current by being passed through so-called step-down transformers and rotary converters. The static transformer is of the usual construction, and artificially cooled in large sub-stations. The rotary converter is, as you are probably aware, essentially a direct-current dynamo-electric machine, provided with collector rings, three or four in number, according to the system employed; these collector rings have suitable connections to the proper points in the same armature winding to which the commutator is connected. Such a machine becomes, therefore, a commutating device for the alternating current. It differs from, and is superior to, a motor-generator in that the alternating and direct currents combine in the same winding so as to materially reduce the armature reaction. In practice the rotary converter behaves as if it possessed no armature reaction; the point of commutation remaining fixed over the entire range of output, without any sparking—in fact, the commutating properties of the rotary are not equalled by the ordinary generator or motor. At the same time the losses due to hysteresis and eddies, as well as the $C^2 R$ in the armature conductor, are materially lessened by combining the two functions in the same armature. As an obvious result the output per pound of material is large, and the efficiency extremely high.

In comparison with the alternating-current motor direct-current generator system, there is a net gain of from 5 per cent. to 7 per cent. in efficiency, in addition to the other advantages mentioned.

The efficiency of the static transformers used in such a system is extremely high, ranging from 96 per cent. to 98·5 per cent., including waste in ventilating apparatus.

If time permitted, there are many other matters I should like to discuss, but I have already taken so much of your time that I will close my remarks with many thanks for your kindness in listening to me. Mr. Rice.

The PRESIDENT: I have several names of speakers before me, but at this hour I feel compelled to adjourn the discussion until April 22nd.

I have to announce that the scrutineers report that the following candidates have been duly elected:—

Members:

Harold Dickinson.
John Westbeech Kempsier.
Frederick Henry Royce.
K. A. Scott-Moncrieff.

Associates:

Henry Alfred Barnett.
Thomas Morland Colson.
Victor Herbert Gregory.
Edward Albert Mitchell.
William Henry Smith.
Harry Edward Yerburg.

Student:

Norman McL. Lawrance.

The Three Hundred and First Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, April 22nd, 1897—Sir HENRY MANCE, C.I.E., President, in the Chair.

The minutes of the Ordinary General Meeting held on April 8th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

A. L. C. Fell. | Charles Edward Grove.

From the class of Students to that of Associates—

Archie Corbett Seaton. | Frederick M. Walker.

Mr. A. H. Dykes and Mr. W. J. Grey were appointed scrutineers of the ballot for new members.

The PRESIDENT: Gentlemen,—May I ask your attention for a few moments while I make a brief announcement? I think we are all agreed that this, the Diamond Jubilee year of Her Majesty's reign, is one which will be memorable in the history of this country. I think you will also agree with me that the occasion is one on which every loyal subject would desire to join in congratulating Her Majesty on the completion of the sixtieth year of her reign. We may take it for granted that every learned Society throughout the kingdom will submit a suitable address, and the Council feel sure that they are only anticipating the wishes of the members of the Institution in deciding to do the same. (Cheers.) I have only to add that, as soon as the address is ready, it will be placed on the table for your inspection, before being presented.

The discussion on Mr. Baylor's paper will now be resumed.

Mr. WALLIS-JONES: I think this Institution is to be congratulated upon the very excellent paper which Mr. Baylor has read. Mr. Wallis-Jones.

The magnetic brake which he has described is very characteristic of American practice in designing a piece of apparatus which is so eminently practical, and which can be used by the most unskilled labour.

I refer particularly to the point that the operator cannot use the brake until the power supply has been cut off. That is a most important feature.

I should like to say a few words with reference to the electric welding of rails.

In this country at the present time there are no electrically welded tracks; but then it must be remembered that there are only something like 80 miles of electric tramways or electric railway in operation here at present, and most of the municipal authorities are only beginning to ask, "What is electric traction?"

I am quite sure, however, that when electric traction develops in this country, the question of electrically welding rails into a solid track will be prominently brought forward.

It will be somewhat interesting to note the various stages through which the electric welding of rails has passed.

The greatest difficulty in the earlier stages was the liability of the rail to fracture at the weld. The first method of welding adopted was that described in Mr. Baylor's paper, viz., by welding the ends of two U-shaped pieces of iron into the web of the rail 4 inches on either side of the rail joint. As a matter of fact, these did not stand very well, as it frequently happened that the web of the rail was torn away where the weld occurred.

In 1894 another method of welding was adopted, viz.: a chock of iron or steel, about 1½ inches wide, was placed on each side of the rail joint, and the whole was welded together solid. This was an improvement, but the percentage of breakage was still too great.

The present method of welding rails is a great advance upon anything that was done in the earlier years.

Mr Wallis Jones.

The rails are now welded end to end, and a thoroughly solid butt weld is made; but the real secret of the present success is the method of welding which has been adopted, and which is known as "snap" welding: *i.e.*, the rails are clamped with a very short projection, the ends are heated as rapidly as possible, and when the right degree of heat has been reached they are squeezed together with as much pressure as possible, the heated metal being squeezed out around the joint in the form of a fin. The results obtained so far have been most successful.

I might perhaps mention that the cause of breakage of continuously welded rails was the strain set up by excessive cold in mid-winter.

It may be taken that 100° represent the maximum fall of temperature between the temperature at which the rails were welded and the lowest winter temperature in the United States. On a 90-lb. rail this represents a strain of something like 157,500 lbs.

When the rails are welded in the manner last described the average minimum strength that can be guaranteed at the weld is something like 350,000 lbs., so that there is a large margin of safety.

It is almost needless to mention the great advantages to be gained from an electrically welded solid track. Briefly, they are as follows:—

1. All trouble in connection with bonding is done away with.
2. No electrolytic troubles arise, as the metals are of the same nature and section throughout.
3. Travelling on a solid track is exceedingly smooth, and wear and tear of rolling stock largely reduced.

To illustrate the method of "snap" welding, I have placed on the table a sample of steel tyre which has been welded in this manner—*i.e.*, very quickly (about 10 seconds), with short projection, and pushed together with great pressure, with practically no subsequent forging.

The tyre in question is about 3 inches wide and about 9-16 inches thick in the middle.

The sample has been broken in a testing machine, and you will

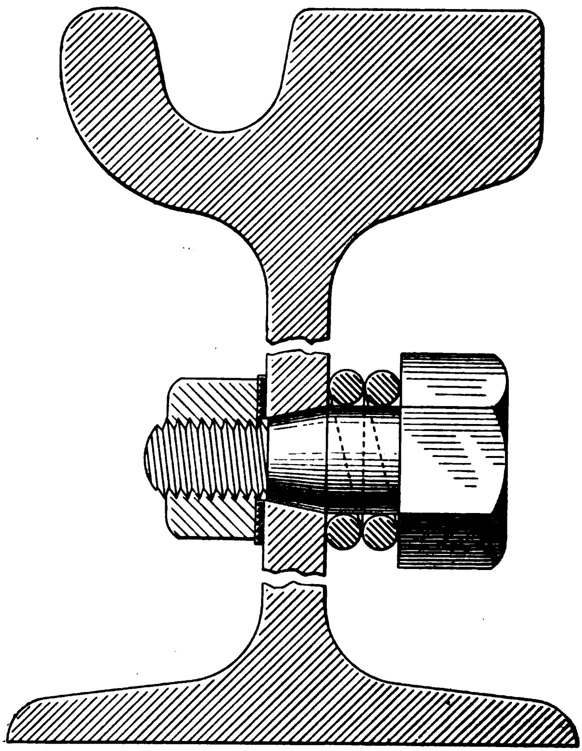
note that the fracture has occurred some 6 inches away from the weld, and this without any predisposing cause, such as punch or chisel mark. Mr. Wallis-Jones.

Four samples, of which this was one, were welded in a similar way, and gave the following results in test :—Breaking strain of sample in ordinary condition, no weld, 26·184 tons per square inch; average breaking strain of four samples electrically welded, 26·9 tons per square inch; average elongation in 5 inches of four samples electrically welded, 11 per cent.

I think that you may take it that welds made in this manner are now quite successful.

Mr. STEPHEN SELLON: I think we ought to be deeply grateful to Mr. Baylor for having provided us with so much experience in connection with the introduction—for it is introduction—into this country of electric traction. The point, however, on which I want to speak to-night is in reference to that portion of Mr. Baylor's paper which refers to bonding. I cannot help feeling that electrical manufacturers do not recognise the vital importance of this in connection with traction in this country, which is, if I may so call it, the genesis of the whole business. I fear that the utility of these discussions is much handicapped by the growing practice of self-advertisement—whether one dynamo is better than another, or whether one engine is better than another. The great point we have to consider, and one which I am sure those who followed the proceedings and the evidence during the Joint Committee inquiry in connection with the electrical protective clauses, will agree with me is that the whole question of the success of electric traction in this country depends upon the satisfactory solution of the leakage problem. I have, therefore, during a long course of experience, paid some particular attention to the possibility of overcoming by any means this first difficulty, and so minimising the dangers of electrolysis. I am not here to suggest any particular system of bonding. I want to bring before your notice that I have seen certain so-called bonding carried out in this country which will without doubt, if practised, destroy this growing industry. The experience in America with regard to Mr. Sellon.

Mr. Sellon. electrolysis was, we all know, very bad, although it has now considerably improved; but we do not want to fight those battles over again here. The vested interests in this country are very much larger than any vested interests in America, and if once we find electrolytic action in any gas or water pipes which belong to the municipalities, it will give the death-blow to electric traction in this country. The subjoined diagram shows



a method of bonding which I have noticed lately introduced by an electrical firm of some standing in this country. The head of an ordinary bolt is connected by brazing or soldering to a copper wire; the bolt is turned down conically and fitted into the rail, the rail having first been bored out with the ordinary drill, and then rimered out afterwards. It is then bolted up by a nut on the other side, drawing the conical sides together. Both those attachments are iron or steel—steel in the case of the rail, of

course, and iron in the case of the bolt. I ask any engineer here who has really studied the question of bonding, for this or any other country, whether he considers this a satisfactory method of bonding, and what electrical connection there can be in a few months' time? I do not believe the manufacturers in this country have seriously considered what is the genesis of the first procedure of electric traction. It is hard that the success of financial companies who spend their time and money in obtaining powers which eventually involve the use of plant, and, therefore, business, to the manufacturer in this country, should be jeopardised by procedure of this description. I had occasion some time ago to enter into communication with a firm of some considerable notoriety in this country in respect to copper requirements. I was discussing with one of the firm the question of bonds, and that gentleman told me that he was surprised I should consider for a moment that a bond was necessary; and he asked me quite seriously whether I had ever considered the substitution of an ordinary auxiliary conductor put down at the side of the rails. I mention this to you simply to show that, while we are discussing the merits of one dynamo over another, or one machine over another, the manufacturers in this country have not even considered the mere alpha of the whole business. Those are the few remarks I wish to make to-night, simply to suggest to you that we should consider the question from its foundation, on the solidity of which depends the future of what promises to be an important industry. Mr. Sellon.

Mr. HOLROYD SMITH: When Mr. Baylor read his paper he omitted much that is printed in the proof. I assume, however, that the discussion is not to be confined only to what he read, but that it may also be extended to the many and interesting exhibits on the table. In the first place, allow me to compliment the author most sincerely upon the comprehensiveness of his paper; but I regret, with Mr. Raworth, that this audience, or the audience which listened to it, was not composed of municipal authorities, tramway shareholders, and the public generally. It would have been a wholesome lesson, setting out as it does the numerous problems that have to be considered and solved before Mr. Smith.

Mr. Smith. an electric tramway can be efficiently constructed and worked. This is a Society of Electrical Engineers, organised for mutual edification and encouragement. Now with the title, "Some "Recent Developments in Electric Traction Appliances," it was natural for us to expect some addition to the details of our technical knowledge on the subject. If the title had been "Modern Practice," or "A General Survey" of electric tramway work, then the paper would have been more consistent with its title, and I could, and still do, most heartily endorse and support the sum and substance of the paper. Direct driving instead of belt driving, multipolar instead of bipolar dynamos, armatures built, not wound, regulating the shunt resistance, &c., &c., are all matters of interest, and important factors in successful working; but, unless some particular method were shown of enabling these desirable objects to be obtained, they do not come under the title of recent appliances or apparatus. The general questions have been ably dealt with in previous papers read before this Society. A good deal of space is given to controllers, and the advantage of the series-parallel method set forth, but the precise details of the latest apparatus for this purpose are not explained. The general description given would apply equally well to the apparatus that I have ventured to put upon the table. There are, no doubt, a great many other members who could introduce similar apparatus for producing a series-parallel arrangement of dynamos and resistances. It will be readily understood by those versed in the modern arrangement, though I admit the puzzle is inside the cylinder. I have abandoned its use, because the result can be obtained in a simpler and more durable manner, and without the intricate complication of the series-parallel controller shown upon the floor, by a controlling switch that I should be glad at any time to describe to this Society. The part in the paper which interests me most is that dealing with the system of electric brakes, and it is introduced in a most refreshing manner. On page 14 of the proof will be found these words: "The idea of converting traction motors into generators "and utilising the current for braking purposes is an old one, and "has been applied in practice to a limited extent for many years."

Such a sentence is encouraging. I fully endorse the statement that the idea is old, and that it has been sucessfully used on the steepest gradients years ago in this country, and that no smashing of the gearing has resulted therefrom; and I feel indebted to the author for recognising my early work, though in this somewhat vague and general way. Now I want to ask a question. Does the author think that making the motors act as dynamos, generating a current to energise an electric brake, is a new method? The fact of so doing is not a recent development, neither is the employment of motors to wind up a brake that shall act on the trailing car new. There may be something new in the manner in which it is done, but there is nothing in the paper to show clearly how the brake is constructed and operated, and therefore without such information it is impossible to criticise the details. I fully endorse the opinion expressed as to the desirability—one might almost say the necessity—of utilising the motors as the brake power, and I am glad to find that this is at last realised; but the success of any principle depends very much upon the way in which it is carried out. I claim that when a thing is praised, and its good work eulogised and brought before the notice of a technical institution, the members have a right to expect to be shown precisely how it is done. Failing to find these recent appliances in the paper, I turn to the exhibits on the tables. The one that might fairly be supposed to be the most recent is the trolley wheel labelled, "Provisionally protected"—a label that seems to me to be hardly in accord with the understanding that questions of patent right are not to be discussed here. I do not raise the question of patent; I simply say that the exhibit so very closely resembles a figure used to illustrate a paper read before the Northern Society of Electrical Engineers on the 8th of April, 1895, that I fail to see how it can be described as a recent appliance. Allow me again to express my appreciation of the paper as an able exposition of the requirements governing modern practice; and again to express my regret that it is not more in accordance with its title. Mr. Smith.

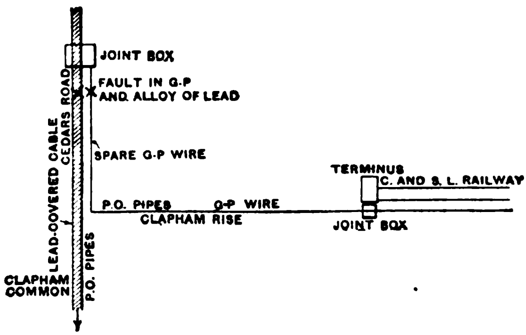
Mr. W. H. PREECE: As an Institution, I do not think we care much who invented this or who invented that; nor do we Mr. Preece.

Mr Preece. mind whether the invention came from England, from America, from Germany, or from France. If there is one thing that distinguishes the electrical fraternity and the practical applications of electricity, it is that they are cosmopolitan. We use the same language, the same measures; and whatever branch of electricity it may be, we in England do not care from what country it comes, and we receive with equal satisfaction a new thing from Kamschatka as from the other side of the Thames. It happens that in 1884 I visited America. At that time there was only one experimental electrical railway in the whole of that country. I went to see it. It was at Cleveland, and I gave an account when I returned to this Institution of what I saw. In that same town of Cleveland, where there was then this single little railway, there are now as many, if not more, miles of railway as there are in the United Kingdom of Great Britain and Ireland. In 1893 I again visited America, and there I found that this single pioneer railway had grown up to something like 10,000 miles of line. I believe that it can now be shown that the mileage of railway worked electrically in the United States does not far fall short of 20,000. I think that of all the practical applications of electricity, of all the illustrations of the applications of engineering, there is nothing more wonderful and nothing more startling than the terrific and tremendous growth of this industry in the United States. We are very much indebted to Mr. Baylor for summarising the experience gained in that country, and in the style and manner in which he has done it. His remarks are not confined to American practice; they deal just as largely with practice in Germany. We find there that, after all, the true mode by which the engineer should gain his knowledge—by experience—is fully brought before us, and we ought to receive it—as with one or two exceptions we have received it—in, as I hope Mr. Baylor will say, a proper and a scientific spirit. On the last occasion we were told that there was something in this paper that reminded one a little of bringing coals to Newcastle,—that it was no use coming here and trying to show us the advantage of direct driving, and of large units, because we knew all about that already in England. I should like to ask the speaker of

the other night if he can refer to one single instance in this country where direct driving has been applied to electrical traction. I have visited them all, I think, and I have not found or seen direct driving in use in this country.* We are rather proud of two large installations—the one in Liverpool, and the other on the South of the Thames—the City and South London Railway. Are they direct-driven? You will find they are rope-driven. Do you find direct driving in Leeds, in Coventry, in Blackpool—that we have heard a little about—or in any other place where electrical traction has been adopted in this country? We do not. We are only now in the South of London, on the City and South London Railway, commencing that experience which they have acquired in Germany and in the United States—that, after all, the correct way to drive this plant is by the direct method. There are a great many things in this paper that deserve discussion here. I will confine myself to one other—that is, the question of leakage and electrolysis. That, as Mr. Stephen Sellon pointed out, is a most serious question. I will give you an illustration on the board. We do suffer from leakage to a somewhat serious extent in this country. It is generally supposed that in this country we are ruled and governed by a kind of grandmotherly Government, which places in the hands of its officers certain powers that they perhaps occasionally have to exercise to the annoyance of the users. But I think that there should be a great difference drawn between interference of legislation and what is known as mere officialism. I speak from this point as an official, and I should like to see the man here who can stand up in this hall and say that, so far as the Post Office is concerned, he has ever been ground down by mere officialism. We have always done everything we possibly could to meet all these industries on their own ground—the ground of practice. This is especially so in the case of leakage. On the South London Railway we suffer very much from leakage. The rails are not bonded. There is no insulated return wire. The outside tube has been supposed to be a return conductor, but the rails Mr. Preece.

* Hartlepool is, I believe, an exception, but I have not been there.

Mr. Preece. alone act as the return. The outside tube does not approach a return. The result is we get great leakage. On this line, not far from Clapham, there runs an underground line belonging to the Post Office. I cannot give geographically the positions of the two, but the subjoined diagram will explain sufficiently the



conditions. In this line of pipes, there are some gutta-percha wires, and one lead-covered cable filled with about 30 paper-covered wires. There are two joint boxes separated from each other by, we will call it, 100 yards. Between these two boxes were some spare gutta-percha wires thrown out of use because faulty. The line of leakage was such that the difference of potential between those two boxes was sufficient to cause a current to pass through these spare gutta-percha wires, and about half-way a fault came from some cause—we do not know what—perhaps lightning—on one or two of these wires. The result was, that in about six months, or from that to twelve months, the current leaking through from the fault on to the lead decayed the lead—the lead was completely eaten away and the paper cable exposed to moisture—and all those 30 wires were suddenly broken down. That is a practical illustration of the effect of leakage on a telegraph line. What I desire to say in regard to all these difficulties, and all these troubles that we meet with, is that they are all remediable. The first thing is to find the cause of the fault: the remedy is then easy. And in the case of all these tramways, we know from the actual experience that Mr. Baylor gives us in the States,—we know from our own experience in England,—that

if the rails be properly bonded, that if you have a proper return of sufficient capacity to carry your currents back again, you get no outside field of leakage, as it were, and neither telegraph nor telephone, or lead water pipes or gas pipes, or anything else, will be disturbed. But I can say this from what I see now—that if something is not done in the South of London there will be trouble in the neighbourhood of Clapham and Stockwell, and round about the City and South London Railway. The same thing is occurring on a smaller scale in Liverpool. But I do not fear it in any of our new railways. The lessons of experience gained in the United States are being adopted by us in this country. Mr. Preece.

Mr. C. H. Gadsby: I had made a few notes on several points in the paper, but, as so much time has been already taken up, I will confine myself to the question of electric brakes. Electric brakes were first introduced in the United States after the adoption of heavy cars and high speeds. I do not think they are likely to find much application in this country for some time—at any rate, until the Board of Trade allow higher speeds than we have at present. One great objection to the use of electric brakes—and it is a point that has not been brought out in the paper—is that, in using the motors as generators to drive currents through the windings of an electric brake, you are at the same time dissipating heat that in ordinary practice is dissipated on brake-blocks and tyres, in $C^2 R$ losses in the armature. With reference to the diagram Mr. Baylor gave of the electric brake, I cannot make it out at all, and I think it is rather a misnomer to call the ordinates the coefficients of friction. I think coefficients of retardation, or something of that sort, would have applied better. There is one other point which I will just mention, and that is with reference to the bonding. I notice in the illustration given in the paper (Fig. 10, Chicago Bond) that the ends of the bonds are turned over the pin. I do not think that is the usual way of applying the bonds; the ends, as far as my experience goes, are always turned the other way, on to the web of the rail. Mr. Gadsby.

Mr. C. E. Grove: I wish to confine my remarks to one point of the paper, and it is that which deals with the advisability of Mr. Grove.

Mr. Grove. employing the three-wire system for cases which are beyond the range of applicability of the ordinary two-wire. The author's contention, as I understand the question, is that if you wish to employ a three-wire system you are practically prevented from doing so by the difficulty of balancing the current in the two sides; and the best thing to do in those circumstances is to employ an alternate-current system of generation, transmit the power by alternating currents to feeding or distributing points on the line, and there convert it through statical transformers and rotatory transformers into continuous currents for working the railway. I think that was the proposition, and I think Mr. Rice spoke on that point. Now I wish to combat that proposition as a *general* proposition, because I think it would be very dangerous to lay that down as a canon in electric traction work. The subject is of particular importance just now, for this reason—that there are before the public schemes for the construction and working of quite a large number of electric underground railways in London. It has been my duty for the last year or 15 months to have worked on designs in connection with that subject, so that I happen to have in my possession a number of facts and figures which have led me to an entirely opposite conclusion to that of the author of the paper; and, as the consideration of a particular case will often carry conviction where general arguments may fail, I ask leave to deal with that general proposition from this particular point of view. There have just been submitted designs for the working of the Central London Railway by electricity, the Central London being the largest railway of the kind that has yet been proposed; and it is significant that the two systems in question—*i.e.*, three-wire *v.* alternating—have been in competition for the carrying out of the work. The engineers to that railway did not themselves choose a system, but simply laid down the conditions that the trains, which weighed so much, were to be pulled along the railway at so many miles an hour, and a 2½-minute service was to be maintained. For the rest the contractor was free to propose what he liked. The Thames Ironworks Company, and at least one other large English company, went in on the three-wire system; the Americans went in, I believe, on the

alternating system. Now what are the conditions? The railway consists of two lines of tunnel, one up and one down, each about seven miles long. The generating station is at one end. It seems to be exactly a case in which the three-wire system is the thing to use. The up line can be used as a positive side, the down line as a negative side; the electricity can be generated at double the ordinary Board of Trade voltage, while at the same time one never has more than the Board of Trade voltage of 500 volts or so in one tunnel. The line is fed by currents which at that potential can be safely transmitted on bare copper feeders, and the feeding points can be kept at approximately the same potential. We have here all the elements of a secure, stable, well-understood, and economical system. The potential is nowhere dangerous to life, and there is little likelihood of leakage, because the rails are simply the neutral conductor, earthed all the way, and carrying, not the maximum current, but only the balancing current between one side and the other—and that not for the whole length, but only for short sections, as between trains in one tunnel and trains in the other tunnel. With regard to the carrying of the balancing current, it is rather difficult at first sight to understand why there should be any considerable difficulty in maintaining a balance. The dynamos are, almost obviously, arranged to work in pairs at 500 or 600 volts apiece, because for the large generators such as would have to be used—dealing with thousands of kilowatts in total—500 or 600 volts is a very convenient voltage for a multipolar bar armature, having regard to safety. In that case, a very simple arrangement of switches at the central station enables you to throw any number of machines on one side, to deal with, what may perhaps be called, fairly constant inequalities of load between the two sides, such as might be occasioned, for example, by a block or breakdown stopping the service in one direction; but, as regards momentary and lesser fluctuations, those can be dealt with conveniently by means of balancing transformers placed across the feeders at certain feeding points. We chose half a dozen, which brought us to each alternate station; but we might, if we liked—and there is no reason why we should not, if we found six insufficient—choose Mr. Grove.

Mr. Grove. a dozen, and put one at every station. Now, by providing a capacity in those balancing transformers of about one-fifth or one-sixth part of the whole output of the station, it seems to me that we provide a very large margin for the shifting of the current from one side to the other, due to irregularities in train weights or speeds, or to supply extra currents in starting the trains—an arrangement facilitated by the use of fly-wheels and other obvious things. There is here all that is necessary for maintaining a very fair balance on the system. And be it remembered that if this system be not adopted, and the railway be supplied by continuous currents which have been first generated as alternate currents, you have, in the apparatus which transforms them, to deal, not merely with a certain fractional part of the station output, but with the whole output. Further, the whole output of the station has to pass through statical transformers to reduce the pressure to the equivalent of the line pressure; because the alternate currents would be generated, I take it, at a couple of thousand volts or more, and the alternate currents at low pressure have then to pass through rotatory transformers or motor transformers. Mr. Rice said the ordinary motor transformers are no good—that you must have rotatory ones; that is to say, you must put the alternating currents in one end of an armature, and take out the continuous currents at the other end. It is rather interesting, by the way, to mark that Professor Mengarini, who has been doing that at Rome, thinks that it is quite impracticable for machines of large size, owing to the sparking which takes place at the continuous-current side, due to the periodic alternations between dynamo and motor working in each alternation of the current.

Now as to the commercial side of the question. In this case we have to deal with a total capacity of 5,500 kilowatts, or something like that, and the question of substituting alternators for continuous-current machines means putting more expensive machines in the central station—perhaps £1 or 30s. a kilowatt more, or, taking a mean figure, an increased expenditure of some £7,000. Then we must have statical transformers capable of dealing with the same output. Of course there is a certain

amount of reserve plant in the station, and it might be urged Mr. Or that we have only to deal with the transformers and apparatus at the feeding points with an output equal to that portion of the station plant which is actually running. But I think if that be examined it will be found delusive. There must be a margin of power there, and it cannot be in the form of some machines idle and some working. To provide against breakdowns and fluctuations, transformers must be employed of larger capacity than are really capable of dealing with the load, and they will be worked on the average at, say, three-quarters of their full load, and therefore below their full efficiency. For traction work also a low periodicity would preferably be employed, again losing a little bit in efficiency. Then there are 5,500 kilowatts in rotatory transformers to be provided, and adding the cost of this transforming machinery to the extra cost of the dynamos there is, at ordinary market rates, which can be got from anybody's price-list—I am not speaking of contract prices, or anything of that sort—about £45,000 of machinery employed over and above what is necessary for the continuous-current work.

Now as to efficiencies. As I have said, in these transformers—which have got to work at something like three-quarters load, and a fluctuating load, and low periodicity—you cannot reckon on getting more than 95 or 96 per cent. as an average efficiency. In the rotating machines you may get 96 per cent. or a little more; but in the two together you have to reckon on a loss of 7½ per cent. or 8 per cent. of the output of your station, in getting the energy into the continuous-current form on the rails. This is really a very serious amount, because it can be easily calculated that the coal bill alone for working that railway, according to what they propose to do, will be something like £25,000 or £30,000 a year; and the total works cost of generating the energy, apart from the question of rents, directors' fees, and establishment charges, will be something like £60,000 a year, more or less. Eight per cent. of that is, say, £5,000; and £,5000, even assuming that the money to the promoters of the company is worth 4 per cent., means the interest on £125,000—a very considerable sum of money. When you add that to the additional outlay on machinery, it appears

Mr. Grove. that there is a very big balance of disadvantage against the alternating system for work of that kind. It does not matter, for the sake of my argument, whether the one scheme on the whole, as submitted, was cheaper or dearer than any other scheme on the whole, as submitted to this particular company. I am dealing simply with general market figures. No matter from whose pockets the money comes, if you spend £160,000 or £170,000 more on one set of plant than on another that would do the work quite as well, that money is wasted, lost to the world, and its expenditure cannot be justified.

The question of cables may be left out of account. The copper for the continuous-current system would be large, but bare; for the alternating system it would be smaller, but insulated; and the cost of insulation would pretty certainly run away with any gain that would be effected by reduction of section. I think, therefore, that the general contention of the author on the point raised is untenable, and that the arguments drawn from American tramway practice do not apply to such a case as that I have sketched.

Mr. Parshall. Mr. H. F. Parshall: Inasmuch as the discussion has now proceeded broadly outside of the limits of Mr. Baylor's paper, and the last speaker has made a specific example of the Central London Railway in advocating the advantages of a three-wire system for electric traction, I may be justified in showing that the three-wire system, according to experience, is inapplicable to such a traction system, and that the three-wire system has certain inherent defects that have not been taken into consideration by its advocates.

The three-wire system as applicable to a large system for electric traction to-day exists in theory only. It has been given repeated trials, and it has repeatedly failed, and it has been replaced by a multiphase system of distribution with substations in which there are large rotary converters. I am speaking of the traction system at Portland, Oregon.

During my connection with the Edison General Electric Company I had an opportunity of studying the conditions and advantages of three-wire systems for lighting work. It was

found, even in lighting work, that the great advantages that have been predicted for the three-wire system could not be realised, and that the loss in the neutral wire was considerable, and that to get the best advantage the neutral wire must necessarily be much larger than was originally contemplated. There are broad differences between a three-wire lighting system and a three-wire traction system. If in the three-wire lighting system the voltage on one side falls below the normal, it is to some extent compensated for, since the resistance of the lamps on that side increases, and the current consumption is cut down automatically. In a traction system the opposite is true. When the voltage falls on one side of the system through overloading, the motor man turns his controller handle to a further position, so as to maintain a given speed schedule; that is, to maintain a constant work at the car, means consuming an excessive current. In other words, the effect of unbalancing in a lighting circuit is differential, in that it is compensated for by the nature of the load; whereas in a traction system it is cumulative, since the work is maintained constant at the expense of current-consumption. Mr. Parshall

At St. Louis a three-wire traction system was tried in a net work—conditions much more favourable to balancing than any rectilinear system such as that mentioned by the preceding speaker. After a thorough trial it was abandoned, since it was found that the extra cost and trouble in compensating from hour to hour was of vastly more consequence than the extra cost of the copper. This occurred in a system where there were several hundred cars, each of which consumed, as a maximum, but a small fraction of the total output of the station—conditions comparable to some extent to a lighting system; the difference being, as pointed out above, that unbalancing in the traction system is necessarily cumulative. Consider such a case as the Central London Railway, instanced by the preceding speaker. There are to be operated between 20 and 30 trains. Each of these trains weighs some 130 tons, and to make an average running schedule of 15 miles an hour the trains at times must attain a speed of some 30 miles an hour and must be accelerated at least

Mr. Parshall. one foot per second. Under the most favourable conditions of control—that is, with the series-parallel controller—these trains will absorb from the system at times 1,000 amperes. I am basing this figure on actual tests made, since I have before me the results obtained in actual practice, comparable with that of the Central London Railway. The average output per line on a three-wire system such as the Central London for the above service would not exceed 3,000 amperes. You have, then, a three-wire system in which one unit, or train, will absorb one-third of the normal current per line—a condition impracticable to compensate for in a lighting system, and a condition which, in practice, introduces the use of compensating machines of such size and character as immediately determines that the three-wire system cannot, in such a service, compete, either economically or practically, with the three-phase and rotary converter system.

Assuming that the rails are to be used as the neutral conductor, the number of sub-stations may be determined from the Board of Trade Regulations. The resistance of four rails bonded together amounts to about 0·13 ohm per mile. Consequently, 500 amperes unbalanced in the neutral would give 6½ volts drop per mile. The Board of Trade Regulations specify that the maximum drop in the rails, when used as any part of the earth return, cannot exceed 10 volts. From the above it follows that these sub-stations, or compensating stations, should not be more than 2½ miles apart, assuming that there may be one train unbalanced on such a section, this train working at its full capacity. From my own experience in electric traction work, I should not be satisfied with any such meagre allowance in compensating arrangements, since under the most favourable conditions, with four trains opposite on one side of the system, it is reasonable to expect that two of these trains may be absorbing maximum power while the other two trains are running light, in which case the amount of unbalancing would be double that which I am assuming for the sake of making a fair comparison. Making allowance for the reversibility of the machines—that is, that each machine is to act from time to time as a motor or

generator to compensate for the capacity of one train—these compensating machines would have to have a capacity of at least 300 kilowatts. Taking three sub-stations as the least number consistent with the Board of Trade Regulations, and compensating for the section near the generating station through the neutral conductor, the loss in these sub-stations becomes considerable. The loss in the neutral conductor seems to have been lost sight of by the three-wire advocates. Mr. Parshall

Taking the case of the system at Portland, Oregon—where the three-wire system has been replaced by the three-phase—and also the case at St. Louis, it was found that the neutral conductor had to be quite as large in the cross section as the outside conductors, and that in this condition the loss in the neutral conductor and in the compensators (about which I shall have more to say presently) is, under favourable conditions, about equal to the loss in transformation in a well-designed multiphase converting station. Where, then, can be the economy in the three-wire system, since in the three-phase system the energy may be transmitted at 5,000 volts, where in the three-wire system it is transmitted at 1,000 volts, or for the same transmission efficiency 1-25th of the weight of copper will do for the three-phase system as for the three-wire system? The duty of the compensating machines in a three-wire system does not seem to have been considered.

If two trains are unbalanced on the opposite side of one of these stations, it has to be relied upon to compensate for this unbalancing by itself. A station 1½ or 2 miles away with a track resistance does not lessen the load liable to come from time to time upon one of these sub-stations; that is, each sub-station must have a capacity sufficient to compensate for all the unbalancing that may occur locally. When half-way between the stations the unbalancing may be compensated for equally by the two sub-stations. This increases the size of the machines in the sub-stations, and likewise greatly increases the deterioration of the commutator.

Another feature of these sub-stations in a three-wire system is that, when the load suddenly drops, the voltage at these

Mr. Parshall.

sub-stations suddenly increases; so that they must, in order to accelerate themselves, to correspond to the increased voltage, absorb from the system a portion of the load that had been on the system—a condition most unfavourable for the operation of commutating machines. All of these difficulties and losses seem never to have been taken into consideration by the advocates of the three-wire system, and I have never yet met an expert of broad experience in traction work who has confidence in such three-wire systems as have been proposed; and I have never yet met an expert in traction work who has had experience in using a three-wire system in practice, who considers that such a system is either economical or practicable to operate. The preceding speaker said that Professor Mengarini had shown that rotary converters beyond 50 kilowatts capacity would give trouble from sparking. I believe I am familiar with the paper in which this was stated. This statement was entirely confined to the single-phase rotary converters. Multiphase rotary converters have entirely different properties. If Professor Mengarini has stated that three-phase rotary converters will not run well beyond 50 kilowatts capacity, then I should ask for an explanation why the 600-kilowatt machines at Niagara, that are generating constantly 3,750 amperes for 365 days in the year, have developed that beautiful blue, glossy commutator which has so recently been the object of admiration before this Institution. A broad difference between a multiphase rotary converter and the single-phase rotary converter is that in the single-phase rotary converter the machine has to deliver from its commutator a constant flow of energy in the consuming circuit, whereas from the alternating-current circuit it receives an intermittent flow of energy—a condition most unfavourable to good commutation. In the three-phase rotary converter there is a constant flow of energy into the machine from the alternating-current side at constant load, so that the machine is working under conditions favourable to good commutation. As I have already pointed out, in the three-phase machines the reaction between the three-phase and continuous-current functions of the machine is such that current-losses, and the effect of the current in the armature upon the field poles to

produce distortion, are largely ameliorated—so largely that a 500-kilowatt rotary converter will carry 1,000 or 1,500 kilowatts without the slightest sparking at the brushes. Mr. Parshall.

In a recent experiment it was found that a rotary converter of 600 kilowatts would maintain itself at synchronous speed, and commutate perfectly up to 900 kilowatts without excitation—an experiment that sufficiently demonstrates the efficiency and practicability of a properly designed rotary converter. The efficiency of a well-designed rotary converter of 600 kilowatts capacity may be taken to be, from the tests made at Niagara, 96 per cent. at full load, and 94 per cent. at half load; of a 200-kilowatt static transformer, 98 per cent. at full load, and 96·5 per cent. at half load. It becomes obvious, therefore, that with wide ranges of load the losses in transformation in a multiphase system are small, and are not greater than those in compensating stations and neutral conductors in a three-wire system of distribution.

There is one point more that I should like to present with respect to three-wire systems, or, in fact, in any system for electric traction where the voltage in the consuming circuit is allowed to vary between wide limits—that is, in a system where the average transmission efficiency is taken as 90. If the loss is entirely in the consuming circuit, or in the consuming circuits as supplemented by feeders, the motors on the locomotive are working at one end of the line at 500 volts, and at the other end at 600 volts. To maintain a constant speed schedule, which is the usual condition in electric traction, and with the other conditions remaining the same, the average loss in the resistance becomes excessive. In a case like that under consideration, I have worked out what the loss would be with most efficient motors and with a series-parallel controller. At the least estimate, the efficiency of the locomotives working on such a widely fluctuating voltage will be some 10 per cent. lower than that of the locomotive working on a system in which there is a constant voltage maintained in the consuming circuit. By 10 per cent. I mean taking the locomotive at the highest efficiency—that is, the locomotive, working under favourable conditions, as 100. This locomotive working on a variable E.M.F. would not have an

Mr. Parshall. efficiency exceeding 90. In the operation of the Dublin Southern Traction Work it has been found that the motors showed an increased efficiency owing to the constancy of the voltage in the line; so that the net result in watts output per car mile has been satisfactory, efficiency considered. There are other features, however, that require enlarging upon, and I am now engaged in writing a paper broadly discussing the advantages of this system.

I would point out that the three-wire system for traction work is purely in an experimental condition; that it has been tried in practice and proved a failure; that it necessitates the use of commutating machines and sub-stations, the loss in which, and in the neutral conductor, is comparable in magnitude with that in the sub-stations in the multiphase system; that the advantages of high voltage transmission are lost; that it introduces additional losses in the locomotive, owing to the fluctuation of voltage in the consuming circuit. (I am aware that this variation of voltage may be compensated for by feeders and boosters. Such an arrangement, however, is at the expense of efficiency, and I will not complicate my discussion by taking it into consideration.)

In conclusion, I would state that, in considering the efficiency of an electric traction system, the loss in transmission may not be in itself considered as final; that the efficiency of the system in its entirety as affecting the efficiency of the locomotive must be considered; and that those who are advocating the use of a three-wire system for electric traction are ignoring many of the factors that are of first importance in determining the ultimate economy of a complete traction system.

Mr. Preece. Mr. W. H. PREECE: May I ask Mr. Parshall one question? With reference to the three-wire system, I have read somewhere —I forget where—that on one of the lines in the United States they have applied the three-wire system in such a way that each coach becomes its own equaliser. There are two trolleys, one picking up the positive and the other the negative current, the wheels being neutral or to earth. Can you give us any information about that modification?

Mr. Parshall. Mr. PARSHALL: That system has been rather carefully

considered; and, although in the United States the circuit-breaker has been brought to a high state of perfection, we are quite unable to provide for the number of short-circuits which are liable to occur on such a system. Mr. Parshall.

Mr. W. GEIPEL: We are all indebted to Mr. Baylor for bringing before us this subject at so opportune a time. We are undoubtedly on the verge of a great development of electric traction in this country, and too much care and attention cannot be devoted to the experience gained in other countries, that we may benefit thereby. At the same time, I cannot help feeling, with other speakers, that there is much in the paper which has been oft told in this country, and it is my belief that Mr. Baylor has the ability to give us considerably more information on other points of this subject which have not hitherto been treated. Mr. Geipel.

I have been more especially interested in what Mr. Baylor says *re* the welding of the rails together; but I observe that no mention is made of the cost of this welding, either by the cast welded method or by electric welding.

There can be no doubt that for good conductivity the electrical method is far away better than the casting method; in fact, it is doubtful if in the case of the latter method it would be safe to do without other bonding to assist.

Mr. Baylor refers to the mechanical difficulty of getting electrically welded joints to stand; but with the enormous strain set up by temperature differences in long lengths, some part of a long continuous rail is bound to give way.

In 1,000 yards there is a difference on length of 2 feet for a temperature difference of 100° Fahr., which is not, surely, an outside limit of temperature variation on the surface of the ground.

Now, if the continuous lengths be made shorter by introducing a small percentage of fish-plate and bonded joints, this difficulty might be overcome.

The number of loose rail joints which strain the car equipment would then be considerably reduced.

But the difficulty which occurs to the contractor is the expense of providing the necessary electricity for the welding

Mr Geipel. during the time of installation, for at this time power will not generally be available at the central station.

If Mr. Baylor would tell us how they get over this difficulty in America, and what they find the cost to be of this electric welding per joint, it would be of much interest.

I am not able to agree with Mr. Baylor that the series-parallel method of control is used exclusively all over; it may be, however, that he refers to American practice.

There is no doubt that with high-speed heavy cars this system is necessary, but for small light cars running at a low speed it is quite a matter for consideration whether one motor is not sufficient. Firstly, there is, owing to the reduced range of speed, less possibility of effecting a saving with the series-parallel control; and, secondly, the first cost and the cost of maintenance is considerably less with one motor. At Hamburg, for example, where they have some 300 cars, they have adopted the single motor for as many as 250 cars, while they use the two-motor series-parallel control on 50 cars only. The power consumed at Hamburg is under 800 watt-hours per car mile.

The most interesting point in Mr. Baylor's paper is his description of the electric brake. The curves showing the braking effect of the eddy-currents are most instructive. The author speaks of difficulty with residual magnetism; but there would appear to be various well-known and obvious ways of overcoming this, perhaps with less complications than seem to have been adopted.

The description of the switch-boards is useful, although the panel system has been adopted for years past in this country; but if Mr. Baylor, instead of illustrating his paper by means of pretty pictures, would make use of diagrams of connections, I am sure we should all find it still more useful. And, by the way, the same remark applies to the dynamo, of which there are given four outside views, but not one section.

I am afraid I cannot accept what Mr. Baylor says as to the laxity of specifications of dynamos—in this country, at any rate—for it is my misfortune, or otherwise, to have to wade through numbers of long-winded specifications almost daily; and I can

assure Mr. Baylor that our consulting engineers cover the points to which he refers, and a good many more. One of the latest pacings which machines have been specified to stand is that the armature shall be dragged round by the scruff of its neck whilst the magnet is fully excited, and that under these most uncomfortable circumstances it must not grunt. Mr. Geipel.

I think Mr. Baylor may rest assured that our consulting engineers require no pricking by the contractors to induce them to specify fully what the dynamo shall, or shall not, do.

Mr. E. K. SCOTT: I should like to say a few words with regard to some of the apparatus. The series-parallel controller seems to me a very neat piece of design; but it would appear that the electric blow-out is rather inefficient, owing to the air gap being so long. The magnetic lines have to pass from the top side of the iron case down to the lower side, right across the contacts—a distance of some 4 or 5 inches—and a good deal of wire must therefore be required in order to get a good magnetic field in such a long air gap. On the Hartlepool trams they have an arrangement whereby the switch-arm itself forms part of the magnetic circuit, and in that way the blow-out has a much shorter air gap. Mr. Scott.

Anyone looking at the large motor exhibited cannot but be struck with the enormous weight which has to be put on to an ordinary two-horse tram-car. There are two such motors on the ordinary tram-cars, each giving about 15 H.P. Surely there must be something wrong when we have to put 30 H P. of motors on to a car which is usually drawn by two or three horses. It seems to me that some combined method of cable and electric traction might be found useful. There is no doubt that the cable traction on the Snow Hill line at Birmingham could not be tackled electrically with anything like the results, both financially and otherwise, which have been attained by the cable. To see those cable cars whizzing round the Snow Hill corner is quite enough to confirm one in that opinion. Could not, therefore, some combined system be arranged, with cable for the steepest hills, and overhead trolley wires elsewhere? In this way also the prejudice to overhead wires in the main streets of the large towns

Mr. Scott. could be met. Take Leeds, for instance. Leeds lies in a hollow, with stiff inclines on nearly all the suburban roads, more especially in the direction of Beeston, where there is a very steep hill about half a mile long. Would it not be possible to work those hills by short cables driven by electric motors in chambers below the level of the road, the motors to be weighted and placed on inclines, so as to automatically keep the cables taut? In the main streets of Leeds people have objected very strongly to the overhead trolley wires, but apparently they will have to put up with them shortly; yet here again is a place, surely, where one could efficiently combine electricity and the cable system. Boar Lane, Briggate, and the other busy streets could be worked with electrically driven cables; Briggate especially, because it is hilly, could have a short cable worked from the North Street end,—Roundhay, Chapletown, Kirkstall, and the other suburban roads being worked with the overhead wires. I particularly mention Leeds because I happen to know the conditions; but I think my remarks would apply to Glasgow and to many other places equally well. It has been suggested that the Birmingham tramways might be worked throughout on the electric cable system; every straight street of any length having a length of cable, with the motor at one or other end; but this, it seems to me, is going a little bit too far, although I believe it would work out rather well for a very hilly place like Halifax, for example. One thing is certain: when we put two 15-H.P. motors on a tram-car, they will only be doing their full work on a very small portion of the total distance; all the rest of the way the motors must work at low efficiency.

With regard to the design of the motors, I have had occasion to go into the matter from the weight point of view, and find, if you make the outside parts—or the "dead parts," if I may so call them—of the carcass of cast steel, that the weight is very considerable. The parts at the ends carrying the bearings are supposed to act as a return circuit for the magnetic lines; but I am inclined to think that the value of this comparatively thin metal—say $\frac{3}{8}$ or $\frac{1}{4}$ inch thick—is not very great in helping the magnetic circuit, on account of the metal being chilled in casting.

An aluminium alloy which is quite mechanically strong enough for the support of the two bearings, &c., is on the market, the use of which will considerably decrease the weight of the motor as a whole. The cost of the alloy per pound weight is, of course, greater, but it is about a third of the weight. On all self-propelled vehicles, of whatever kind, weight is a serious consideration, and the use of aluminium in the way indicated would go far toward enabling us to lighten the motor part of the equipment. Mr. Scott.

Mr. RAWORTH: Mr. Scott will be very pleased to hear that his ingenious notion of a combined cable and electric tramway has been worked out by Mr. Christopher Anderson, of Leeds, who has a patent for the same. It may save Mr. Baylor some time in replying if I mention this fact. Mr. Raworth

Mr. ALBION T. SNELL [*communicated*]: Among the many important issues raised in Mr. Baylor's interesting paper, none appears to me to be of more moment than his condemnation of a three-wire system for railway work. Judging from the paper, and from the speeches of several experienced American engineers, it would appear that the three-wire system had been tried and found wanting. In my opinion, the evidence adduced is not sufficient to warrant a complete condemnation without further investigation. Mr. Snell.

I gather that the chief difficulty lies in the balancing of the load between the two sides, and that continuous-current transformers have proved ineffective as compensators. I also believe that the three-wire system has been tried for street tramway work, but *not* for railway work, such as the South London Railway or the Central London Railway.

In connection with the American trials, it is important to ascertain the arrangement of the station plant—whether the main generators were coupled across the "outers," and the middle wire was connected only to a *small* pair of balancing machines? or whether the generators of, say, 550 volts each, were coupled in series with each other and the middle wire to the point of their interconnection, so that a large proportion of main plant was available for balancing the load? Very different results might be anticipated from the two arrangements.

If the balancing generators in the power station be large

Mr. Snell. enough, there should be no difficulty in maintaining a balance; for the two sides are practically independent, and could be worked separately if necessary. The "outers" could be fed in the usual way at suitable intervals; and continuous-current transformers with two armatures, one across each side of the system, although not absolutely necessary for balancing, would tend to decrease the balancing current demanded from the station.

From a practical point of view, it appears to me that the three-wire system, suitably arranged, is applicable for power as well as lighting work. That a two-wire system with rotary transformers at feeding points may cost less to instal in some cases is possible, although I doubt it, but at the present moment I fail to see that it offers any practical advantage over the three-wire system. I assume that the rolling metals are properly bonded in each case, and that the distance from the power station to the end of the line does not exceed, say, 8 miles.

If Mr. Baylor will kindly give some further information on these points, he will increase the usefulness of his interesting paper.

Mr. Edmunds. Mr. H. EDMUNDS [*communicated*]: With regard to Mr. Baylor's paper on "Recent Developments in Electric Traction "Appliances," I regret that business engagements prevented my being present when the paper was read. I have, however, carefully gone through the various points brought before the notice of the Institution, and think we must thank Mr. Baylor for having condensed and brought up to date a very full and lucid account of the most recent practice in connection with electric traction. It has been objected by some members that the paper contains nothing new; but we are not all equally gifted with my friend Mr. Raworth, and Mr. Holroyd Smith, in knowing something of everything that is taking place; and there are certainly many members who, like myself, are not connected directly with this subject, and who feel pleased when a man of Mr. Baylor's experience is good enough to lay before us, in such a clear and concise form, all the various important points that ought to be considered in this matter.

There has been an objection that the paper savours too much

of American practice, and too little reference is made to English and Continental practice; but, when we consider the rapid strides and enormous developments which have been made during the last few years in America in connection with electric traction, we must admit, perhaps regretfully, that we must look to the United States for a much wider and broader experience than we can possibly obtain here ourselves. Mr. Edmunds.

I was, unfortunately, unable either to hear the paper read, or to be present during the whole of the discussion, and therefore have not had an opportunity of hearing all the various points raised by others. I hope, therefore, if I should allude to anything that has been already discussed, I shall be pardoned on that account.

Among other things, Mr. Baylor discusses the merits of the three-wire system, and, I think, shows very fully many of its disadvantages; and I think his suggestion with regard to the use of motor generators, operated from the centre through the multiphase current transmission, is one of great importance, and one which lends itself probably better than any other proposal for taking advantage of high-pressure currents for transmission, and lower pressure direct current for operating the motors on the cars in their several sections. Some few years ago I attended the demonstration at Frankfort, where power was transmitted from Lauffen; and I brought over to this country the first set of drehstrom apparatus, which was supplied by the Allgemeine Company of Berlin. This was seen by a great many people at my office in Westminster—among others, by the late Mr. Greathead, who was very much struck and charmed with the idea of being able to combine the advantages of high-tension currents in connection with stationary transformers working multiphase motors generating direct currents; and he told me he believed that, when the work in this country was expanded, this would probably be the best method of working. An opinion from a man like Mr. Greathead, at that time, I consider to have been a very valuable one; and the more recent practice described by Mr. Baylor shows that his feeling was right.

Mr. Baylor also opens up the important question of electric

Mr. Edmunds. brakes, and, I think, shows conclusively that the electric brake is the only possible one to depend upon in connection with electric traction. It would be useless for me to attempt to show better than he has done the reasons for this.

In conclusion, I would like to say, as a member of this Institution, that we all appreciate and thank Mr. Baylor, or any other of his American friends, for giving us so full and lucid a description of what their practice is. Of course, in common with other English manufacturers, one regrets to see so much money going to the United States for the purchase of material which we feel ought to be just as well manufactured in this country; but until the manufacturers of this country show that they can produce equal or better results at the same price, they must not be aggrieved if the English investor purchases in the best market; and I think the time of the members of this Institution has been well spent in learning what their practice is, and I trust it will spur us on to "go one better" on this side. There has been, unfortunately, too much of the English self-satisfaction displayed in connection with matters of electrical engineering. Many have not taken the trouble to know or care what is going on abroad, and imagine that because a thing is done in England it is necessarily done in the best manner; and I have no doubt that, if we can show Mr. Baylor, or his English company over here, equally good results, with the same margin of profit to themselves, they will feel it politic to buy in this market rather than abroad.

Mr. Brown. Mr. C. E L. Brown (Baden) [*communicated*]: In his paper Mr. Baylor makes some remarks with regard to three-phase traction, which seem to indicate that he is not quite fully informed as to what has been done in this respect over here.

First, as to the system not having made much headway. Since the pioneer line at Lugano (which, by the way, is in Switzerland, not in Italy), my firm has entered into contracts for the complete electric equipment of three other railways, besides securing the order for the locomotives to be used on the Jungfrau Railway—all on the three-phase system. Seeing that the Lugano line has not been at work quite a year, I think it can hardly be said that the system has made no headway.

Secondly, Mr. Baylor states that the torque of a three-phase motor is limited, and cannot be pushed as in the case of continuous-current motors. With regard to this, I would point out that the torque of any motor, whether for continuous-current or one-, two-, or three-phase alternate current, depends on the current passing through its armature, and on the strength of its field. Since alternate-current motors work with a normal field strength of about half the value usual in continuous-current motors, it is evident that there is much more room to push the motor. Even without raising the voltage, and consequently the field strength, a multiphase motor will easily give double its normal power; and, further, the field may easily be raised without the use of any transformer, by the simple device of changing the windings from star to mesh connections. In fact, in each case, the limit may be said to be the load which will burn out the windings; and, owing to the better distribution of the material in the three-phase motor, it is at a decided advantage in this respect. For the same reason I beg to differ from Mr. Baylor's remark that a multiphase motor must be designed for its maximum power, instead of for its average output as with continuous-current motors. Mr. Brown.

Thirdly, as regards weight for equal outputs, I must inform Mr. Baylor that three-phase motors are lighter for the same power than continuous-current motors at the same speed. This is chiefly due to the more symmetrical arrangement of the material, and to the fact that the motors can conveniently be built with more poles than in the case of continuous-current motors,

Finally, with regard to Mr. Baylor's remark that the three-phase system will only be employed in certain exceptional cases, I would point out that the plants now executed or in hand include almost every leading type of traction. The ordinary town tramway is represented by the Lugano plant. The Gorner-Grat line and the Jungfrau Railway are examples of steep rack railways on mountains. The line from Stansstaad to Engelberg, with a length of 18 miles, represents the average light railway, the gradients being for the most part such as to permit of working

Mr. Brown. by adhesion. A section of about $1\frac{1}{2}$ miles has a gradient of 25 per cent., and is fitted with a rack, a special electric locomotive being attached for this. And, lastly, the line from Burgdorf to Thun, for the electrical equipment of which my firm has just secured the contract, is an example of what we may expect to see commonly adopted in the near future—an ordinary normal-gauge railway worked electrically by high-speed motor and trailler cars, the power being derived from a distant waterfall, and transmitted by high-tension three-phase currents, to be transformed to medium tension for the motors.

Mr. Baylor. Mr. A. K. Baylor, in reply, said: I think nearly every point that has been raised, either in my paper or in the discussion, has been pretty well dealt with during the evening by some one speaker or another, so that, on my part, there is very little to be said.

With regard to the remarks of Mr. Jones on the welding question, I would say that the present method of welding was first suggested by Professor Elihu Thomson, the inventor, and was departed from, only to be returned to again later on.

I agree fully with Mr. Sellon that the question of bonding is of the utmost importance, and cannot be too carefully considered; but I feel sure that public interests are amply safeguarded by the present regulations of the Board of Trade limiting the drop in the ground return. If any inferior method of bonding, such as that described by Mr. Sellon, is put in use, the contractor or operators of the line will be obliged to renew it as often as the track resistance exceeds the prescribed limit, which, with bonding of this description, would probably be every few months. It seems to me, therefore, that this question is serious chiefly to contractors and others responsible for the working of electric lines. These requirements play an important part in the design of plant, as they determine whether direct transmission shall be used or the plant be subdivided.

This question of subdivision, and my remarks on the three-phase and three-wire systems, seem to have brought about a locking of horns between the advocates of both systems, particularly as applied to one particular line which is rather prominent just now.

In their various arguments Mr. Grove and Mr. Parshall have Mr. Baylor. pretty well covered the ground *pro* and *con*. As to the technical difference between three-phase and three-wire systems, I can add nothing to Mr. Parshall's remarks as supporting the statements embodied in my paper; but I think Mr. Grove is in error on certain points not specifically taken up in the discussion. He claims that for the particular line used as illustration the expenditure for machinery would be some £45,000 greater with three-phase than with three-wire distribution. The relative expense for cables is left out of account, as he has taken it as approximately the same in both systems. I cannot understand how Mr. Grove arrives at his values for electrical machinery and feeders. From the best information at my command, based on innumerable sales of machinery of the sort under discussion, the excess of cost is in favour of the three-phase system by some three or four times the amount—£45,000—given by Mr. Grove. This is assuming the same general conditions in both cases, with equally constant potential on the consuming circuit.

Mr. Grove shows a formidable array of figures representing the loss in coal due to the lower efficiency of the three-phase system; but experience has shown that the relative efficiencies that he has estimated for the two systems are not correct, and that the difference, taking the locomotives into consideration, is in favour of the three-phase system.

The questions raised in Mr. Snell's communication have been already well covered in the discussion. The chief difficulty certainly lies, as Mr. Snell has assumed, in the matter of balancing, and it is this that must always stand in the way of free use of the three-wire system for traction purposes; and wherever this question arises, it must be settled upon the exact conditions to be met with for the given service.

I would say, for Mr. Snell's information, that the practice followed in America in trials of the three-wire system has been to connect the ordinary 500- to 550-volt generators in pairs of two in series so that the neutral wire was connected between the two generators. One reason for following this plan was that it allowed the use of the existing plant practically without change, excepting

Mr. Baylor. in the switch-board, although such an arrangement is to be preferred in any event.

Certainly in theory there would appear to be much to recommend the use of the three-wire system on lines exceeding the limits of 500 volts transmission; but in practice, wherever it has been applied to traction work, it has been found inapplicable, and has been discarded after actual trial. That is certainly strong ground to stand on in condemning the system for traction work generally.

Mr. Gadsby points out, in the course of his remarks, that I have omitted to mention in the paper one great objection to electric brakes. He says that, in using motors as generators to drive current through the coils of an electric brake, you are at the same time dissipating heat (that in the ordinary brakes is dissipated on the brake blocks and tyres) in $C^2 R$ losses in the armature. Mr. Gadsby forgets that the great efficiency of the electric brake is derived from the fact that the stored energy of the moving car is made to destroy itself, so to speak. The internal resistance of the armatures, therefore, in this case is not a loss, but a part of the resistance brought into play to bring the car to rest. It is only necessary in practice to prevent the flow of current in the armatures exceeding the safe capacity of the conductors. The means of accomplishing this I have described.

In answer to Mr. Geipel's inquiry as to the cost of electric welding and cast jointing of rails, I can say that the figures which have been given in special instances (which I think represent very closely the average charge), compare almost exactly with the cost of fish-plate joints. The prices I have in mind apply to 9-inch girder rails, upon which at least six-bolt fish-plates would have been used if the rails were not welded. Against the cost of welding there is, of course, credit for fish-plates, bolts, and the labour of drilling and fitting.

It is usual, where electric welding has to be done, to get enough trolley wire erected and sufficient power at the station to deliver current for the welder at 500 volts from the trolley wire.

Of the electric welding that has so far been done, much the greater portion has been on existing lines which were already

fully equipped electrically and open for service. When trolley current is not available, however, it is necessary to use an independent plant—*i.e.*, a portable engine and dynamo—carried along with the electric welding machine. Mr. Baylor.

With regard to Mr. Geipel's remarks on the use of series-parallel controllers, I wish to say that the possible saving to be effected by series-paralleling depends primarily on the frequency of starting and stopping. Possibly this is what Mr. Geipel means by the expression "range of speed." My reference to the use of series-parallel control naturally did not refer to single-motor equipments.

On the matter of dynamo specifications Mr. Geipel has fallen into the error of an earlier speaker. I certainly cannot complain of absence of dynamo specification, although I have not yet been met, as Mr. Geipel has, by any requirements for machines that can be "dragged around by the scruff of their necks," whatever that may mean. My reference to specifications was in the line of hoping that the time would come when some universal standard would be adopted, so that every individual consulting engineer would not establish standards of his own, such as requiring that the dynamo should not "grunt," as Mr. Geipel has put it.

From his communication, I think Mr. C. E. L. Brown has not understood me clearly in reference to the use of alternating-current motors. I did not make the statement that the system had made *no* headway, but that the application of this class of apparatus for traction work had made *little* headway; and I think Mr. Brown's final remarks as to the installations that had been made under this system rather bear out my remarks than otherwise.

I cannot follow Mr. Brown's apparent objection to my statement that the torque of such a motor at a given voltage is limited. The torque of a three-phase motor is limited by the impedance of its circuits, and beyond a given load the torque falls off rapidly, while in a continuous-current machine the torque increases indefinitely with the current. For ordinary traction work the average torque required is considerably less than the maximum.

Mr. Baylor. If a three-phase traction motor is so built that its maximum torque is its burning-out limit, the motor will be inefficient at the torque it would be called upon to give the greater part of the time. Mr. Brown also states that three-phase motors are lighter for the same power than continuous-current motors at the same speed, but I do not think Mr. Brown means this is the case if given conditions as to efficiency and maximum torque are to be met. I think, furthermore, that my general statements as to the use of three-phase motors in traction work are sound, and that experience so far has shown that such motors do not meet the requirements of ordinary service as well as direct-current motors.

With reference to the title of the paper, and one or two other points of criticism, I have very little to say. A better title might have been chosen, and I am perfectly well aware that a great deal of the matter in the paper cannot be looked upon as novel by the eminent engineers that sit in this room. At the same time, the references that I have made represent the latest phase of electric traction practice in the United States, where this work is at least up to date, and my impressions of electric traction in England have been gained from what I have seen in practice. Therefore I may be excused if I have inferred in the paper any lack of appreciation here of points that are in America well established, theoretically and practically. If I have attacked the intelligence of the English engineers on these points, I must apologise. I am glad to hear that the main principles of electric traction are so well known to some of the engineers of England, and I am sure that under their guidance we may now look for progress in electric traction in England which will lead the world. As to the use of direct-coupled generators in traction work, the only plant, ancient or modern, in the United Kingdom, that I know of, is the remodelled Bristol station, where four such units are now in service, and giving great satisfaction to the operators of the road, I believe. I may say that these machines were imported from America; but I do not think that need count against them, if Mr. Preece's very kind remarks may be taken as representative, as I am sure they may be.

I can only add, in conclusion, that I am highly gratified by the reception that has been given me, and your kind attention to the paper; and, if an active discussion is any criterion, I feel that the matter has awakened some little interest. Mr. Baylor.

The PRESIDENT: It must be gratifying to Mr. Baylor to see that his paper has elicited so much instructive criticism. Some of the speakers have complained that the author has not said as much as he might have on the subject of his paper, but I think you will all admit that his critics have done their best to remedy that defect. The President.

It is somewhat remarkable, but it is I think the fact, that we have had a paper on electric traction, followed by an interesting discussion, during the whole of which no reference whatever has been made to storage batteries.

One of the speakers has referred to a combined system of trolley and cable which might be useful under certain conditions. The suggestion reminds me of another combined system—that is to say, batteries and overhead trolley—which is now being tried experimentally at Hartlepool with satisfactory results. Such a system might recommend itself in towns where the overhead lines are objected to in the crowded districts, while, on the other hand, they are quite unobjectionable in the suburbs.

The description of the electric brake is, I think, extremely interesting. I do not know how it would compare with the ordinary brake as regards efficiency, but I find it on record in my notes that on a Midland tramway, descending an incline of 1 in 16 at the rate of 10 miles an hour, a car was pulled up within 15 yards: this I thought a very good performance.

As we have another paper before us to-night, I propose, if you do not object, to sit a little later than the usual hour. Mr. Raworth's paper is a very short one, but before commencing it I have to perform the agreeable duty of asking you to pass by acclamation a hearty vote of thanks to Mr. Baylor for his paper.

The resolution was carried unanimously.

THE GENERATION OF ELECTRICAL ENERGY FOR TRAMWAYS.

By J. S. Raworth, Member.

Mr. Raworth.

The development of the dynamo-electric machine, and its gradual adaptation to new purposes, have provided engineers with a series of interesting, but extremely puzzling, problems.

The first problem was to *drive* the dynamo; the second, to drive it at *an even speed;* the third, to drive it *economically* at *full load;* the fourth, to drive it economically at light load.

In 1880 almost any engineer would have tackled the whole four with that lightness of heart which many possessed who took on only one at a time, and found to their sorrow that the infant dynamo, like other young persons, could set them questions which they could not answer.

By slow and painful processes these four problems have been wholly or partially solved; and although we cannot certainly say that the dynamo has no terrors in store for us, yet we feel that we can now approach him with some of the confidence of the lion-tamer, who at least knows the exact moment when to get outside.

My attention has been directed to this question of electric traction by certain articles in the American Press, by some events which I have observed in this country, and by the fact that some central station engineers have evinced a very strong desire to supply current for traction at the modest price of 3d. per unit, "to improve their load-factor"—a very happy thought,—possibly too happy for this world.

The principal object of this paper is to show that a suitable and paying price for the supply of electric energy for tramway purposes, under reasonably favourable conditions, is *one penny* per unit, and that the said supply will reduce the cost of the lighting current, when both are generated in the same station, by an amount which, though small, is definite.

It is necessary at this point to turn back to the year 1891, when, during the discussion on Mr. Crompton's paper on the

generation and distribution of electrical energy, Mr. Willans told us what no living man previously knew—that the curve of total steam consumption of a constant expansion engine is practically a straight line, and consequently the most economical load of such an engine is its full load. He also showed us that the steam consumption at no load, of such an engine, non-condensing, is about one-third of its full-load consumption. Mr. Raworth.

These were very awkward facts. The only pleasant thing about the Willans diagram is that, as the line is straight, one can draw it with a ruler; but it was not Willans's fault that the line was straight, and we are under great obligation to him for having called our attention to it, and for having, as it were, planted a lighthouse on a submerged rock.

The immediate outcome of this discovery was that engine cylinders were made smaller; no margin was left for fall of boiler pressure or for overload, simply because any such margin would obviously have entailed waste of steam.

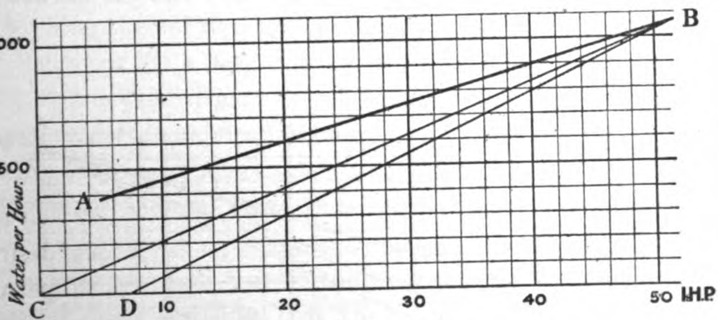


Fig 1.

The effect of the straight-line diagram is shown in Fig. 1, where the I.H.P.'s are abscissæ, and the steam consumptions ordinates. The Willans line is shown at A B.

The line C B is drawn from the origin to meet the line A B at the point B. Vertical measurements from the base line to the line C B are proportional to the I.H.P. The loss, therefore, at any given power is shown by the distance between the lines A B and C B.

Mr. Raworth.

Indicated horse-power, however, to the electrical engineer, is but a stepping stone in the path to his goal, which is electrical horse-power.

For his benefit, therefore, we must draw another line, D B, the distance C D representing the indicated horse-power necessary to drive the dynamo magnetised, but without external load; the vertical distances between the lines A B and D B represent the losses at various powers per electrical horse-power.

The electrical horse-powers are not shown by the figures, but may be ascertained by counting the squares to the right of the point D along the base line.

As Mr. Willans pointed out, these losses are terrible: for instance, in an engine taking 23½ lbs. of steam per E.H.P. at full load—that is to say, about the best commercial result obtainable—the consumption at half load is 32 lbs., and at quarter load 52 lbs.

These are all well-known and thoroughly established facts; the question for us is, "Can they be ameliorated or cured?" The answer is, "Yes;" or, more precisely, they can be ameliorated considerably, and to some extent cured.

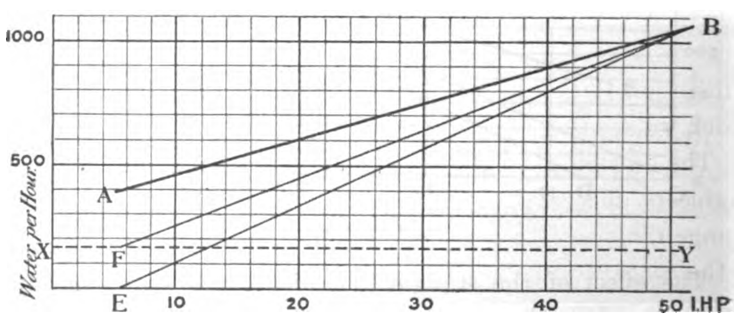


Fig 2.

The first method of amelioration is shown in Fig. 2, in which everything remains as in Fig. 1, except that by increased efficiency in the engine and dynamo the toe of the electrical horse-power line has been moved back from D to E; the line E B is thereby

made more nearly parallel to the Willans line A B, and the losses therefore reduced. The prospect of improvement in this direction is small, but we may possibly get 3 or 4 per cent. Mr. Raworth

The next step is to apply a condenser to the engine: the effect of this is more strongly marked, as it raises the base line to the position X Y, and from the point F, vertically above the point E, we draw the line F B. The new base line is nearer to the Willans line at all points than the original base line, and the lines A B and F B have become more nearly parallel.

The effect of the use of a condenser, as compared with the non-condensing example quoted above, is to reduce the full-load consumption per electrical horse-power from 23½ lbs. to 20 lbs., the half-load from 32 lbs. to 24 lbs., and the quarter-load from 52 lbs. to 33 lbs.

In both these examples there is considerable loss by working much below full load; and this fact has been so fully realised by electrical engineers, that great care has been exercised by those responsible for the design of electric lighting stations to sufficiently subdivide the plant, and by those who superintend the working thereof to make use of that subdivision so that there may be very little loss due to working engines underloaded. But even the subdivision cure has brought with it its own peculiar disease, namely, complication of pipes and valves, involving much constant waste in condensation; and, worse still, the necessity for skilled supervision to make those constant changes in the plant which we all recognise as necessary to secure economical working.

The bearing of all this on the traction question is that some engineers, fully realising that they cannot by any possibility change their engines as quickly as their load changes, have come to the conclusion that a unit of electrical energy, generated under the conditions attending the transmission of power for traction purposes, will cost as much as, if not more than, the unit now costs them for lighting.

We shall see.

If we had no better prospect before us than that which is set forth in the gospel of straight lines, which has sunk so deep into the minds of electrical engineers that the limitation of it to

Mr. Raworth.

constant expansion—that is, to throttle-valve engines—appears to be entirely overlooked, then our Diagram No. 2 would form the basis of the best results we could expect under present conditions.

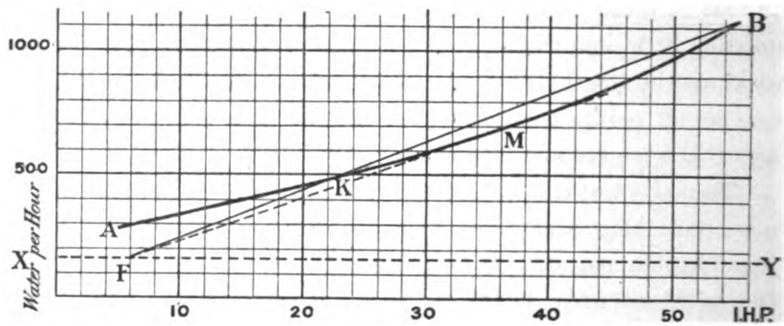


Fig 3.

But automatic expansion introduces an entirely new feature: the straight line A B of Fig. 2 becomes the curved line of Fig. 3, which, it will be observed, from the point of maximum load, B, down to the point K—which represents about one-third load in electrical output—shows everywhere a gain instead of a loss of steam per unit of energy generated; and even below the point K, down to no load, the losses are much less than in Fig. 2.

The most economical load is indicated by the point M, where a line drawn from the point F touches the steam curve tangentially.

In an automatic expansion engine working with a constant boiler pressure the mean pressure referred to the low-pressure cylinder—or, in other words, the horse-power developed—is an index of the point of cut-off; that is to say, a high mean pressure means a late cut-off, and a low mean pressure an early cut-off; therefore, to get a fairly high—that is, an economical—ratio of expansion, the mean pressure must necessarily be much lower than the practical maximum, which is about 50 lbs. per square inch. From an electrical engineer's point of view the engine must be underloaded.

The practical application of this ancient and approved theory to electric traction work is as follows:—

In the first place, as the average load remains fairly steady throughout the day, and the momentary variations are as erratic as they are great, there is obviously no advantage to be gained by subdivision of the plant; the problem therefore approaches that of factory or mill driving, and the solution will probably be similar—viz., by one engine large enough to do the work, and one man to oil it. Mr. Raworth.

There must, of course, be a spare engine and dynamo; but there is no necessity to keep the engine under steam, as the conditions of traction service in respect of absolute continuity are not so rigorous as those of electric lighting.

Assuming for the moment that this solution be adopted, I propose to prove that by it, whether it be the best solution or not, electrical energy can be supplied at the terminals of the generator, as stated previously, for less than one penny per unit.

Take as an example the case of a comparatively small tramway requiring 1,000,000 units per annum. The tramway day is 16 hours, with about 14 on Sunday, making 110 per week, or 5,720 per annum. The average load to produce a total of 1,000,000 units per annum is 174·82 kilowatts.

The actual ascertained load-factor during the working hours (varying according to circumstances) appears to be about 60 per cent., but I prefer to err on the safe side, and to assume only 50 per cent., which brings out the plant capacity at 350 kilowatts, or 470 electrical horse-power. The total I.H.P. to be provided is therefore—

E.H.P.	470
$\frac{6}{94}$ loss in dynamo	30
$\frac{10}{90}$ loss in engine	55·5
Total I.H.P.	555·5

Quite a small engine compared with the ordinary run of cotton-mill engines.

The low-pressure cylinder need not be larger than 30 inches diameter, working with a maximum mean pressure of 43 lbs.

Mr. Raworth.

The average indicated horse-power would be—

$470 \times \frac{50}{100}$	... ...	235 E.H.P.
Loss in dynamo...	... ...	30
Loss in engine ...	... ...	55·5
Average	... ...	320·5 I.H.P.

which is equivalent to a mean pressure of 25 lbs. per square inch referred to the low-pressure piston.

This mean pressure, with boiler at 150 and a 27-inch vacuum, corresponds approximately with 20 expansions, and is as nearly as possible the mean pressure adopted in modern cotton-mill engines.

The usual ascertained consumption of steam under such circumstances is $14\frac{1}{2}$ lbs. per I.H.P., and of coal 1·7 lbs., corresponding to an evaporation of $8\frac{1}{2}$ lbs. of water per lb. of coal.

In the paper on cheap steam power which I had the honour to read in November, 1896, before the Northern Society of Electrical Engineers, I raised this figure to 2 lbs., to cover all incidental losses, and was informed by several speakers that I was much outside the actual results obtained. Nevertheless, I propose to adhere to the 2 lbs. Thus we get our cost made up as follows :—

$$\frac{320{\cdot}5 \text{ H.P.} \times 5{,}720 \text{ hours} \times 2}{2{,}240} =$$

	£	s.	d.
= 1,636 tons, at 10s.	£818	0	0
Engine-driver, in two shifts, at 36s.	187	4	0
Sunday work, 1-6th	31	4	0
Stoker, in two shifts, at 24s. ...	124	16	0
Sunday work, 1-6th	20	16	0
General man, at 24s.	62	8	0
Oil, waste, &c.	52	0	0
	£1,296	8	0

Net cost per unit, 0·311d.

We now come to capital and incidental charges.

Taking the value of the plant in duplicate, including cost of land and buildings, at £17,000—of which £11,000 stands for plant, and £6,000 for land and buildings—we have— Mr. Raworth.

Rates and taxes				...	£125
Repairs,	2½ per cent.	on	£11,000	...	275
,,	1 ,,	,,	5,000	...	50
,,	0	,,	1,000	...	0
Depreciation,	5 ,,	,,	11,000	...	550
,,	2 ,,	,,	5,000	...	100
					£1,100

= 0·264d. per unit.

We have only one other item to consider—that is, *management.*

At the outset it would appear that the plant would require no more management than the steam plant of a cotton mill; but, as I am extremely anxious to find good berths for my numerous friends, I provide for one engineer in charge at £300 per annum, and £100 for office expenses—total, £400.

= 0·096d. per unit.

Our total expenses now stand thus:—

Generation	0·311d.	per unit.
General charges	0·264d.	,,
Management	0·096d.	,,
Total	0·671d.	,,
Balance	0·329d.	,,
Selling price	1·000d.	,,

Our total profit is, therefore, 1,000,000 units at 0·329d. = £1,370 16s. 8d., or 8 per cent. on £17,000.

This result is arrived at by taking commonplace figures, with, in every case, a margin on the safe side, and with a clear reserve of 100 per cent. of power over mean load.

There is actually a much larger reserve, for I have taken the maximum mean pressure at 43 lbs., whereas 48 lbs. would be still a safe figure.

Mr. Raworth. Incidentally it is worth remarking that the limitation of the working plant to one engine and dynamo is an extremely economical arrangement, because it reduces the switching gear to one large change-over switch, which may be worked by an ordinary reversing lever standing out of the floor; and the services of a switch-board attendant can be dispensed with.

On the other side of the engine, the simplification of pipes and valves is equally noticeable.

The next subject for consideration is the effect of an electric traction load on the load-factor of an electric lighting station.

It is obvious at the outset that the mere fact of the traction load-factor being higher than the lighting load-factor cannot be taken as influencing the result; for the unit of output for traction purposes, by reason of the higher load-factor, will command a lower price—the price, in fact, at which it could be produced independently of a lighting station.

The question still remains, What influence will the presence of a traction contract have on the cost per unit?

In the first place, it would increase the load-factor of the chief engineer; and if he did not at the same time demand an increase of salary, the effect would be appreciable. I suggest that a corporation possessing specially benevolent sentiments might raise him £100 a year, thus saving £200 of the £300 which I have allowed for. Further, one spare boiler might possibly be dispensed with at a saving in capital of, say, £1,000, the charges on which are worth £120 a year. Further, it would probably be possible and convenient to take steam for the day-load lighting plant from the traction boiler, thus saving one stoker on one shift—say £60 per annum.

Beyond these three, I cannot find any openings for further savings. They therefore stand thus:—

Manager	£200
Boiler	120
Stoker	60
Total	£380

Quite a considerable sum, but small in comparison with some people's expectation. It is sufficient to raise the profit on the traction plant from 8 per cent. to 10 per cent., or to reduce the price of the unit from 1 penny to 0·918. Mr. Raworth.

In all the above calculations as to the cost of generating power for electric traction, commonplace efficiences are taken, such as any engineer will freely guarantee; the boiler is only called on to evaporate 8½ lbs. of water per lb. of coal, and a very considerable margin is allowed for banking fires and other incidentals. The cost of coal is taken at 10s. a ton, which is higher than the usual price in Midland and Northern towns.

A paper dealing with the generation of power for electric traction would not be complete without a special reference to the problem of engine construction which is supposed to hang round the neck of this young daughter of electrical science.

In the technical journals articles on this subject constantly appear, and as constantly terminate without saying one single word of any value to anybody. They tell us that direct coupling is more economical than belt driving, that it is desirable to have a condenser, that the use of compound engines is becoming common; and they might add with equal truth that Queen Anne is dead.

I have not yet seen any reference to the necessity for automatic expansion which was explained and insisted on by Captain Sankey in his paper on the governing of steam engines, read before the Institution of Mechanical Engineers in April, 1895.

No pretension was made by the author to originality in his treatment of the subject, but it was, nevertheless, so elegant and so perspicuous that I have no hesitation in saying that it ought to be assimilated by every electrical engineer, and particularly by those who have any interest or duties in connection with electric traction.

Further, the aforesaid articles, whilst they omit all reference to automatic expansion, and to average or maximum mean pressure, all agree in giving special prominence to an idea, which

Mr. Raworth

may be most easily explained by the following quotation from th paper of Mr. Richard McCullock on the modern power house :—

"The main objection which was urged against the direct-"connected generator was the fact that the shocks resulting "from overloads were thrown directly on the engine, and that "there was none of the cushioning effect that a belt connection "might supply. While this is undoubtedly true, the best "argument which may be submitted against it is that none of "the installations of direct-driven generators can trace any "trouble to this source.

Thus we find American practice refuting American theory.

The theory, however, is not "undoubtedly true," but undoubtedly erroneous, as will be evident from the following considerations :—

(*a.*) In an automatic expansion engine the maximum initial pressure on the H.P. piston is attained every stroke, at any load, from quarter to full.

(*b.*) The size and strength of the parts of the low-pressure engine are always made equal to those of the high-pressure.

(*c.*) The maximum stress on the valve gear occurs at light load, not at full load.

(*d.*) Inertia stresses are reduced by reduction of speed.

The torque on the crank-shaft is undoubtedly increased by increased mean pressure; but, as crank-shafts are built to withstand the strain caused by water in the cylinder, the steam pressure never troubles them. I have myself seen cylinder covers torn off, pistons broken, and even cylinders lifted off their standards by water, without any damage to the crank-shaft.

Electric traction involves sudden variations in load, but, barring short-circuits, there is no difficulty in arranging the power plant so that in no case shall the maximum load exceed the power of the engine: therefore neither the strains nor the wear due to such causes will ever exceed those which are daily dealt with in electric light stations; for it must be remembered that electric light engines are quite commonly, even usually, built to work with a mean pressure of 42 lbs. per square inch,

and do so work without distress or trouble of any kind. Should, however, some person imagine that varying loads are more trying to steam engines than a constant full load, then, this little island of ours will provide them with any number of examples of steam engines driving rolling mills in which the fluctuations of load are more violent than any that can be found in connection with electric traction. Mr. Raworth.

The effect of short-circuits, which used to be serious, is now minimised by automatic circuit-breakers which amply protect the dynamo.

On the question of governing, it is unnecessary to say anything. With slow-speed engines and relay governors a large fly-wheel may be necessary; but with high-speed engines fitted with automatic shaft governors acting directly on the H.P. eccentric, special fly-wheel power is certainly not required.

The rapidity and precision with which shaft governors perform their functions are so remarkable that I hesitate to describe them without the support of a demonstration; but, following the example of George Stephenson, who said that his locomotive could run 16 miles an hour, I may say that the total variation in speed may easily be covered by 2 per cent.

The PRESIDENT: The discussion on this paper will be commenced at our next meeting, on the 13th of May.

I have to announce that the scrutineers report the following candidates to have been duly elected, viz.:—

Foreign Member:

André Hillairet.

Associates:

Sidney Andrews.	Reginald M. Hook.
Harry H. Beit.	Henley Lionel Howard.
W. B. Edgar.	S. C. Maulik.

Students:

John T. Callaghan.	Reginald G. F. Dumaresq.

William Laurence Waters.

ABSTRACTS.

P. DE HEEN—THE EXISTENCE OF ANODE RAYS ANALOGOUS TO THE CATHODE RAYS OF LENARD AND OF CROOKES.

(*Comptes Rendus, Vol.* 124, *No.* 9, *March*, 1897, *p.* 458.)

In these experiments the author employs a system of metal plates, fitted with points over one of their surfaces.

These plates are suspended by silk threads, parallel, but independently of one another. The Crookes tube contains the ordinary arrangement of disc forming a cathode, and a cross forming an anode.

By means of the above arrangement a large number of combinations can be arranged, yielding interesting results. In the first set of experiments four of the above plates—*a*, *b*, *c*, *d*—were connected on one side of the tube, and four plates—*a′*, *b′*, *c′*, *d′*—on the other side. With this arrangement the shadow of the cross is observed with whatever connections of the poles, and a cathode or anode shadow is produced. At the moment of the reversal of the pole of the coil, the shadow increases greatly in size, and it is only after a minute that it regains its normal dimensions.

The author also describes a number of results obtained with other arrangements of connections.

P. DE HEEN—THE PHOTOGRAPHY OF ELECTRIC RADIATIONS OF THE SUN AND OF ITS ATMOSPHERE.

(*Comptes Rendus, Vol.* 124, *No.* 9, *March*, 1897, *p.* 452.)

The author's previous researches showed that the impressions produced by electricity or infra-electricity on a fogged photographic plate, determine either an increase or a decrease of the fog according to circumstances. By this method the author photographed the sun, by placing a strongly fogged plate in the field of the objective of a small Secretan telescope, the time of exposure being about two seconds.

The resultant image shows that the power of unfogging the plate is increased from the centre of the sun towards the periphery, the edge being represented by a clear ring.

The author states as a conclusion from these experiments that, if the photosphere is principally the seat of luminous radiations, then the electric or infra-electric phenomena emanate, on the contrary, from the sun's atmosphere. As the density of the atmosphere increases towards the edge, so does the power of unfogging the plate; and it is at the edge that a clear ring is observed. By this method it is possible to photograph the solar atmosphere independently of eclipse phenomena. The author suggests examining the spots and protuberances of the sun by means of larger instruments.

J. PERRIN—DISCHARGE BY RÖNTGEN RAYS: THE PART PLAYED BY THE SURFACES ACTED UPON.

(*Comptes Rendus*, *Vol.* 124, *No.* 9, *March*, 1897, *p.* 455.)

The author has previously shown that a body placed in a gas which is in a condition of rest, is discharged by the *x* rays, if the latter meet, in the gas, the lines of force emanating from the body.

The effect is influenced by the nature of the gas, but not by that of the charged body.

When the rays fall upon this body, then the laws hitherto given do not suffice. MM. Benoit and Hurmuzescu have shown that the rate of discharge is influenced by the nature of the metals which are met. This constitutes a new effect, which is added to, but without alteration to, the effect due to the gas.

For the sake of brevity, the author calls these the "metal effect" and the "gas effect," respectively.

It was ascertained by experiment that the angle between the electric field and the rays may vary between 90° and 0° without alteration to the gas effect, thus verifying in a new manner the independence between the gas effect and the field which produces it.

It then becomes easy to compare these two effects. The compensation method employed and described by the author is as accurate as that used in previous experiments. The apparatus consists of a condenser formed by two metal plates. In one of these plates is an aperture, which is covered by a sheet of aluminium foil. The rays which fall normally to the plates pass through this window into the condenser, where they produce the gas and the metal effect.

The metal effect is *nil* when the two inner surfaces of the plates are coated with a thin layer of petroleum, alcohol, or even water. The effect becomes measurable when a single one of these surfaces is coated with a layer of gold, and the effect is doubled when the other face is treated in the same manner.

These experiments, and others, show that the metal effects due to the two faces are additive and without alteration. It is also shown that the metal effect is purely superficial; the subjacent layers having no influence. Neither does the sign of the charge.

The metal effect was found to be independent of the temperature, between the temperatures of 15° and 120° Fahr. When the value of the field increases, the metal effect varies appreciably, as also does the gas effect; the quantity of electricity discharged tending rapidly towards a limited value, and then remaining independent of the field. At an equal distance from the source, the metal effect per unit of surface is constant, and is independent of the angle of inclination of the rays. When the distance from the source varies, then the metal effect per unit of surface varies inversely as the square of the distance.

The influence of pressure on the rate of discharge has been studied by MM. Benoit and Hurmuzeşcu, but, as at that time the presence and importance of the gas effect were unknown, the law which they derived from careful observations does not hold.

From the above observations, the gas effect may be explained, if it be admitted

that at each point in the path of the x rays through the gas, the former liberate equal quantities of positive and negative electricity moving along the tubes of force containing them.

Abandoning the provisional name of "gas effect," the author proposes to adopt the term "cubic ionisation," as the phenomenon involves a separation of electricities. It will be necessary to obtain the coefficients of cubic ionisation for different gases. The metal effect is also readily explained by supposing that at the contact of a conductor the ionisation of the gas is very intense, and varies with the nature of the conductor and with the pressure; and the author proposes to call this phenomenon the "surface ionisation" of the gas in contact with the conductor.

The coefficient of surface ionisation is fixed for any gas and metal when one has chosen the gas for which the coefficient of cubic ionisation is unity. These new coefficients would form physical constants of the same nature as surface tensions or electro-motive forces of contact. The author formulates the general law of discharges by the x rays in the following manner:—The quantity of positive electricity lost in the time dt by a conductor situated in a gas in a state of rest at a pressure p, under the action of a regular source of intensity I, is equal to

$$I\,dt\left[K\,p\iiint\frac{dv}{r^2} + K'\,Q(p)\iint\frac{ds}{r^2}\right.$$

This is independently of the temperature, neglecting absorption, and assuming the electric field to be intense enough to allow of the limiting rate of discharge to be attained. In this formula, K represents the coefficient of cubic ionisation of the gas, and K′ its coefficient of surface ionisation at the contact with the conductor. The triple integral is extended to a volume common to the rays, and to the lines of force emanating from the conductor, the double integral being extended over the electrified surface struck by the x rays; and the value r represents the distance from the source to the element of surface or of volume. Each element of surface or of volume is affected by the positive sign, if the lines of force traversing it emanate from the conductor, and by the negative sign if they terminate there.

HENRI BECQUEREL—RESEARCHES ON THE URANIUM RAYS.

(*Comptes Rendus, Vol.* 124, *No.* 9, *March*, 1897, *p.* 438.)

In these researches on the discharge of electrified bodies, the author used a two-gold-leaf electroscope, different types of discharge electroscopes, and a one-leaf electrometer working with dry cells.

The instruments were protected from disturbances by surrounding them with an earthed metallic shield. It is found that uranium discharges electrified bodies in air at a distance, at potentials of below 1 volt up to 3,000 volts—with either positive or negative electricity.

If a piece of uranium be insulated, and then electrified, it is then found to discharge spontaneously in air, and the rate of discharge depends on the potential.

The author draws attention to the fact that when a mass of metallic uranium is displaced with regard to other conducting bodies, the electric capacity of the

system varies; and, as with a given rate of discharge the rate is inversely proportional to the capacity, the results of the experiment are only comparable so long as the capacity is taken into account or is kept constant. Experiments relating to the discharge of the two-leaf electroscope by a uranium disc are then described, and the effect may be illustrated by divers experiments. In one case the author employs an electroscope, the ball of which is surrounded by a large paraffin tube, a little smaller than the diameter of the uranium disc, and on the base of which the disc rests at a distance of a few centimetres from the ball of the electroscope. In another case, instead of employing an electrometer, the uranium disc was connected to a very sensitive electroscope, in which case the gold leaf indicates a continuous discharge, which stops as soon as the potential has become lower than that which is necessary to provoke a discharge.

These experiments succeed very well with potentials of a few volts. If high potentials be employed, it is essential to prevent losses by convection into the atmosphere, which might mask the phenomenon.

The disc of uranium employed in these experiments loses its electric charge very quickly; the effect is due to the particular radiation of uranium, but it may also be strongly influenced by the sharp corners of the sample.

In order to study the phenomena of charge and discharge under more regular conditions, a uranium sphere was employed.

The loss of charge was much slower with the sphere than with the disc.

The sphere can be charged by influence to a much higher potential than the disc.

With regard to the action of air on the discharge, the author showed a few months ago, that gases which have been submitted to the action of the uranium rays maintain the singular property of discharging electrified bodies, when the gases come in contact with these bodies.

The result is that, when the discharge takes place in gases in a condition of rest, the conducting property which the gas acquires must play an important part in the phenomenon.

For example, when the discharge is made in air, if the air be removed as it is modified under the action of the uranium, then the discharge becomes slower. The effect is found to increase with the velocity of the current of air, and the results of an experiment show that the removal of the air reduced the action of the uranium to one-third of its value.

Experiments with rarefied air tended towards the same results. In some preliminary experiments made by placing the uranium sphere under the bell of an air-pump, the author found that the discharge was slow in carbonic acid, and faster in hydrogen than in air.

With regard to the law of the fall of potential as a function of the time, the rate of discharge of electricity by a given surface of uranium is a function of the value of the potential V. The determination of this function has presented many difficulties, because the electroscopes employed have varying capacities with the different positions of the gold leaves; consequently the results obtained are not directly comparable. The law of the loss of charge with weak potentials appears to resemble the law of the rate of cooling of bodies but this is not a conclusive

statement. Between 1,500 and 2,500 volts the rate of loss had not appreciably varied, but this result may have been due to the insensitiveness of the instruments employed.

M. BERTHELOT—ON THE ELECTRIC ABSORPTION OF NITROGEN BY CARBON COMPOUNDS.

(*Comptes Rendus*, *Vol.* 124, *No.* 11, *March*, 1897, *p.* 528.)

This new series of experiments was carried out with the object of defining more completely the absorption of nitrogen by carbon compounds, such as benzine and sulphide of carbon, under the influence of an electric discharge; to also study the conditions favourable to its absorption, its limits, the nature of the products to which it gives rise, the reactions of these products on free oxygen, and also their decomposition by heat.

These experiments were mostly performed with nitrogen prepared from ammonium nitrite.

Under the conditions of these experiments, it was found that the rate of absorption of nitrogen by benzine was about the same with a Marcel-Deprez interruptor working at either high or low frequencies.

With a Foucault interruptor giving a lower frequency but stronger sparks the absorption was much less. The number of interruptions is then favourable.

In the presence of an excess of benzine, it is possible to completely absorb nitrogen.

The absorption is more rapid with sulphide of carbon than with benzine, but slower with thiophene than with sulphide of carbon.

The three above substances absorbed nitrogen without liberating any gas; the author had, however, previously discovered that organic substances submitted to the action of the discharge may liberate hydrogen, acetylene, ammonia, &c.

The nature of the products was examined in the case of benzine only, and the above absorption forms a product corresponding to the general composition of diphenyl phenylenediamine:—$C^6H^5 - C^6H^5 - C^6H^4(NH^2)^2$. Distillation of the product, liberated ammonia gas in abundance, as well as benzine, water (resulting from the oxidation by air), a weak trace of aniline, and a bituminous liquid containing nitrogen compounds.

An investigation was made to ascertain whether there was any regeneration of free nitrogen. The nitro-benzic derivative produced a certain amount of ammonia gas, but without the regeneration of free nitrogen. These experiments have helped to explain the nature of nitrogen compounds which are formed under the influence of the discharge acting on organic compounds. The interest of the subject is increased by the analogy which exists between this order of reactions, and those which take place between the nitrogen of the atmosphere and vegetable systems.

The author showed that nitrogen could be continuously absorbed in Nature, under conditions comparable with those of the electric discharge, by experiments on hydrates of carbon, submitted either to very weak electrical pressures, or to the action of atmospheric electricity itself close to the earth.

M. SWYNGEDAUW—ON THE DISCHARGE PRODUCED BY A SPARK, AND THE WORKING OF THE HERZ EXCITER.

(*Comptes Rendus, Vol.* 124, *No.* 11, *March*, 1897, *p.* 556.)

The resistance of a spark depends on the length, the section, temperature (of which it is a function), and also on the nature of the luminous conductor which constitutes the spark. In the discharge of a condenser into a given metallic circuit, interrupted by a spark, in order that the total resistance of the circuit may attain that critical value below which the oscillations become possible, it will be necessary to spend a certain amount of energy, w, to heat the spark to the temperature, θ, which corresponds to this critical temperature. This quantity w may be above or below the potential energy, W, of the condenser.

If $w < W$, the oscillations become possible; if $w > W$, the oscillations are not possible. $W = \frac{1}{2} C V^2$, where C is the capacity of the condenser, V the potential to which it has been charged. If V be maintained constant, and the capacity be gradually diminished, then, at a certain moment, W will be less than w, so that the discharge of a condenser which is oscillatory in the case of large capacities will become continuous with sufficiently low capacities. This is contrary to the deductions from Thomson's theory, where the resistance is supposed to be constant. The nature of the discharges from the Herz exciter are then considered. A single discharge from the Herz exciter does not exhibit any oscillatory character. The oscillations only take place when the apparatus has been working for some time; and this fact can be conceived by admitting that the temperature of the spark must exceed a certain temperature θ.

The case is then considered where the Herz exciter is placed and excited under such conditions that the discharge is oscillatory. If R, L, and C be the resistance, self-induction, and capacity respectively of the vibrator, then the period of oscillation defined by the relation,

$$\tau = \frac{\pi}{\sqrt{\frac{1}{LC} - \frac{R^2}{4L^2}}},$$

varies from an infinite value to the normal value, $\tau = \pi \sqrt{LC.}$

It is probable that, during this period, the discharge of the exciter has performed several oscillations, on account of the very low capacity and of the very small period of oscillation, τn, of the vibrator. If in this discharge, under variable resistance, a "period" be determined by the duration between two consecutive zeros of the current, then the following result is arrived at:—

In each discharge, the Herz exciter successively emits vibrations of decreasing periods, up to the normal period, $\tau n = \pi \sqrt{LC.}$

C. MARÉCHAL—A BRAKE FOR MEASURING THE EFFICIENCY OF MOTORS OF SMALL POWER.

(*L'Éclairage Électrique, Vol.* 11, *No.* 18, *April*, 1897, *p.* 210.)

This brake is free from most of the disadvantages possessed by other brakes, equilibrium being obtained by the variation in the length of the arm of a lever—a length which can be exactly determined,

A belt passes round the pulley, A, of the motor, and round a wooden pulley, B, of same diameter as A, fixed to a spindle parallel to that of the motor.

This belt is fixed to the wooden pulley in order to drag it round, and the portion of the belt passing round pulley A has holes made in it for purposes of lubrication.

The difference of tension, τ, can be adjusted by means of belt-tighteners.

To the spindle of the wooden pulley is fixed a graduated disc, D, to which a radial rod is fixed. The end of this rod carries a pan in which weights can be placed.

When the motor is running, and there is a difference of pull in the belt, the graduated disc will deflect.

If τ is the difference of pull in the belt, and r is the radius of the pulleys, then the couple acting will be $\tau\ r$, and the resisting couple $P\ l \sin \alpha$, where l is the length of the arm and α its deflection. The power in watts is given by the formula,

$$P\ w = 1{\cdot}0273\ N\ P\ l \sin \alpha.$$

The angle α is measured by the displacement of a graduated disc before a fixed index.

When the lubrication is properly adjusted, the deflection α remains sufficiently constant to allow the set of readings to be taken.

Another advantage of this brake is that several outputs can be measured by merely adjusting the belt-tighteners, and without having to alter the weights. The brake is fitted with stops to prevent the lever from rocking round too far.

This brake has been successfully employed for measuring the output of small motors by the pupils of the Ecole Supérieure d'Electricité and the Société Internationale des Electriciens.

C. E. GUYE—SOME REMARKS ON THE VARIATIONS OF TEMPERATURE OF A CONDUCTOR CARRYING ALTERNATING CURRENTS.

(*L'Éclairage Électrique*, *Vol.* 11, *No.* 18, *April*, 1897, *p.* 216.)

The author gives formulæ which express, under certain conditions, the variation of the temperature of a wire carrying alternating currents in function of time; and deduces remarks from them relative to the alternating-current arc, and to the use of thermal instruments for the measurement of these currents.

These formulæ necessitate the following conditions:—(1) The conductor should be a wire sufficiently fine, or a strip sufficiently thin, that the temperature may be considered uniform throughout the section; (2) It should be sufficiently long to neglect the loss at the end connections.

The author assumes that the loss of heat by radiation takes place according to Newton's law—that is, proportionally to the excess of temperature of the wire above that of its surroundings—which is very approximately exact, so long as this does not exceed 50° to 60° C.

If p is the mass of the wire, c its specific heat, τ the excess of temperature,

R the resistance of the wire, J the mechanical equivalent of the caloric, and k a coefficient depending on the dimensions and on the nature of the wire, and including at the same time radiation, convection, &c ; then, if

$$a = \frac{k}{pc},\ b = \frac{R\, I^2}{J\, pc},\quad \omega = \frac{2\pi}{\tau},\ \alpha = \text{arc tan}\ \frac{a}{2\,\omega},$$

$$\tau = \frac{b}{2\,a}\left[\ 1 - \frac{a}{\sqrt{a^2 + 4\omega^2}}\ \sin\,(2\,\omega\,t\ +\ \alpha)\right]\ +\ C\,e^{-at},$$

C being an integral constant. The case is then considered where the cooling does not follow the simple law of Newton, assuming that the variations of temperature under regular conditions take place through sufficiently narrow limits, since to treat the case in a general manner would necessarily lead to very complicated calculations.

The formula in this case only differs from the above by a constant:

$$\tau = \tau o\ +\ \frac{b}{2\,a}\left[\ 1\ -\ \frac{a}{\sqrt{a^2\ +\ 4\,\omega^2}}\ \sin\,(2\,\omega\,t\ +\ \alpha)\right];$$

consequently the variations of temperature are of the same nature as when Newton's law is assumed, providing that these variations are small.

With reference to the alternating-current arc, these formulæ show how there may be a lag between the maxima of temperature and of current, as suggested by Professor Thompson some years ago; it is probable, however, that this lag is less than in a metallic wire, on account of the importance of convection and of the high temperature of the arc.

The author further points out that, as the maxima of temperature coincide with the maxima of conductivity of the arc, this, with other causes, may influence the shape of the curves representing the pressure and current of the alternating-current arc. It is, however, difficult, with our present knowledge of the cooling and the conductivity of the arc, to estimate its exact importance.

H. ABRAHAM—THE ABRAHAM-CARPENTIER INDUCTION OSCILLOGRAPH.

(*L'Éclairage Électrique, Vol.* 11, *No.* 17, *April*, 1897, *p.* 145.)

The measuring instrument which was chosen consists of a galvanometer with a movable coil, this being more sensitive than the permanent-magnet galvanometer.

The author considers the theory of the instrument, and offers a solution of the problem concerning the proportionality between the deflection of the instrument and the current whose variations are to be recorded.

It is necessary that the phenomena of self-induction should be as small as possible, and that resonance effects should be guarded against. With regard to maximum sensitiveness, this is realised under the following conditions:—

1. That the resistances of the two circuits should be equal.
2. That the coefficients of self-induction of the windings should be equal to that of the movable coil.
3. That the magnetic field should be as strong as possible.

4. That the bobbin should be elongated, and its moment of inertia made equal to that of the mirror.

The instrument itself consists of three parts.

The first part, which is the most delicate, is the galvanometer. The field is produced by an electro-magnet. The movable frame is only a few millimetres wide, and is entirely covered by the mirror, and nearly fills the whole air space.

The frame is carefully balanced, to prevent any irregular vibrations or tilting.

The second part of the apparatus consists of accessory circuits, mounted on an independent board, and comprises: (1) The flat coil carrying the current I; (2) the induced coil of the auxiliary circuit; (3) on the same frame, the auxiliary inducing circuit, and the induced winding, which will be connected to the galvanometer, this double coil being movable in order to adjust the term $\frac{dI}{dt}$; (4) the rheostat with cursor for adjusting I.

The third part consists of the registering apparatus. A beam of light is thrown on to a plane mirror which reflects it at a right angle on to the concave mirror of the galvanometer, and thence on to a photographic plate, where its movements are recorded.

For experiments of precision, the plane mirror is carried on a small pendulum. The mirror is only used at the moment when its angular velocity is approximately at its maximum, and consequently very exactly constant.

The oscillograph is calibrated experimentally.

A. BLONDEL—SOME NEW APPLICATIONS FOR OSCILLOGRAPHS.

(*L'Éclairage Électrique, Vol.* 11, *No.* 17, *April*, 1897, *p.* 158.)

Oscillographs, may be used either for measuring the variations of a current or of a difference of potential. The author suggests that these instruments may also find an interesting application in the study of rapid variations of resistance of a conductor.

In the case when the conductor carries a current, it would be necessary to employ two oscillographs, one acting as an ammeter, and the other as a voltmeter. The variations of current would be recorded on a uniformly rotating drum, covered with a sheet of gelatino-bromide paper. The variations of resistance are then represented by a new curve representing the ratio of the two above curves.

In certain cases it might be interesting to represent the variations of resistance not as a function of time, but of the current itself. In this case the two oscillographs would be placed at right angles, one being fitted with a converging mirror with a vertical axis, and the other with a long plane mirror working on a horizontal axis, the beam of light reflected from the former being also reflected from the latter. Under these conditions, the final image received on a fixed photographic surface consists of a curve in rectangular co-ordinates, representing the variations of the difference of potential as a function of the current.

In the case of phenomena involving slow variations, the oscillographs may be replaced by simple Deprez-Carpentier galvanometers. By this method the open-

and closed-circuit characteristic curves of continuous-current machines can also be recorded.

The same method admits of also directly recording the hysteresis curves of samples of soft iron, by employing a small dynamo as recently suggested by Professor Ayrton. The oscillograph may also be applied for measuring the rapid variations of a resistance, not carrying a current, in function of the time. For instance, this could be applied to the method recently suggested by M. P. Janet for the study of incandescent lamp filaments.

On breaking the current through the lamp, an accumulator giving a few volts would be connected to it, and the variation of this small current recorded by the oscillograph. From this current it would be easy to obtain the resistance. The heating effect due to this small current can be allowed for.

ANON.—THE CONNECTION OF OVERHEAD TROLLEY WIRES ON BASCULE BRIDGES.

(*L'Éclairage Électrique, Vol.* 11, *No.* 16, *April*, 1897, *p.* 109.)

This method has recently been applied by the Allgemeinen Electricitäts-Gesellschaft, of Berlin, to the bascule bridges over which the new electric tramways run at Dantzig.

The portion of the trolley wire passing over the bridge is in halves; one end of each half is connected to a fixed pillar at each end of the bridge, and the other ends are fixed to two movable arms which meet together over the centre of the bridge when the latter is closed, and which are raised when the bridge is open.

A spring contact is employed for uniting the ends of the trolley wire.

H. MOISSAN—ON THE TRANSFORMATION OF THE DIAMOND INTO GRAPHITE IN THE CROOKES TUBE.

(*Comptes Rendus, Vol.* 124, *March*, 1897, *p.* 653.)

Mr. Crookes has shown (British Association Meeting, Sheffield, August, 1879) that when diamonds are placed in one of his tubes they soon lose their lustre, and become covered with a blackish coating. The author's experiments were carried out on a diamond presented to him by Mr. Crookes in the above condition with the object of investigating the nature of this black coating.

It was heated at 60° in a mixture of chlorate of potash and nitric acid.

The action on the black crust is very slow. It was removed after four successive treatments, and was found on analysis to consist of graphite.

After the fourth attack the diamond is not quite transparent; its surface is of a chestnut-brown colour. By further treatment the diamond becomes transparent. The temperature at which the diamond is transformed into graphite must be very high—above 2,000°. The slowness with which the graphite is attacked is an indication of its stability. The higher the temperature to which graphite has been submitted, the greater its resistance to oxidation.

ANON.—THE SUSSMANN ELECTRIC MINER'S LAMP.

(*L'Éclairage Électrique, Vol.* 11, *No.* 14, *March*, 1897, *p.* 19.)

In November, 1896, some trials were carried out in Belgium with the above lamp, and with apparently satisfactory results.

The apparatus consists of an accumulator containing a solid electrolyte, the whole contained in a metallic box, and of an incandescent lamp enclosed in a special case fixed above the accumulator. The accumulator has pasted plates. An ebonite box measuring 125 × 65 × 65 mm. is divided into two compartments, each containing two negative and one positive plate separated by rubber bands. The two cells are connected in series, thus giving about 4 volts.

The complete lamp weighs more than the ordinary miner's lamp, its weight being about 1·95 kilogrammes. The lamp will burn from 12 to 16 hours under normal conditions. The power required for charging is about 3·7 H.P. for every 100 lamps.

The light given by the incandescent lamp is from 2·5 to 5 times that of an ordinary miner's lamp which had been well cleaned.

The lamp offers the great advantages of keeping alight in any position, and of not being extinguished by a current of air or by an explosion.

G. LE BON—THE NATURE OF DIFFERENT KINDS OF RADIATIONS PRODUCED BY BODIES UNDER THE INFLUENCE OF LIGHT.

(*Comptes Rendus, Vol.* 124, *No.* 14, *April*, 1897, *p.* 755.)

The author considers the two kinds of obscure radiations which are produced when light falls on a body. These effects become mixed in certain experiments, notably those on polarisation and on refraction. The first order of radiations is constituted simply by ordinary light, which remains on the bodies which are illuminated, and is similar to phosphorescence, from which it differs but by its invisibility. The author will show in a subsequent contribution, that this invisible light can be refracted and reflected in the same manner as ordinary light. The second order of radiations are of a very different character, and consist of what the author has previously termed "black light."

This intermediate form of energy has the following characteristic properties:—The invisible radiations produced by light falling on the surface of bodies discharge an electroscope, but do not charge it again. They pass through electric screens. They act on photographic plates through opaque bodies. All bodies, either metallic or organic, when exposed to light, acquire the property of discharging an electroscope. With some, the discharge is more rapid if the instrument has received a negative charge; others are indifferent to the sign of the charge. The only difference between bodies lies in the rapidity of the discharge.

Certain experiments made by the author, are sufficient to differentiate the above radiations from electricity.

These radiations are more akin to the x rays, but they differ from the latter by several fundamental points, and notably by their mode of propagation.

The author, in conjunction with Professor de Heen, has studied the conditions under which photographic plates are most easily impressed through opaque bodies.

Professor de Heen has discovered that it is necessary to slightly fog the plate before exposure.

In these photographic experiments the author had to guard against the objections raised against his previous experiments—heat, pressure, stray light, &c —and had to employ a special frame for containing the photographic plate.

After illuminating the frame with different colours of the spectrum, it was found that red light appeared to be the most active. The author states, in conclusion, that if every time bodies are subjected to light, they produce a particular form of energy, then this form of energy, hitherto so little known, would be one of the commonest in nature.

G. LE CADET—ON THE VARIATION OF THE ELECTRIC CONDITION OF HIGH REGIONS OF THE ATMOSPHERE DURING FINE WEATHER.

(*Comptes Rendus, Vol.* 124, *No.* 14, *April*, 1897, *p.* 761.)

On the 24th March last, the author, assisted by M. A. Boulade, made a balloon ascent for the purpose of testing a new apparatus for collecting electricity in high atmospheric regions. The apparatus consists of strips of nitrate of lead, ignited by suitable means and lowered from the car, to different distances, by means of two graduated brass wires, connecting them to the differential galvanometer; the wires being coiled on insulators, consisting of drums of sulphur.

The weight of the total apparatus is only 2 kilogrammes, whereas the weight of the apparatus employed in the former experiments was 80 kilogrammes.

The following are the electrical results obtained; the electric field measured at (11 a.m.) starting at the earth:—

Observatory	...	Alt. = 300 m.	Field = 156 v.
Gas Works	...	Alt. = 175 m.	Field = 225 v.

The following are the successive readings obtained during the ascent:—

AT 30 METRES BELOW THE CAR.

(Δn = vertical distance of the collectors = 5 m.)

Hour.		Absolute Altitude.	Δv.	$\frac{\Delta v}{\Delta n}$.
H.	M.			
1	12	1,680 m.	+ 140 v.	+ 28
1	15	1,700	158	32
1	17	1,780	149	30
1	19	1,810	155	31
1	20	1,850	158	32
1	22	1,880	145	29
1	23	1,900	149	30
1	40	2,200	149	30
1	51	2,300	145	29

The value of the field may be considered as constant during the readings, and its mean value taken as + 30 volts at an altitude of 1,850 metres.

The registering electrometer at the observatory gave, during the same interval of time, a mean value of + 118 volts; from which the following conclusion may be drawn:—The electric field is certainly weaker above an altitude of 1,500 metres in the atmosphere, than on the surface of the earth.

H. BECQUEREL—ON THE LAW OF DISCHARGE OF ELECTRIFIED URANIUM IN AIR.

(Comptes Rendus, Vol. 124, *No.* 15, *April*, 1897, *p.* 800.)

The author stated in his previous communication that metallic uranium, owing to a radiation effect which it possesses, will not retain an electrical charge which is communicated to it.

In a subsequent paper will be published the results obtained in different gases, and at different pressures.

The present paper deals with the law of the discharge of uranium as a function of the time and of the potential of the electrified bodies

The previous communication (*Comptes Rendus*, vol. cxxiv., 1897, p. 441) showed that with weak potentials, the function connecting potential and time is analogous to that which deals with the cooling of a body: that is, $-\frac{dv}{dt}$ is nearly proportional to the potential, v; but, on the other hand, with high potentials the value $\frac{dv}{dt}$ increases very slowly with the potential, and tends to become constant. The present experiments show the relation between these two limiting laws. The experimental values which are given relate to the spontaneous variation, in function of time, of the potential of an insulated sphere of metallic uranium 13·7 mm. diameter, connected to the needle of a quadrant electrometer, insulated with paraffin. The quantity of electricity, $\frac{dQ}{dt} = C\frac{dv}{dt}$, which an electrified system loses in one second is a function both of the capacity C and of the rate of fall of potential; consequently, in order that the variations of potential may be a measure of the loss of electricity, it is essential that the capacity of the system should remain constant.

The measurements in these experiments were necessarily very delicate, and, in order to partly eliminate errors due to imperfections in the electrometer, curves were drawn representing the mean of the readings. The results are very nearly represented by the expression,

$$\frac{dv}{dt}\left(a + \frac{b}{v}\right) = -1.$$

The values of a and b in two sets of experiments were $a = 4·53$ and $b = 31$, and $a = 4·06$ and $b = 47$ in the second series; and in a third series $a = 4·6$, $b = 20$ The differences between these figures are due to experimental errors.

The coefficients a and b should be proportional to the capacity of the system.

The discharge of electricity $-C\frac{dv}{dt}$ from the surface of a sphere should be only a function of the difference of potential between the sphere and the surrounding medium. For systems having capacities C and C′,

$$C\left(\frac{dv}{dt}\right) = C'\left(\frac{dv}{dt}\right)';$$

whence $$\frac{a}{C} = \frac{a'}{C'} = \alpha, \text{ and } \frac{b}{C} = \frac{b'}{C'} = \beta;$$

α and β being values dependent on the surfaces of emission, and on the surrounding medium. This proportionality is borne out by experiment.

It is seen that the absolute dimensions of the potentials for which the term $\frac{b}{v}$ or $\frac{\beta C}{v}$ is large or small with regard to the coefficient α C, vary as the capacity of the system.

The author concludes by stating that the uranium salts, which he has kept from all known rays for a year, continue to emit radiations without any diminution in intensity, and produce photographic impressions through opaque bodies.

FOUVEAU DE COURMELLES and G. SEGUY—EXPERIMENTS MADE ON A NEW CATHODE APPARATUS, WHICH PRODUCES "x" RAYS, AND WHICH CONSISTS OF SEVERAL BULBS FIXED TO THE SAME GAS CIRCUIT.

(*Comptes Rendus, Vol.* 124, *No.* 15, *April*, 1897, *p.* 814.)

This apparatus consists of two vacuum tubes connected to a spherical reservoir. Each of the vacuum tubes contains an anode and an anti-cathode, placed in the same axis. The anode rays are directed towards the base.

This apparatus possesses the following interesting properties:—

1. When exhausted, it is found, on observing the fluorescent and cathode effects, that the internal pressure is not equal at all parts of the tube. The nature of the discharge at different parts shows that in one of the bulbs connected to the cathode the vacuum is much more complete than at the other extremity of the same gaseous circuit.
2. If current is sent through either tube, then cathode and Röntgen rays are obtained at will in either tube, with stratifications in the other.
3. The two tubes connected to two different coils each produce x rays. From this it may be concluded that the unequal division of the medium is produced during the passage of the current. The exhausted apparatus is then equally filled with stratifications.
4. With the tubes connected in series and exhausted, it is observed with a very great and resisting rarefication that x rays are produced on both sides, but in uneven quantities. The tube containing the cathode of the coil produces a greater effect than the other, which is simply connected to it.
5. When the tubes are connected in parallel, the less resisting tube produces x rays, and in the other there are flashes at intervals.

This combination of two or more tubes connected together has the advantage of simultaneously producing two images of the same object, which might be of great use in stereoscopic work.

The authors suggest that by means of these tubes it becomes possible to calculate the exact position of a foreign body in an organism by the determination of two or more triangles. This would be a simple application of the method suggested last year by MM. Buguet and Gascard.

M. G. AGAFONOFF—COMPARISON OF THE ABSORPTION OF LUMINOUS RAYS AND RÖNTGEN RAYS BY CRYSTALLISED MEDIUMS.

(*Comptes Rendus*, *Vol.* 124, *No.* 16, *April*, 1897, *p.* 855.)

The same series of crystals were used in these experiments as those in the author's previous investigations on the absorption of light and ultra-violet light.

The experiments were carried out on the same lines as adopted by M. Meslans.

From the study of the negatives obtained, the author finds that there exists in a general manner a sort of opposition between the absorption for the luminous rays and for the Röntgen rays.

The sulphates, which are generally very transparent to ultra-violet light, are generally of an extreme opacity to the Röntgen rays.

The opposite is the case with most of the crystallised organic compounds.

The nitrates absorb the luminous rays more than the sulphates, and less than organic bodies; the Röntgen rays, on the contrary, more than the sulphates and more than the organic bodies.

It appears also that, whilst the nature of an acid plays about an equally important part in the absorption of the two radiations, the nature of a base has, however, a greater effect in the case of Röntgen rays.

J. J. BORGMAN—THE THERMO-LUMINESCENCE PROVOKED BY THE RÖNTGEN RAYS AND THE BECQUEREL RAYS.

(*Comptes Rendus*, *Vol.* 124, *No.* 17, *April*, 1897, *p.* 895.)

The author refers to the description (*Wied. Ann.*, vol. lx.. p. 269, 1897) of experiments by M. Hoffmann on the thermo-luminescence under the action of electric sparks from a Toepler machine. Both M. Hoffmann and Professor E. Wiedemann attribute the cause of the excitation of the thermo-luminescence (under the conditions of their experiments) to the action of certain rays which are produced in the sparks set up between two electrodes, and which Professor E. Wiedemann has termed "discharge rays."

The author repeated M. Hoffmann's experiments, with the object of ascertaining whether this thermo-luminescence is not also produced by Röntgen rays and the rays emitted by uranium (Becquerel rays).

In M. Hoffmann's experiments a well-calcined mixture of $Ca\,SO^4$ + 5% $Mn\,SO^4$ was used.

This did not show any phosphorescence, even after a long exposure to an arc lamp. Under these conditions it did not even show any signs of thermo-luminescence, but this effect took place very vigorously under the action of the discharge from a Voss machine. Neither the author nor M. Hoffmann have observed any effect due to the nature of the electrodes. The Röntgen rays provoked a very strong thermo-luminescence.

The author concludes from his experiments that the thermo-luminescence of $Ca\,SO^4$ + 5% $Mn\,SO^4$ is provoked not only by discharge rays, but also by the Röntgen rays and by the rays emitted by uranium (Becquerel rays).

A. RAPS and A. FRANKE—ON RENDERING GALVANOMETERS OF HIGH SENSIBILITY INDEPENDENT OF EXTERNAL MAGNETIC FIELDS.

(*Beiblätter, Vol.* 21, *No.* 3, *p.* 249.)

In order to prevent the perturbations which act with equal strength on both magnets in all directions (such as earth currents), and to obviate the inconveniences incidental to the use of the iron ring suggested by Du Bois and Rubens, two different methods are proposed. In the first method one or two bundles of thin iron wires are brought near to the weaker magnets in a horizontal direction outside the galvanometer casing, so that the component lying along the axis of the bundles is strengthened. The distance of the bundles from the magnets, and their direction relatively thereto, must be chosen according to the relation between the moments and the difference in the directions of the two magnets. In this manner matters can be so arranged that perturbations taking place in one direction have scarcely any effect on the instrument, while those occurring in a direction at right angles thereto affect the sensibility, but not the position of rest. In order to compensate for the component which is produced by reason of the magnets not being turned exactly through 180°, two small auxiliary magnets are rotatably mounted on the moving system externally to the two principal magnets; these magnets being much lighter and weaker than the principal magnets, and not appreciably increasing the moment of inertia. These auxiliary magnets are so magnetised that the sum of their moments is only slightly greater than the component of the two principal magnets. By turning the auxiliary magnets the combination can be made perfectly astatic.

A. v. WURSTEMBERGER—AN APPARATUS FOR THE OBJECTIVE ILLUSTRATION OF POLYPHASE CURRENTS.

(*Beiblätter, Vol.* 21, *No.* 3, *p.* 258.)

The illustration is effected by the aid of hydrostatic and hydro-dynamic laws, which are, in many respects, analogous to electrical laws. The polyphase current

represented consists of three alternating currents which are of the same frequency, and have a relative difference of phase of 120°. These three currents are passed along three insulated conductors, and have the peculiarity that they do not require any return conductor; if the ends of these conductors be connected at a point, the quantity of positive electricity flowing towards the point of connection at any moment is equal to the quantity of negative electricity supplied thereto at the same moment. The apparatus consists of three pumps, which pump water either into or out of a common reservoir. Each separate pump acts so that the delivery or removal of water takes place according to a sine law. If now the pumps be so arranged that there is a difference of phase between their motions of 120°, the water level in the reservoir will be constant.

E. RUTHERFORD—ON THE ELECTRIFICATION OF GASES EXPOSED TO RÖNTGEN RAYS, AND THE ABSORPTION OF RÖNTGEN RADIATION BY GASES AND VAPOURS.

(*Philosophical Magazine, April*, 1897, *p.* 241.)

This paper deals with experiments made by the author to investigate the way in which electrified gases can be obtained by means of the Röntgen rays, and also to examine the properties of the charged gas.

A gas becomes a temporary conductor under the influence of the Röntgen rays, and preserves its power of conducting for a short time after the rays have ceased to act. Since the conduction in the gas is probably due to the convection of charged particles which travel through the gas with a velocity of the order of 1 cm. per second for a potential gradient of 1 volt per cm., it is possible to separate the positive from the negative conducting particles before they give up their charges to the electrodes.

The method of separation employed was to direct a rapid current of air or other gas along the surface of a charged electrode in a vessel exposed to the Röntgen rays. The vessel used consisted of a metal cylinder in which was secured axially a glass tube extending from one end to a short distance into the cylinder, and serving both to admit the air or gas and to hold a charged conducting wire. This wire was supported in the centre of the tube by thin metal spikes, and charged by being connected to one pole of a battery of small lead cells, having its other pole connected to earth. The charged gas passed away at the other end of the cylinder through a metal tube several feet in length into an insulated conductor connected to one pair of quadrants of an electrometer, the other pair being connected to earth. The metal cylinder was also connected to earth, and made so as to allow the rays to readily pass through the side. The bulb and Ruhmkorff coil were placed in a metal tank having a hole in it, so as to screen the outside apparatus from electrostatic disturbances, whilst allowing the rays to fall on the part of the charged wire within the metal cylinder.

When the bulb was not working, however rapid a current of air was sent along the charged electrode, no electrification was observed in the inductor; but

the moment the rays were turned on, the inductor became charged oppositely in sign to the charged wire. This shows that the effect cannot be due to conduction through the air, since the charge on the inductor would then be of the same sign as that on the electrode. A small plug of glass wool placed in the metal tube between the generator and the inductor completely stopped all electrification, since the electrified particles freely gave up their charge to the wool.

The amount of electrification increases with the E.M.F. of the charged wire up to a certain point, and then diminishes, and is closely connected with the value of the E.M.F., which is just sufficient to give the saturation value of the current through the gas. It was found that the presence or absence of dust in the air employed did not affect the results.

It was observed that negative electrification was discharged from the electrified gas by zinc and tin more easily than positive, whilst copper discharged positive and negative electrification with equal facility. Aluminium and lead discharged negative electricity more readily than positive.

The author also experimented with other gases, and compared the absorption of energy by various gases and vapours.

H. STARKE—ON A METHOD OF MEASURING THE SPECIFIC INDUCTIVE CAPACITIES OF SOLID BODIES.

(*Wiedemann's Annalen*, 1897, *No.* 4, *p.* 629.)

The principle of the method is the same as that of the Schwerbe method of determining the specific gravity of solid bodies. The pondero-motive or electromotive properties of an electric field filled with a non-conducting fluid remain unaltered in direction and magnitude by the introduction therein of a solid insulator, only when the fluid and solid substances have the same specific inductive capacity or dielectric constant. This condition can be easily obtained if the dielectric constant of the fluid can be varied within a sufficiently wide range by mixing therewith a second fluid having a different dielectric constant. If now the point at which equality of the dielectric constants of the fluid and solid media is attained, can be determined with sufficient sharpness, the dielectric constant of the solid body can be easily determined by measuring that of the fluid.

The author employed Nernst's method for determining the dielectric constants of the fluid mixtures. The said mixtures were composed of benzol, xylol, benzine, and many other substances; and for dielectric constants above 4 2 a mixture of benzol and ethylene chloride was employed, pure ethylene chloride, at 0°, having a dielectric constant = 11·31. The standard or calibrating fluids for the condenser vessels were purified benzol and ether.

Some of the results obtained by this method are appended, where the glasses are Schott's Jena glass, n_D being the index of refraction for the D – line, k = dielectric constant, and d = specific gravity:—

Kind of Glass.	Fabrik No.	n_D.	d.	k.
Borate crown	S. 186	1·50936	2·24	5·48
Boro-silicate crown	01948	1 51180	2·47	6 20
Phosphate crown	S. 169	1·52090	2·58	6·39
Borate flint	S. 4	1·60305	3·17	7·66
Barium crown	01610	1·57519	3·21	7·81
Baryt flint	01777	1·60284	3·40	8 28
Heaviest Baryt crown	01922	1·60899	3·55	8·40
Silicate crown	01087	1·51883	2·54	7·20
Crown with high disp.	01385	1·52333	2·70	9·13
Half flint	01469	1·6129	3·58	7·77

Material.	Source.	k.
Sulphur	... ...	3·34—4·14
Ebonite	... ...	2·80
Potash alum	... ...	6·67
Fluor spar	Stolberg, Harz	6·92
Rock salt	Stassfurt	6·29
Sylvine	,, ...	4·94
Quartz	... ...	4·73 ($k_c = k_a$)*
Calcspar	Iceland	$k_c = 8·25$; $k_a = 8·54$
Beryll	Nertschinsk	$k_c = 7·85$; $k_a = 7·44$
Gypsum	Montmartre	5·04
Mica	Finbo bei Fahlun	5·80
,, ...	Canada	5·84
,, ...	... ...	6·62

* The values lettered k_c are for plates cut perpendicular to the optical axis, and those lettered k_a for those cut parallel thereto.

M. WILLIBALD HOFFMANN—ON SOME EFFECTS OF THE ELECTRIC FIELD ON A GLOW LAMP.

(*Wiedemann's Annalen*, 1897, *No.* 4, *p.* 642.)

If a well-exhausted discharge tube be brought near an electric glow lamp through which a current is passing, it will be noticed that force will be exerted by the discharge tube on the filament of the glow lamp. On a long-continued succession of separate discharges an oscillation of the filament is set up; more

rapid discharges force the filament into a fixed position, or give it a certain rigidity; it tends to maintain its position in space relatively to the discharge tube when the glow lamp is brought near to or removed from the discharge tube.

It was found that the oscillations only occurred during the discharges, and not during the previous charging up of the electrodes. They did not take place when a metal plate was placed between the lamp and the charged plate, whether the metal was connected to earth or not, thus showing that the phenomena were purely electrostatic.

P. CZERMAK—PIN-HOLE CAMERA PICTURES WITH RÖNTGEN RAYS.

(*Wiedemann's Annalen*, 1897, *No.* 4, *p* 760.)

These pictures have already been made independently by Röntgen and Dorn, but the author has pursued the subject somewhat further than these investigators.

When pictures are taken with a pin-hole camera, all those places are shown on the tube emitting the x rays, which send effective rays through the hole of the camera. From the photographs given in the original paper, the greater intensity of the rays emitted by the platinum plate over those emitted by the glass walls of the tube can be readily seen.

These pictures were taken with an exposure of 30 minutes, the whole of the camera being closed for safety by an aluminium plate which is only permeable by the x rays. When the hole was not so covered a negative was obtained with six minutes' exposure, since the green phosphorescent light possesses considerable actinic power.

The author was able to obtain pictures by reflection from discs of tin, zinc, and steel with a one-hour exposure; the x-ray photography, however, simply showed the uniform disc without any image, so that only diffuse reflection took place.

CLASSIFIED LIST OF ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM

Appearing in some of the principal Technical Journals during the Month of APRIL, 1897.

S. denotes a series of articles. I. denotes fully illustrated.

ELECTRIC LIGHT AND POWER.

S. Hanappe—The Three-Phase Installation in the Laboratory of the Mons Special School.—*Ecl. El.*, vol. 11, No. 18, p. 193, April, 1897.

G. Richard—Mechanical Applications of Electricity.—*Ibid.*, p. 201 (I.).

M. Planck—On Electric Radiations which are caused by Resonance and which are Damped by Radiation.—*Wied. Ann.*, No. 4, vol. 60, March, 1897, p. 577.

DYNAMO AND MOTOR DESIGN.

Marcel Brillouin—The Theory of an Auto-Exciting Alternator.—*Ecl. El.*, vol. 11, No. 15, April, 1897, p. 49 (I.).

A. Potier—On a Property of Asynchronous Motors.—*Ibid.*, p. 77 (S. I.).

G. Richard—The Mechanical Applications of Electricity: "The Sautter and "Harlé Electric Servo-Motor."—*Ibid.*, No. 16, p. 101 (I.).

Alex. Rothert—The Theory of the Three-Wire Machine with a Double Magnetic Field.—*E. T. Z.*, April, 1897, No. 18, p. 230 (S. I.).

TRACTION.

G. Pellissier—On the Calculation of the Distributing Circuit of Electric Tramways.—*Ecl. El.*, vol. 11, No. 15, April, 1897, p. 68.

Anon.—The Electric Connection of Overhead Trolley Wires to Bascule Bridges.—*Ibid.*, p. 109 (I.).

Ch. Jacquin—The New Heilmann Electric Locomotives.—*Ibid.*, No. 17, p. 150 (I.).

F. Ross—The Electric Tramways of Hanover.—*E. T. Z.*, No. 13, April, 1897, p. 178 (I.).

MAGNETISM.

Dr. Kalischer—On a New Effect of Magnetism on Light.—*E. T. Z.*, April, 1893, No. 18, p. 221.

INSTRUMENTS AND MEASUREMENTS.

GEO. J. BURCH—The Tangent Lens-Gauge.—*Phil. Mag.*, 5th series, No. 263, April, 1897, p. 256 (I.).

H. SENTIS—On the Surface Tension of Water and of Saline Solutions.—*Jour. de Phys.*, 3rd series, vol. 6, April, 1897, p. 183 (I.).

A. BLONDEL—On Photometric Units.—*Ibid.*, p. 187.

H. ABRAHAM—An Induction Oscillograph.—*C. R.*, vol. 124, No. 14, April, 1897, p. 758 (I.).

MM. FOVEAU DE COURMELLES and G. SEGUY—Experiments made on a New Cathode Apparatus, generating *x* Rays, and having several Bulbs connected to the same Gas Circuit.—*Ibid.*, No. 15, p. 814.

H. ARMAGNAT—The Measurement of Resistances.—*Ecl. El.*, vol. 11, No. 15, April, 1897, p. 59 (S. I.).

ANON.—The Kelvin Ammeter.—*Ibid.*, p. 75 (I.).

ANON.—The Burstall Electric Thermometer.—*Ibid.*, p. 76 (I.).

ANON.—Felten & Guilleaume Joint-Coverings.—*Ibid.*, p. 76 (I.).

H. ABRAHAM—On the Abraham-Carpentier Induction Oscillograph.—*Ibid.*, No. 17, p. 145 (I.).

A. BLONDEL—A few New Applications of Oscillographs.—*Ibid.* No. 17, p. 158 (I.).

ANON.—The Universal Switch of Ch. Mildé, Son, & Co.—*Ibid.*, p. 165 (I.).

H. ARMAGNAT—The Measurement of Electro-motive Forces.—*Ibid.*, No. 18, p. 205 (I.).

C. MARÉCHAL—A Brake for the Measurement of Efficiency of Small Power Motors.—*Ibid.*, p. 210 (I.).

ANON.—The Pearson Switch.—*Ibid.*, p. 212 (I.).

C. E. GUYE—Some Remarks on the Variations of Temperature of a Conductor carrying Alternating Currents.—*Ibid.*, p. 216.

A. EBELING and ERICH SCHMIDT—Researches on Du Bois's Magnetic Balance.—*E. T. Z.*, No. 13, April, 1897, p. 208 (I.).

MAX. TÖWE—On a New Method of Measuring Temperature Coefficients.—*Ibid.*, No. 15, p. 218 (I.).

THOMAS MARCHER—The Taking of Curves.—*Ibid.*, p. 220 (I.).

K. R. KOCH—On the Installation of Lightning Conductors.—*Ibid.*, p. 232 (I.).

W. NERNST—On the Use of Rapid Electric Vibrations with the Bridge Combination.—*Wied. Ann.*, No. 4, vol. 60, March, 1897 (I.).

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1. The Institution's Library is open to members of all Scientific Bodies, and (on application to the Secretary) to the Public generally.

2. The Library is open (except from the 14th August to the 16th September) daily between the hours of 11.0 a.m. and 8.0 p.m., except on Thursdays, and on Saturdays, when it closes at 2.0 p.m.

An Index, compiled by the late Librarian, to the first ten volumes of the Journal (years 1872–81), and an Index, compiled under the direction of the Secretary, to the second ten volumes (years 1882–91), can be had on application to the Secretary, or to Messrs. E. and F. N. Spon, 125, Strand, W.C. Price Two Shillings and Sixpence each

JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXVI. 1897. No. 130.

The Three Hundred and Second Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, May 13th, 1897—Sir HENRY MANCE, C.I.E., President, in the Chair.

The minutes of the Ordinary General Meeting held on April 22nd were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Students to that of Associates—

Walter Francis Daniell.	Arthur Edmund du Pasquier.
Edward Macgregor Duncan.	J. C. Vaughan.
H. H. Stanley Marsh.	John Ortelli Zerega.

Mr. W. H. Tasker, Member, and Mr. F. Crawter, Associate, were appointed scrutineers of the ballot for new members.

Donations to the Library were announced as having been received since the last meeting from University College, London; Mr. W. P. Maycock, Sir David Salomons, and Mr. A. A. Campbell Swinton, Members; and Mr. F. C. Raphael, Associate; to all of whom the thanks of the Institution were unanimously accorded.

The PRESIDENT: We will now proceed with the discussion of Mr. Raworth's paper on "The Generation of Electrical Energy "for Tramways."

Mr. Geipel. Mr. W. GEIPEL: I am sure we are all much indebted to Mr. Raworth for bringing this very interesting paper before us, and for putting it into such a concise and understandable manner. The gospel according to St. John—I beg pardon—John Raworth—of straight lines and curves, is one which is meet to be read and inwardly digested by all electrical engineers, for in it are verily words of truth. I suggest, however, that the use of the words "the loss of steam per unit" is just a little misleading. Would it not have been better to have spoken of the "increased "consumption per unit"? I think, also, that it would have been interesting if Mr. Raworth, in constructing the curves, had taken also as a basis the practice generally adopted of specifying that the maximum economy should be obtained at two-thirds or three-quarters of full load. I entirely agree with the author in his tirade against the practice of excessive subdivision of the plant into numerous units. I know of one new station (it has not yet been put into use) where there are no less than 12 units of plant already installed—small units, so to speak. It must be remembered that, in addition to the increased capital outlay, involving loss by interest and depreciation, there is the enormous increased loss which must result owing to the extra radiation from the complicated pipe arrangements; for, as we know, every square foot of bare surface involves a loss, if continually in use during the year, of at least half a ton of coal, or, if covered, of one-sixth of a ton of coal. Bearing in mind these facts, the importance of not subdividing the plant excessively is brought home to us very strongly. I am glad to see, however, in the specifications which it has been my pleasure to read through during the last few months, that there is a decided tendency on the part of electrical engineers to specify a very much larger unit. I quite agree with the author in his estimate of costs; in fact, I think he has certainly erred on the safe side. It is undoubtedly advisable, for tramway work at any rate, to adopt in the first place not more than two units;

but it must be borne in mind that the population of England is not like the population of France: our population is an increasing one, and therefore our tramways will increase and will require rapid extensions of the plant. This, of course, involves an increase in the number of units employed. I notice that Mr. Raworth, in speaking of combined stations, and of the saving resulting from the combination, does not see more than three points in which a saving can be effected. If a stoker is saved by the combination during the day load, would not also a dynamo attendant be saved? I see that Mr. Raworth's allowance is £60 for a stoker, but there is no allowance made for a dynamo attendant. I observe also that, while a deduction is made for interest and depreciation on one boiler, there is no deduction made for the radiation of that boiler, which will take place, of course, during the day load; that is to say, that if one boiler can be dispensed with there will be reduced radiation, which of course is a serious loss on light load. I also think that in a combined station we might make a still further saving by giving up the running of two engines and running the day load of the lighting circuit by a motor generator. This would run quite well off the tramway engine, and there would be a resulting saving both in steam consumed and in the loss by radiation. I quite agree with what Mr. Raworth says about the fallacy of the bending of the crank-shaft. There is no doubt that crank-shafts are bent in actual use, but my experience of them is that bending is almost invariably due to the heating of the crank-pin bearing and too sudden cooling thereof, which distorts the shaft. Mr. Geipel.

Mr. E. Tremlett Carter: I feel, Sir, that the Institution of Electrical Engineers is greatly indebted to Mr. Raworth for bringing his paper before us; for the substance of the paper is rich in matters of importance to traction engineers, and the appearance of the paper itself is exceedingly well timed. I feel sure that the many important questions brought forward by Mr. Raworth will give rise to a valuable discussion this evening. Mr. Carter.

In order not to occupy time more than is necessary, I will refrain from giving further voice to thanks to Mr. Raworth, and will pass on at once to comment upon the details of his paper;

Mr. Carter. and if in some respects I disagree with his conclusions, it must not be thought that I do so in any captious spirit, or that I am not in agreement with him in the main.

Mr. Raworth states that "some engineers, fully recognising "that they cannot by any possibility change their engines as "quickly as their load changes, have come to the conclusion that "a unit of electrical energy, generated under the conditions "attending the transmission of power for traction purposes, will "cost as much as, if not more than, the unit now costs them for "lighting." I do not know who the engineers are to whom he refers; I can scarcely believe that Mr. Raworth, or anyone having a close acquaintance with the generating expenses of traction power stations, can be among their number. For the benefit of such engineers, and of all who may hold similar views, I have prepared the accompanying table showing the generating expenses at a number of power stations. Mr. Raworth has gone to cotton mills for his basis of practice in traction stations; but in the sub-

Table I.

GENERATING EXPENSES OF STEAM-DRIVEN ELECTRIC POWER STATIONS.

Item.	Liverpool Overhead Railway.		Metropolitan Railroad of Washington.	Manchester Electric Supply.	Raworth Ideal.	Crompton Ideal.
Period of working	Jan.-June, 1896.	July-Dec., 1896.	Sept., '96 Feb., '97.	April, '95 Mar., '96.	Twelve months.	Twelve months.
Train mileage ...	313,010	320,993	—	—	—	—
Units generated ...	1,343,129	1,377,060	2,835,520	1,926,000	1,000,000	5,000,000
——	Pence per unit generated.					
Coal	0·118	0·121	0·225	0·380	0·196	0·270
Oil and stores ...	0·038	0·062	0·046	0·138	0·013	0·030
Wages and salaries	0·460	0·459	0·198	0·418	0·198	0·620
Repairs & renewals	0·174	0·215	—	0·132	0·234	0·400
Total	0·790	0·857	—	1·068	0·641	1·320

Mr. Carter.

Table II.

PENCE PER UNIT GENERATED.

Item.	Chicago City Railway.		Metropolitan West Side Elevated of Chicago.
	Station 1.	Station 2.	
Fuel	0·244	0·183	0·167
Labour, &c....	0·109	0·037	0·107
Stores, &c.	0·024	0·019	0·025
Repairs, &c....	0·001	0·002	0·015
Interest and depreciation	0·165	0·078	0·200
Total	0·544	0·319	0·514
Cost of station	£77,000	£80,000	£127,500
Capacity, kilowatts	5,250	5,280	4,500
Units generated per month	1,067,200	2,375,000	1,460,000

joined Tables I. and II. you have direct evidence, based upon a comparison between traction and lighting stations, that at the present time traction stations can be run for considerably lower cost than can lighting stations. I have taken the data for these tables from the official accounts of the concerns dealt with, and in Table I. the dates are attached. The only exception is that I have not been able to obtain from the Board of Trade accounts the number of units generated at the Liverpool Overhead Railway power station in the last half of last year—the second column under that heading—and so have been obliged to estimate those units from the train mileage, taking the figure given by Mr. Cottrell in his British Association paper last year for the units generated per train mile. Besides data acquired by actual practice at power stations, I have placed in parallel columns in Table I. the ideal costs estimated by Mr. Crompton and Mr. Raworth. Mr. Crompton's data are for lighting stations, and were given in his paper on "The Cost of Electrical Energy;" * Mr. Raworth gives his data in the paper immediately under discussion. The data in these two tables leave no room for doubt in my own mind that

* *Journal Inst. E. E.*, vol. xxiii.

Mr. Carter power for electric tramways, and generally for traction on rails by electric power, can be generated at a much lower cost than can power for lighting, under present conditions. I think, however, that these same figures show, as do others which one might derive from innumerable foreign traction power stations, that Mr. Raworth will have some difficulty in arriving at that degree of economic production which will enable him, in a station of the small size described in his paper, to make a profit by the sale of energy at only one penny per unit, *i.e.*, if he is going to generate the power by steam.

Professor PERRY: Have you not included interest and depreciation in Table II., and not in Table I.?

Mr. E. TREMLETT CARTER: Yes. The capital accounts of the traction concerns in Table I. do not separate capital spent on the station from that spent on rolling stock. Hence I could not get the interest and depreciation of the capital outlay on the station. In Table II. I was able to do so.

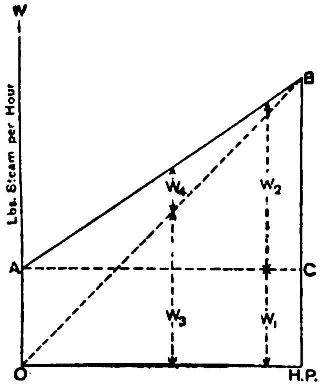
Professor PERRY: It is a pretty big item.

Mr. CARTER: Yes; but the separate items in Table I. compare with those in Table II., though the totals do not.

An important matter raised by Mr. Raworth is the relation of the consumption curve of cut-off-governed steam engines to the straight line found by Willans to represent the consumption diagram for throttle-governed engines. Not that there is any question of using throttle-governed steam engines in traction stations, where they would be practically useless, or, at any rate, extremely uneconomical; and I should suppose there is not an engineer competent to judge of the question who would advocate their use. But the bearing of the straight-line law on the matter exists, nevertheless, and it touches a point of importance, both theoretically and practically, as I hope to show.

Referring to the diagram Fig. 1A, it will be seen that the line A B corresponds to the line A B in Mr. Raworth's Fig. 1, and represents the Willans line. Now Mr. Raworth adopts a novel method of analysing the steam consumption represented by that line, and a method which I think is fallacious. He constructs the line O B, and designates the vertical distance between O B

and A B "the loss" of steam, implying that the vertical distance between O B and the horizontal axis represents the useful Mr. Carter.



$$W = W_1 + W_2 = W_1 + \omega \text{H.P.} \quad . \quad . \quad . \quad \text{(Willans)}$$
$$W = W_3 + W_4 = a \text{H.P.} + \beta \left(1 - \frac{P}{100}\right) \quad \text{(Raworth).}$$

Fig. 1A.

consumption. In other words, Mr. Raworth divides the consumption into two parts, W_3 and W_4, and consequently would express the equation to the consumption as follows:—

$$W = W_3 + W_4 = a \text{ H.P.} + \beta \left(1 - \frac{P}{100}\right),$$

where a and β are constants, and P is the percentage of full load at which the engine is running. On the other hand, it has hitherto been the custom to express the consumption by the equation,

$$W = W_1 + W_2 = W_1 + \omega \text{ H.P.},$$

which is obtained by adding the constant consumption, W_1, to the consumption W_2, which is strictly proportional to the power developed. The constant consumption, W_1, is determined, in Fig. 1A, by drawing the horizontal line A C. Apart from the greater complexity of Mr. Raworth's equation, it is necessary to observe that it does not represent the physical conditions of the case. There is no physical quantity corresponding to β; *i.e.*, there is no consumption of steam—or "loss" of steam, as he expresses it—that may be said to be a maximum with the engine

Mr. Carter. running at no load, and to decrease continuously and uniformly up to full load.

This is all the more apparent when we examine Mr. Raworth's application of his special interpretation of the Willans line to the case of engine friction, or of the losses arising outside the engine cylinder. In Fig. 2A I have drawn the Willans lines for the

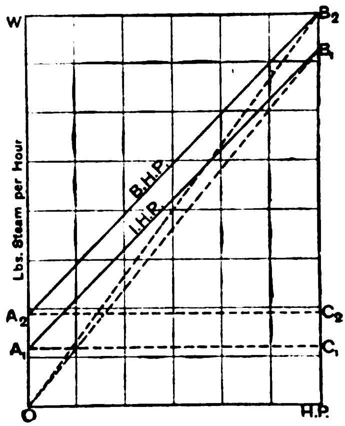


Fig. 2A.

I.H.P. and the B.H.P. of a throttle-governed steam engine. They are parallel lines; and we know that if the speed is constant, as it is assumed to be, the difference between the lines represents a consumption required to overcome a constant, steady friction in the engine. This consumption is shown by the vertical distance between the horizontal lines $A_1 C_1$ and $A_2 C_2$. But if we apply Mr. Raworth's method of analysing the Willans line we arrive at totally different and misleading results. Drawing the lines $O B_1$ and $O B_2$, Mr. Raworth would estimate the loss in each case as the interval between the line A B and the corresponding line O B; so that the effect of the constant, steady engine friction appears *not* to cause a *constant* loss of steam to overcome it, but actually increases the amount of steam usefully consumed, as represented by the interval between O B and the horizontal. In fact, at full load the *loss* of steam, as defined by Mr. Raworth, falls equally to zero, whether we consider the engine friction or neglect it, and all that engine friction at that load does is to increase the consumption of useful steam. The result is manifestly absurd.

Since Mr. Raworth has based his comparison between the consumption of throttle-governed and of cut-off-governed engines upon the method I have just criticised, it follows that that comparison must suffer to some extent from any fallaciousness that the method may possess; but it by no means follows, and indeed it is not the case, that all his conclusions are erroneous. I have already stated my belief that throttle-governed engines are of no use in a traction station, but I do not quite agree with Mr. Raworth that the reason for their unsuitability arises from an inferior economy at full load. In fact, the economy of a throttle-governed engine—which is at its best at full load—is often higher than that of a cut-off-governed engine at a corresponding load. The unsuitability of throttle-governed engines arises from their having no reserve of power to fall back upon: when the throttle valve is full open they have reached the limit of the power they can give you. With a cut-off-governed engine it is possible to obtain, though at some sacrifice of economy, a very considerable excess of power above the normal, up to, in point of fact, the power which may be developed by working non-expansively throughout the entire stroke. In Fig. 3A I have plotted a Mr. Carter.

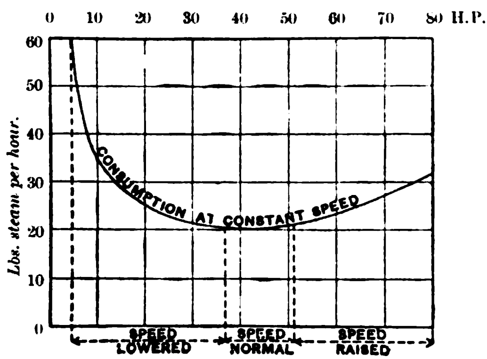


Fig. 3A.

curve showing the steam consumption of a cut-off-governed engine running at constant speed. The engine is rated to run on a normal load of 40 H.P., but can work up to double that power. Such an engine would be specially suited to a traction station with a 50 per cent. load-factor (counting only the running hours),

Mr. Carter. and where only one engine would run at any given time. With such an engine the economy is seriously impaired, whether the engine runs at much above or much below its rated load—in the former case because of insufficient expansion; in the latter case because of the excessive condensation, arising from very high ratios of expansion and the consequent fluctuation in cylinder temperature.

This brings me to the question of the number of engines it would be advisable to instal in a traction station. The sweet simplicity of having only one running engine, and a similar engine as a stand-by, is very attractive, and its advantages are obvious. Its disadvantages, although not so obvious, perhaps, are nevertheless very real. I venture to think that only where the capacity of the station has to be small will it be advisable to adopt this practice, though there are no doubt many small stations in America with only two, and in some cases absolutely only *one*, engine installed. For the general run of stations, however, the practice has the objection that it involves too great a capital outlay on stand-by or reserve plant. Suppose the maximum power required of the running engine is 5,000 H.P., then, if there is to be a correspondingly large stand-by engine, the station capacity would have to be 10,000 H.P., or 100 per cent. more than the maximum power required. If, however, the plant were divided into three equal sets, the maximum of 5,000 H.P. would be obtained from two engines, each of 2,500 H.P., while a single 2,500-H.P. engine would serve as a stand-by, making the station capacity only 7,500 H.P., or not more than 50 per cent. over the maximum power needed.

I do not think that steam-engine builders sufficiently realise that the conditions of working in a traction station are *not* identically the same as in a cotton mill. I will grant that, so far as the sudden variations in load are concerned, the mill engine, driving a forge train or a number of heavy machine tools, is subjected to just as trying conditions as the electric tramway engine. What I do disagree with is the idea that in a traction station the same average power is maintained throughout the hours of working, just as it is in many, if not most, mills. If we

neglect the sudden variations in power, due to the starting of cars, &c., we shall find that the load diagram of a tramway or railway station does not generally differ in character from the example I have plotted in Fig. 4A. I have selected from a con- Mr. Carter.

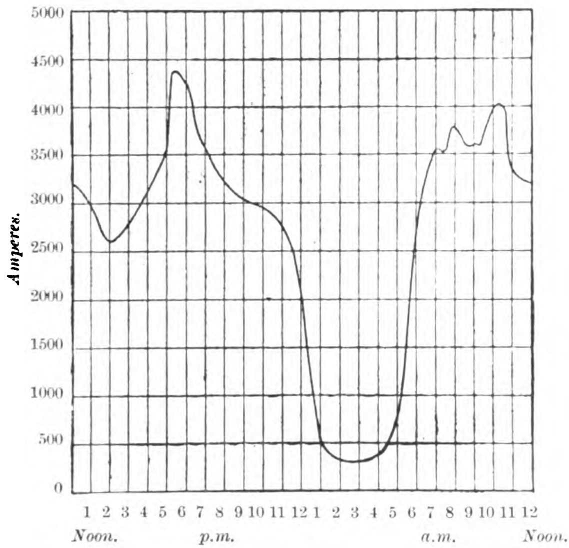


FIG. 4A.—Load Diagram of Philadelphia Traction Power Station.

siderable number of instances a load diagram of one of the stations of the Philadelphia Traction Company; and, though it would scarcely be correct to state that they all are as irregular as the one I have selected, I may say that the general characteristics are always the same. In extremely rare conditions will it be possible to run a traction plant on an absolutely, or even approximately, constant mean load, from hour to hour and from day to day.

It is of vital importance that this variation in the *mean* power—*i.e.*, the power, neglecting momentary variations—should be taken into account in designing a traction station, and especially if it is intended to have only two, or any small number of sets of generating plant. Consider what a terrible state of ineconomy would arise from running a single engine, at constant speed, on such a load as is represented in Fig. 4A. Even with

Mr. Carter. the best design of cut-off governing, it would be impossible to avoid running the engine for many hours continuously every day under extremely uneconomical conditions. One remedy that suggests itself is to further subdivide the plant; and if there were three sets instead of two, with two working and one standing idle, a better economy might be attained. There is, however, an alternative method of working to which I wish to direct the attention of members, especially as it has the advantage of rendering the economy of a single working engine much better than would usually be the case. Referring again to Fig. 3A, it will be seen that there is a certain small range of power—from about 38 H.P. to about 52 H.P.—at which the consumption of steam is very nearly a minimum. The range of cut-off represented by this range of power is evidently the range within which it is of importance to keep the engine, whatever be the load upon the engine. But if the power should either exceed or fall below the two limits specified, so as to get outside of the range stated, it would be impossible to keep the cut-off within the economical range, unless the speed of the engine were to be altered. The method of regulation that I propose consists in altering the speed of the engine in the manner shown in Fig. 3A: *i.e.*, when the load should rise above the economical limit, the speed would be so increased as to keep the cut-off within the economical limit; and when the load should fall below the economical limit, the speed would be lowered so as to delay the cut-off sufficiently to maintain it within the economical limit. It is really surprising what an economy can be effected by thus raising or lowering the speed of the engine some 25 per cent. or so relatively to a normal speed.

As applied to plants sustaining load of the nature of tramway loads, the method of governing I have suggested would, of course, only affect the speed of the engine when the variation of power was not transient or momentary in character. The governor would respond to momentary or sudden changes in load by altering the cut-off in the usual way, the speed not necessarily changing; but when any change in load remained in existence for more than a few seconds, or a few minutes, as the case might

be, the governor would control the speed of the engine in such a manner as to bring the cut-off into a more economical adjustment. At the same time, of course, the field excitation of the dynamo would be automatically controlled, so that the change in speed of the armature might not affect prejudicially the voltage developed on the feeders. I have designed the details of such a governor, but it is not necessary now to enter into particulars; the general principle of the method will be made clear by Fig. 3A. Mr. Carter.

I trust my few criticisms of Mr. Raworth's admirable and well-timed paper will be taken in good part, and that he will accept my assurance that, however badly I may have expressed them, they are not intended to clash with any one of the main conclusions arrived at in his paper; and I sincerely hope that Mr. Raworth's paper will be the means of directing the attention of tramway proprietors in this country to the great advantages which electric traction possesses over all other methods of propulsion.

Professor PERRY: May I ask Mr. Raworth if anybody governs by both the throttle and the cut-off in the same engine in cases where few units are employed and there are great fluctuations in the loading? The advantages are obvious, and the plan is easily carried out. It would be a nice sort of problem, how to obtain the best economy for the range of power which is most usual in the station; there being possibly a very poor economy at the extreme limits—on the one hand, high initial pressure with late cut-off, and, on the other, low inital pressure with early cut-off. I know of no engine in which this is done. Prof. Perry

Mr. R. E. B. CROMPTON: Professor Perry can see at the generating station of the City and South London Railway a Willans engine governed by a combined system of throttle valve and cut-off gear; but, as Mr. McMahon, the engineer to the line, is present, perhaps he can tell us about it first hand. The question of governing steam engines by the throttle valve or by automatic cut-off gear appears to recur at regular intervals of time. Each system has its advantages and its disadvantages. At the present time the automatic gear appears to be in favour; the Mr. Crompton.

Mr. Crompton. question has been so often discussed by mechanical engineers that I do not propose to go into it, especially as I am somewhat rusty on the question, but I cannot help pointing out to you that I think that Mr. Raworth has claimed too much for automatic cut-off governing. It certainly does give a much prettier diagram, having square instead of rounded corners, and theoretically the steam is used to better advantage; but, on the other hand, there are considerable disadvantages, well known to mechanical engineers, which were very apparent in the old days of single-cylinder engines, when all the expansion was done in one cylinder. The early and sharp cut-off at light loads produced by the automatic cut-off gear caused such a severe steam-hammer-like blow on the piston and moving parts that engines fitted with these automatic gears were difficult to keep in repair. Moreover, at times of light load, when automatic gear was used with slide valves, the mechanical efficiency of the engine was exceedingly low. Engines fitted with this gear were difficult to keep in sufficiently good repair to enable the advantages of the automatic gear to be apparent, so that in many cases steam-engine builders have returned to the older method of doing a great part of their governing by throttling. Another reason for the return to throttling was that the drying of the steam was greater owing to the wire-drawing action. For these reasons I doubt whether we shall get the whole of the great advantages from automatic expansion that Mr. Raworth expects. It is only fair to him to point out that his valve gear is one which will suffer very slightly from the above causes, so that in his case no doubt automatic governing will be more effective than is usually the case. The late Mr. Willans, who had given a great deal of time and thought to this matter, leaned very strongly in the direction of throttle governing; and there is no doubt that by so doing he saved us, who have used so many of his engines, a great deal of trouble and annoyance. Perhaps the best form of governing of this class that I have seen is that at the City and South London Railway.

Mr. McMahon. Mr. P. V. McMahon: Mr. Crompton has referred to the Willans engine on the South London Railway. As far as the

governing of that engine goes, I think it is the most perfect I have ever seen in a traction station. I have heard of American engines governing within 2 per cent. between no load and full. The Willans engine does not come up to that, but I think it may be safely said that 4, and at the outside 5, per cent. from no load to full load is the maximum variation in speed. The automatic expansion gear is so very sensitive that it seems to be undecided which way it will move for small variations in load. The total output of the set is 250 amperes, and with a change of load of about 20 amperes up or down, the automatic expansion is on the move, and finally settles down into its new position without variation in speed. Again, with regard to the question of one large unit, I am afraid that most traction engineers would like to consider the question of putting one unit down for important stations like the City and South London Railway, where the load varies like that shown on Mr. Carter's diagram, Fig. 4A. For about three hours in the morning and three hours in the afternoon two large sets are fully loaded, and for the remaining ten hours of the day the two large sets would only be running at about half load. With a view to more economical working, the Willans set was put in about two years ago, and runs in connection with one larger set at light load, effecting a saving in coal and oil consumption that pays the interest on the first cost of the engine and a little over besides. Mr. McMahon.

Mr. C. E. Grove: In further reference to the question of double governing, Professor Perry might perhaps be interested in watching a case (which is not at work just now, but will soon be at work) at Shoreditch Station, where it has been introduced by the engineers for another purpose. That station is a continuous-current lighting station, and has been put down in conjunction with destructor plant, and with Mr. Halpin's system of thermal storage. The boilers are not quite up to the full-load capacity of the plant in the evening, but they will have been storing energy thermally through the day. The engines will begin their evening load at 200 lbs., which falls throughout the time of full load to about 120 lbs. To meet that condition, the engineers have adopted the double system of governing. Mr. Grove.

Mr. Gadsby. Mr. C. H. Gadsby: With reference to the subdivision of plant, I consider two units too small a number. In almost all traction work the mean load, neglecting momentary fluctuations, varies considerably throughout the day; and I consider three units to be the proper subdivision of plant, if we can determine the length of tramway that is to be put down, and the service. But of course in the present condition of things in this country almost all tramways are expected to develop, and it is difficult to determine the size of the units to be installed. The variation of the output is shown on Mr. Tremlett Carter's curve No. 4A. The load throughout the day varies, so that for six hours it is about double of that for the remaining 12 working hours; so that for the light loads it is better to run one plant, putting a second into parallel for heavy loads, the third set being always in reserve.

One speaker referred to Mr. Raworth's omission of the dynamo attendant. As far as my experience goes at traction stations, it is not usual to have dynamo attendants. One engine-driver to look after the engine and dynamos is all that is usual in stations of ordinary dimensions, and in large stations there is usually one attendant at the switch-board in addition. We have heard a good deal recently about lighting from traction stations off the traction plant. I was glad to hear that Mr. Raworth had the good judgment not to advocate that sort of work. I have had charge of a traction station in which there have been 30 short-circuits in a day, and I should like to know what sort of lighting would be obtained under such conditions.

One speaker has referred to the advantage of varying the speed of the engine to get over the difficulty of the losses in economy at light load. That, however, I think is quite impracticable, even if the theory were correct. You might as well lose your steam as lose your power later on in the regulating resistances involved in such a scheme, if your voltage is to be constant.

With reference to double governing, I do not think such complications are desirable in traction work, because the cost of generating a unit of electrical energy for traction work only forms a very small percentage of the total operating expenses—

probably not more than 10 or 15 per cent.; and to save 2 or 3 per cent. of that with complicated mechanism is not of much advantage. Mr. Gadsby.

Mr. GEIPEL: I think Mr. Gadsby has misunderstood me. I did not suggest that an engine attendant as well as a dynamo attendant was necessary; I suggested one man to look after the plant, not two, and that that man would be saved just as much as one stoker. Mr. Geipel.

Mr. E. GARCKE: It was not my intention to speak. On the occasions I have been able to come here it has been more for the purpose of listening than for speaking; but I should like, before Mr. Raworth replies, to express a hope that we shall have the satisfaction of hearing the views of some of the engineers who are acting for municipalities who at present are supplying current for electric lighting, and who are in negotiation, or contemplating negotiations, for the supply of current for traction work. It has been my duty during the last two or three years to carry on many negotiations with municipalities with the view of their supplying current for electric traction, and I cannot but express my disappointment with the results of some of those negotiations, and also that so far none of those engineers have taken part in the discussion and made good the position they have taken up in the course of those negotiations. The views taken by those engineers are important as affecting the electricity supply undertakings in the country. In the course of my negotiations I have had to combat some of the most extraordinary statements with regard to the cost of generating current for the purpose of tramways. It seems to me that the majority of electrical engineers who occupy those positions seem to regard the question entirely from the point of view of what it costs to generate current for electric lighting. It seems to me that the experience of the past few years, and the admirable system which Mr. Wright, of Brighton, has enunciated, ought to have shown that the cost of the generation of the current for traction does not depend upon the conditions that obtain in the case of electric lighting. I have had to combat most seriously propositions of this kind—that we should pay for the supply of Mr. Garcke.

Mr. Garcke. electricity for traction a price which is in excess of what it would cost to work tramways by steam or horses. I have had to break off negotiations for very important traction schemes in this country because I could not see my way to pay as much as threepence per Board of Trade unit. Now an opportunity such as is offered by Mr. Raworth's paper ought to be taken advantage of for the purpose of threshing out this question and settling it. It seems a very great pity that questions that can be discussed amicably and fairly, like the present, should be altogether avoided. It is an important question in this way, because there are already a large number of electric lighting stations in working order in this country where opportunities are afforded for the establishment of electrical tramways, and if the engineer will not take a reasonable view as to what ought to be paid for the energy, the result will be that these schemes will be put back and preference will be given to schemes where it will be unnecessary to negotiate with municipalities with regard to the supply of electric current. For that reason I venture to submit that it is a matter of considerable importance that this question should be thoroughly sifted and discussed. If Mr. Raworth is wrong, I shall be one of the first to benefit by the knowledge; whilst, if he is right, I venture to predict a very extensive development for electric traction in this country in conjunction with established electric supply stations. Looking at it simply from a commercial point of view (and I do not venture to put forward any other view), it must be a very economical thing that an established electric supply station, having the power available, should use their buildings and their steam power for the purpose of supplying a current for the 16 hours a day for which it is required for tramways, instead of leaving the whole of the plant and the whole of the capital idle for something like 20 hours a day.

Mr. Wordingham. Mr. C. H. WORDINGHAM: I had not intended speaking on this paper, because I had not the good fortune to be present when it was read, nor have I had time to study it; but, after what the last speaker has said, I feel it incumbent upon me to say a word or two. As a municipal engineer I should not like it to be thought that we are all so grasping as to ask 4d., or even

3d., per unit for electric power. In Manchester we are at the present time supplying private consumers who take at least four horse-power for not less than 48 hours per week at 1½d. per unit, and we are working at a profit. I do not think there is any doubt, therefore, that electrical energy can be supplied profitably at 1½d. for long hours of consumption. I do not mean to say that one necessarily would supply a private company at that price, but there is no doubt that electrical energy can be supplied profitably at that rate; at any rate, that is our experience. Mr. Wordingham.

Mr. S. V. CLIREHUGH: Mr. Garcke's reproaches have rather gone home. I had not intended speaking, but, as one who is about to put down a station which I think will meet with Mr. Raworth's approbation, and also having been in contact with Mr. Garcke on the question of making arrangements for supplying the traction company in which he is interested with power, I think the few remarks I intend to make may be of interest. I believe the station which the Corporation of Ashton-under-Lyne is now putting down is unique in its way, since we are putting down the large units, in comparison to the output of the station, which Mr. Raworth has advocated, and we are combining traction with electric lighting. I think we are bound to be grateful to Mr. Raworth for his paper, because for those of us who are concerned with local authorities it is an enormous help. Those authorities only look at the matter from the point of view of the cost of generating electricity for lighting purposes, as shown by the Board of Trade returns, whereas they ought to look at it from the point of view of the cost of a horse-power-hour. Mr. Raworth reminded me this evening that I was to a certain extent the instigator of this paper by suggesting to him that we ought not to consider the question of Board of Trade units in traction work, but the cost of horse-power-hours. Whether they are electrical horse-power or indicated horse-power hours is a matter of comparatively little moment. There is hardly any point in which I am at issue with Mr. Raworth, beyond the fact that I think he might have made a rather better case out for himself in combining lighting and traction plant. The actual running cost is important, because a good deal more may be saved by the judicious use of Mr. Clirehugh.

Mr. Clirehugh. accumulators; that is to say, it is quite possible to run your lighting in parallel off your accumulators or off your traction machine. In a small town, say of 40,000 or 50,000 inhabitants, the tramways are stopped at night, and the whole plant is shut down at 12 o'clock, and the lighting supply is entirely run off the accumulators; whereas during the daytime the supply could be run off the accumulators or off the traction machines. The last speaker but one dismissed that very important fact almost contemptuously by saying that nobody who had had any experience of traction work would attempt to run their lighting plant off the same engines. I think that is rather a sweeping remark to make, and I do not think it ought to go uncontradicted. I myself totally disagree with it. I have had a certain amount of experience with traction stations (we are all familiar with many hundreds of traction diagrams), and I believe absolutely that electric lighting can be run satisfactorily off the same engine that supplies the traction. It is a question of the two things—the governing and the fly-wheel capacity. With regard to short-circuits, I think that has been somewhat overrated. Mr. Raworth opens his paper by mentioning the development of the dynamo-electric machine for generating power for tramways. I think the whole question of the plant and generating power for tramways has been made rather a bugbear. We all seem a little bit afraid of it, and I think the Americans have frightened us. In my opinion, they have done it designedly. As a matter of fact, lighting dynamos in America, when applied to traction work, are, and have been, unsatisfactory. The reason, my opinion is, is that the factor of safety was cut down very much too low. In this country a dynamo which we consider good enough for lighting purposes is good enough, or very nearly so, for traction purposes.

Mr. Morse. Mr. SYDNEY MORSE: There is one point which I should like to mention, which I think will do no harm. Though the local authorities, as Mr. Garcke has told you, ask such large rates for current for electric traction, they have done a great deal of good towards the extension of electric traction in this country by the very excellent reports which they have published of the tours of their committees on the Continent. No doubt those tours have

been very enjoyable to the committee, but the committee have at the same time given us some very valuable and useful information, which we will turn to profit in the course of a very short time. For example, the report of the Sheffield committee gives us this information—that at Elberfeld the price for current is a penny farthing per unit; and at Hamburg, although they pay 18s. a ton for their coal (they use Welsh coal), they only pay a penny a unit. Mr. Raworth, I think, based his figures on coal at 10s. per ton. At Berlin the price is a penny; at Dresden a penny halfpenny, including 20 per cent. which goes to the municipality. At Liége it is only a penny halfpenny. I venture to say, therefore, that Mr. Clirehugh has been very courageous, and at the same time wise, in making a contract for the supply at a reasonable rate. I don't know whether I am at liberty to mention the present price. Mr. Morse.

Mr. CLIREHUGH: It does not matter; it is three halfpence.

Mr. MORSE: Mr. Clirehugh says that it is three halfpence. I think he will make a very good profit out of it.

Mr. A. J. LAWSON: Before Mr. Raworth replies, I should like to direct your attention to an omission from all these prices for current which are given, and that is the omission of the minimum quantity which is to be taken. It has a very important bearing indeed upon the whole question of costs. Mr. Lawson.

Mr. F. J. WARDEN-STEVENS [*communicated*]: All electrical engineers who are interested in the combination of electric tramways and lighting will thank Mr. Raworth for his contribution to the Institution on this subject. There has been, and is still, great uncertainty as to how far the combination is a desirable one. It would appear from the paper that the acquisition of the tramway load is not of any considerable use to the lighting engineer. Mr. Warden-Stevens.

It is rather difficult to see why the load-factors of the manager, stoker, and boiler are the only ones improved. Surely the assistant engineers', engine-drivers', and attendants' load-factors are also very much improved, to say nothing of the capital saved, by having one central station instead of two, and any engines and dynamos that could be dispensed with if one system were suitable

Mr. Warden-Stevens. for both, such as 440 volts across the outers of a three-wire network, and 440 volts for tramways, with boosters to regulate the pressure.

Although the mains would be separate, they could often be drawn into the same conduits, with a saving for excavation, and there would be a distinct improvement in the load-factor of the distribution staff, as also with the clerical staff. I quite agree that the traction plant receives very little benefit from the lighting plant as regards coal per unit; but the lighting plant will receive a help, especially if accumulators can be used to reduce the plant for the lighting by the amount of the overlapping of the load curves. This benefit would be considerable in a seaside town where the tramways are closed for the winter, and the units delivered for lighting in the summer are few, but the load is fairly high for a short time.

I am aware that the title of the paper is "The Generation of "Electrical Energy for Tramways," but Mr. Raworth makes the lighting a point in his results; and I think there are likely to be misunderstandings, as he has not fully dealt with this matter, although he has made statements.

Referring to the curves, I presume these are actual results, and that the engines were similar in every respect, except that of the method of governing.

It is difficult to see why, of the two engines, the automatic expansion engines should be so much better at all loads than the throttle-governed one. If the engines were not similar in every respect, many circumstances may have altered the case, such as wet steam, lagging, &c. I should hardly think that the automatic expansion engine would show such good results at the low loads with the wet steam found in central stations, but no doubt the engine was tested under better conditions.

If Mr. Raworth's curves are correct, it follows that automatic expansion engines are more suitable for *all* work, and not only for traction, especially as the governing is so good. I do not see how anyone can disagree with Mr. Raworth as to the suitability of direct coupling for traction work. There can be no greater change of load than a short-circuit. Every engine and dynamo

made ought to be calculated to stand a sudden short-circuit, or it is of little use, and the variations of load for traction are negligible compared with this. Mr. Warden-Stevens.

Mr. MARK ROBINSON [*communicated*]: I desire to thank Mr. Raworth for a valuable paper, in the general conclusions of which I agree; but at the same time to express concurrence in Mr. Tremlett Carter's objections to the form of Mr. Raworth's diagrams. Mr. Carter was right in starting the "Willans "straight line" (A B, in his figure 2A) from the vertical corresponding with *no* indicated horse-power, even though the first observation ordinarily obtainable corresponds with the comparatively considerable horse-power required to overcome the engine friction. The consumption when running at no horse-power, and even at less than no horse-power, has been ascertained by Captain Sankey, by actual test at Thames Ditton. The engine was coupled to a dynamo, used as a motor, and the current was regulated so that diagrams were obtained at very low mean pressures, at no mean pressure, and even at negative mean pressures—*i.e.*, when the engine was working as a pump and drawing steam out of the boiler. The point A was proved to lie in the prolongation of the line obtained by observations under ordinary loads. Mr. Robinson.

Surely the "loss" which Mr. Raworth wishes to show is better expressed by the usual consumption curve, where the base line represents horse-power or mean pressure, and the ordinates represent pounds of steam per horse-power-hour. It seem to me almost impossible to combine *total* steam consumption with steam consumption *per horse-power*, in the manner proposed by Mr Raworth, without some risk of confusion.

It is better in such diagrams to show as a base line, not horse-power, but mean pressure, for in fact the whole comparison between throttling engines and variable-expansion engines turns upon the mean pressure employed, and upon almost nothing else. If the idea of horse-power is thought too helpful to be given up, it might for convenience be assumed that one pound of mean pressure on the L.P. pistons gives exactly one horse-power, as was suggested in Captain Sankey's well-known paper on governing, in which he also

Mr. Robinson.

proposed that such an engine should be called a "standard "consumption engine." Doubtless the 53 I.H.P. which represents full load in Fig. 3 was obtained at so high and uneconomical a mean pressure, that there was a great saving in steam per I.H.P., and even per E.H.P., as the load fell off. But, if so, the comparison implies the use of a throttling engine under circumstances in which no competent engineer would use one. The matter has been treated fully, and I think conclusively, in Captain Sankey's paper above-mentioned (see Fig. 6, Plate 22, and at p. 160, *Proceedings of the Institution of Mechanical Engineers*, 1895).

Reference has been made by Mr. Crompton to the Willans engine at the City and South London station at Stockwell, which governs by altering the cut-off down to a certain load, and then by throttling down to no load. That engine simply carries out in practice the principles embodied in Fig. 38 of Mr. Willans's paper on steam engine trials (*Proc. Inst. C.E.*, vol. cxiv., part iv.). Mr. McMahon has kindly testified to the goodness of its governing, as being about the most perfect he has ever seen in a traction station; but, as a fact, the governing has been deliberately made *less* accurate in this engine, for reasons connected with the governing of other engines with which it had to run in parallel. There can be no doubt that this system of governing is ideally perfect from the point of view of steam economy. It has been applied by Messrs. Willans & Robinson in a great number of engines, not only for traction purposes, but for mill driving and the like—in fact, in all cases (which there is never the smallest difficulty in determining when the facts are known) in which it is really advantageous to use automatic gear. The Willans engines working the tramways at Liége (where the low charge made for current was mentioned by Mr. Sydney Morse) are so fitted; so are the Willans engines supplied for tramway work in Dublin, Bristol, and elsewhere. I cannot quite agree with Mr. Raworth that a fly-wheel governor, which more or less throttles the steam opening at the same time that it alters the cut-off, carries out the system in question. So long as governing is by

variable expansion, surely there ought to be no throttling, and when throttling begins there ought to be no more change in the point of cut-off. Mr. Willans's advocacy of throttling, as against varying the cut-off, was partly due to a natural reaction against the extravagant, and often ridiculous, claims made for automatic gears, but perhaps chiefly to the fact that during his lifetime the application of his engines was almost entirely for electric lighting purposes, where it was far more useful to enforce the necessity for keeping up full loads, than to provide for light ones. Mr. Robinson.

It should never be forgotten that the greatest saving from automatic gear, even in traction work, which is an ideal field for it, arises rather from the facilities it gives for letting an engine work wastefully (*i.e.*, at an uneconomically high mean pressure) for short periods, *without making it wasteful through insufficient expansion at all other times*, rather than from any direct effect in producing economy at light loads. At very light loads, automatic governing is even a direct source of loss; and, though I agree with Mr. Raworth in considering it indispensable for traction work, it is very easy to exaggerate its economic advantage.

Mr. Raworth. Mr. RAWORTH, in reply, said: I quite sympathise with the views expressed by Mr. Garcke, and, with him, I feel somewhat annoyed at having been allowed to get off so extremely easily this evening. I have put forward views in my paper which (although I am glad to hear from Mr. Tremlett Carter that they are quite reasonable, and in agreement with results obtained in America) have nevertheless excited considerable opposition. I have been told that, although my figures are to a certain extent plausible, they were altogether wrong, and that some day or other I should find it out;—I only hope I may live as long. I quite agree with Mr. Geipel that it may be possible to find some further savings to be achieved by the combination of lighting and traction plants. Of course any further savings that may be discovered will be to the benefit of my argument, and I hope I may be a ratepayer in one of those municipalities that may realise those advantages. Although Mr. Tremlett Carter agrees with me perfectly as to the main trend of my arguments I have adduced, he takes exception

Mr. Raworth.

to my form of diagram. I should like to point out why I produce the diagrams in the form I have chosen, and not in the form suggested by Mr. Carter. In the first place, Mr. Carter's diagrams do not quite reproduce the Willans idea, which was to plot the actual total steam consumptions at various loads; but the point A in Mr. Carter's diagram is over the origin of the diagram, where there is really no indicated horse-power at all; Willans's diagram begins at the point A, as I have shown it in Fig. 1—*i.e.*, when the engine begins to run at full speed. That is the earliest point at which one can make a measurement and obtain an ordinate. Looking at Fig. 1, you will observe that I have called the distances measured between the line D and the line A "loss." Mr. Carter does not think that is a proper expression, but I think we shall agree perfectly if we commence with a definition of the word "loss" The best result obtainable from a throttled engine is the result of full load, as shown by the vertical measurement at the point B, representing 23 or 24 lbs. of water consumption per electrical horse-power hour. Then if you work at any lower power than full load you are making a loss upon that result, and it is only in that sense that I have used the word "loss"—a loss of steam per E.H.P. as compared with maximum efficiency. I have put the diagrams in this form simply for the sake of the educational effect of direct comparison. Mr. Carter's diagrams come to the same thing in the end. Referring to my Figs. 1, 2, and 3, you observe the same arrangement of lines in each. You get Willans's line A B in the first case, and Willans's line A B under improved conditions in the second. In the third case you observe the corresponding line A B for an automatic expansion engine. Thus you see at a glance exactly how *that* line lies with regard to the line F B. Not only does it not always run above the straight line F B, but for a long portion of its career it goes below it, actually making a saving at lower powers as compared with the amount of steam per electrical horse-power which you have ascertained to be necessary for the full-load output. It is only at powers below the point K that loss occurs, and that loss is marked in red so that you may avoid it. You will have noticed

that the American station which Mr. Tremlett Carter has quoted as having given such excellent results, has an output of more than twelve times the output of the station which I have suggested for your consideration. Therefore it was quite necessary for me to take up the subject from the beginning, and to show you in what manner similar results could be achieved with a reasonable output; for, so far as I know at present, there is no tramway, either running or projected, in this country that will require more than a million units per annum. Mr. Lawson is perfectly correct in stating that the cost of supply is influenced by the number of units required per annum. The figures I have given are based on an output of a million units; and if you wish to know what it will cost for a smaller output, you can easily transpose the figures and work the result out for yourselves. Mr. Raworth.

I now come to Professor Perry, who asked the very important question, Has anyone governed engines by combined expansion and throttling? I think it is very generally known now that Messrs. Willans & Robinson are so doing. The method they are now employing is the outcome of a most excellent paper by Captain Sankey, or *vice versâ*, which showed very clearly that with pure automatic expansion the black line of total steam consumption in Fig. 3 would cross over the Willans line (not shown on *this* diagram), and would show a very serious loss at light loads—not only the loss shown in the figure to the left of the point K, but actually in excess of that obtained with the ordinary Willans diagram shown in Fig. 1. Captain Sankey pointed out that this could be prevented by turning the engine into a throttle-valve engine, after the expansion gear has reduced the mean pressure to about 14 lbs. per square inch, from which point the curve would follow the Willans law; therefore he brought out a most ingenious invention, whereby he combined automatic expansion gear with throttling, but with what practical result I do not know. In the shaft governor, one sample of which I make, and which is known all over the world, you get the required result without the exercise of any ingenuity whatever. An automatic shaft governor at light loads gives throttled diagrams. The result is that we get the exact combination that

Mr. Raworth.

Captain Sankey has obtained by other means. That is the reason why the curve in Fig. 3 runs down to such a low point, and shows the losses to be so extremely small at light loads. Mr. Crompton says that he thinks it was because Mr. Willans so greatly appreciated the practical advantages of throttling, as compared with automatic expansion, that he went in so very strongly for throttling. I must confess I always thought the reason why Mr. Willans went in for throttle governing was that he did not see his way to apply automatic expansion to his engine. If he had seen how to do it, we should have had all the advantages of automatic expansion explained to us with the same force and lucidity with which the defects of throttle governing have been expounded to us during the past eight or nine years.

I should like to mention that the conditions explained to us by Mr. McMahon in regard to the generating station of the South London Railway are not exactly the same as those of a tramway. They are in some degree similar, but they are not completely so.

Mr. Gadsby says he does not like two units, but he would prefer three. This is a question of the conditions of the service, the size of the installation, and other matters which I have not been able to go into. The only statement I have made is: "The "problem, therefore, approaches that of factory or mill driving; "and the solution will probably be similar—namely, by one "engine to do the work, and one man to oil it." It is quite obvious that when you require a station of 5,000 or 6,000 horse-power, the question of how many men are employed sinks into comparative insignificance. The other factors in the bill are so large that the amount of labour forms a very small percentage of the whole. I shall be quite prepared to discuss the question of how many engines there should be, when circumstances demand it; but at present I have no case in my mind which will require over a thousand horse-power, and I think I am perfectly justified in saying that in such cases the solution will probably be as stated.

I am very glad to hear that Mr. Wordingham has been successful in getting the cost of his current down to a penny halfpenny per unit. I was very much pleased to hear what Mr. Clirehugh had

to say. Being a Manchester man, I am perfectly certain that he would have much difficulty in convincing a cotton spinner that, while he is producing his power for his own mill at something like a farthing per horse-power, it cannot be produced for less than three halfpence for traction. Mr. Raworth.

Mr. Warden-Stevens has raised many points which I know are in the minds of electrical engineers, but which were not referred to in the discussion.

In the scheme worked out in the paper no assistant engineers or attendants are required, so I could not save any wages under that head by a combination of lighting and traction plants.

Such combination would not save the cost of any engines and dynamos, because the lighting and traction loads overlap, and full provision has to be made for both.

If accumulators were resorted to, a saving in engines might be made, and in some special cases in dynamos also, but it has yet to be demonstrated that the total cost would thereby be reduced. My opinion is that it would not.

Referring to the curves, that given in Fig. 3 was taken from the "Universal" engine, merely because I had it at hand. Other engines with crank-shaft governor give similar curves; and, as I stated in the paper, the nature of the curves of automatic expansion and of automatic expansion combined with throttling at low load, has been fully explained by Captain Sankey, who is an authority on the theory of the steam engine.

Mr. Warden-Stevens is wrong in stating that the automatic expansion engine is better at all loads than the throttled engine; for at full load the results are alike, but at lower loads the automatic engine derives the full benefit of the high boiler pressure, and gets more work out of the steam than a throttled engine, down to the point of about 14 lbs. mean pressure, below which the loss by initial condensation overbalances the gain by expasion. The shaft governor, however, possesses the merit of throttling the steam at light loads, being in this respect better than the ideal automatic cut-off, to which ideal the Corliss trip gear very nearly, though not quite, attains. Therefore at light loads the curve given by the shaft governor becomes nearly coincident with the Willans straight line.

Mr. Raworth.

The way in which the practical result accords with Captain Sankey's theory is most interesting, and is another example of the value of theory.

Mr. Warden-Stevens is quite right in assuming "that automatic expansion engines are more suitable for *all* work," but it has not always been so, because a few years ago automatic gears were so expensive and troublesome that "the game was not always worth the candle;" now, however, the improvements in the automatic crank-shaft governor have rendered it actually cheaper and simpler than a throttle governor.

The full advantages of the crank-shaft governor can only be realised when it is arranged to control the admission valve only, for otherwise it produces excessive compression, and, consequently, loss at light load.

I thank Mr. Robinson for his contribution, but I wish to point out that with any other form of diagram than that chosen I could not have so clearly demonstrated the absurdity of applying the Willans law to automatic expansion engines. The urgent need of such a demonstration is shown by Mr. Robinson's own words (May 23rd, 1895): "Whether you reduce the power in one way "or another, the result is so bad in every engine (in comparison "with what a throttling or variable-expansion engine may "achieve when it works at full load) that the only moral to be "drawn is, Do not work at light loads at all, or, if you must do it, "do it as little as possible."

I quite agree that it is better to show mean pressure rather than horse-power to an audience composed of steam experts, and for the benefit of such I will now give the required information.

The mean pressure at 53 H.P. in Diagram No. 3 was 47·5, and at all these powers the mean pressures were 1 H.P. × 0·9; the engine complied very closely with Captain Sankey's ideal experimental engine, which gives 1 H.P. for 1 lb. mean pressure.

It is a fortunate circumstance that the Willans "steam engine "trials" contain three examples which serve most admirably to illustrate the argument of the paper, and to show, further, that there is no great waste involved in working an engine up even to 47 lbs. mean pressure. Mr. Willans's experimental engine had

according to Captain Sankey, an efficiency of 90 per cent. at 40 lbs. mean pressure; and the dynamo which I have quoted in the paper has an efficiency of 94 per cent.: this combined efficiency is therefore 84·6 per cent. The mean pressure required to drive the engine and dynamo without external load is $40 \times \frac{100 - 84 \cdot 6}{100}$ - 6·15 lbs. Mr. Raworth.

The three examples referred to will be found in Mr. Willans's paper under the following reference letters:—

M 2.	E 3.	O 3.
$c\frac{125}{4 \cdot 82}$	$c\frac{130}{10}$	$c\frac{135}{15 \cdot 5}$

The following table gives the results of the trials—the deductions therefrom being shown in italics:—

	M 2.	E 3.	O 3.
Steam chest pressure (absolute)	135·69	139·63	139·92
Mean pressure	46·89	31·67	25·23
Mean pressure required to drive engine and dynamo	*6·15*	*6·15*	*6·15*
Mean pressure available as electrical power	*40·74*	*25·52*	*19·08*
Pounds of steam per I.H.P. ...	16·72	15·18	14·98
„ „ „ E.H.P. ...	*19·15*	*19·0*	*19·8*
„ „ „ unit ...	*25·7*	*25·5*	*26·6*

These figures correspond with a curve slightly flatter than that shown in Fig. 3, but they show a higher degree of economy than is assumed in the paper.

It should be observed that the results obtained at 46·89 lbs. mean pressure are highly economical; indeed, it is extremely doubtful whether they have ever been surpassed under any circumstances, excepting, perhaps, in the example marked E 3, and even in that case the gain is infinitesimal.

The trial O 3, which gives something less than half power, is only 3¾ per cent. worse than the full power.

It must be remembered that although Mr. Willans, did not use automatic expansion gear in his trials, he obtained the same result by altering the cut-off of the engine prior to the

Mr. Raworth. commencement of each trial. The results he obtained proved two things—

A. That the power of an engine can be reduced by cut-off without serious loss of efficiency down to half load.

B. That very high mean pressures may be used by the aid of automatic expansion gear without producing a wasteful result.

I have nothing further to add, except to thank you for your attendance, and for the interesting remarks that have been made upon my paper.

The President. The PRESIDENT: I think we may take it for granted that most of us are agreed as to the desirability of increasing the size of our units when possible.

I have just added to the Oxford generating station an additional engine and motor, the capacity of which is nearly three times greater than any of the previous units. But assuming that I were to offer to supply the tramway company with electricity at a reduced rate (I am not anxious to do it at 1d. per unit), I don't think I should feel quite comfortable with only this one large unit available. In fact, I have already begun to consider the desirability of putting down another. In laying down large plant which is to some extent unremunerative for a certain portion of the day, one has to think of the capital it is necessary to provide; this is a very serious matter for those who are responsible for the finances of the company. I do not think the glowing picture Mr. Raworth held out of £200 or £300 a year profit would be sufficiently tempting to induce the average electric light company to raise the large amount of capital which would be necessary for the privilege of efficiently supplying the local tramway with current at such a low rate They would prefer to confine their business to supplying current for electric lighting. I have no doubt many companies with their present plant could generate in two or three weeks as much electricity as they could sell in a year; nevertheless, we are obliged to have a certain amount of reserve power, in order to enable us to get over the "peak," and serve as a stand-by. Our difficulties would be increased if we were at the same

time serving the tramways, as they would be calling on us for current during the time of our greatest load. To avoid the risk of breaking faith with the public, it would be necessary to put down additional plant, and this plant would probably have to be duplicated, thus entailing a very heavy capital expenditure. The President.

Before passing on to the next paper, I will ask you to accord a hearty vote of thanks to Mr. Raworth.

The motion was carried by acclamation.

The following paper was then, in the absence of and at the request of the author, read by Professor S. P. Thompson, V.P.:—

DISTURBANCE OF SUBMARINE CABLE WORKING BY ELECTRIC TRAMWAYS.

By A. P. Trotter, Member.

On August 6th, 1896, the electric tramway service in Cape Town was opened, the cars running from the boundary of the suburb of Mowbray to the corner of Darling Street and Adderley Street (see Fig. 1).* The rails are 81 lbs. to the yard, and are double bonded with "Chicago" bonds, except between Adderley Street and the tramway works, where the track is double and the rails are triply bonded. A test made on September 2nd showed that the fall of volts on the rails was from half a volt to about one volt. From two to three tenths of an ampere passed between the earth plates at the works and the dynamos. There are two earth plates, but by a mistake only one wire was brought from them. A test made between the earth plates and the feed service of the boilers showed that 2 volts caused $2\frac{1}{2}$ amperes to pass. The works are about one and a quarter miles from Adderley Street. Mr. Trotter.

The submarine cable is laid in a loop round Table Bay (Fig. 1), in order to avoid anchorages, and is landed at the foot of Adderley Street at a cable hut. From this hut a pair of cables are laid under the street to the Standard Bank, at the corner of Adderley Street and Darling Street, a distance of about 430 yards. The

* Such of the figures as are not inserted in the letterpress will be found on the plate at end of the paper.

Mr. Trotter. office of the Eastern and South African Telegraph Company is situated in the Standard Bank building, and looks out on to Darling Street. The first mile of the cable is at a mean distance of about half a mile from the tramway.

As soon as the tramway service began, the working of the siphon recorder was found to be seriously disturbed. Numerous "kicks" occurred, and these being superimposed on the received signals made it difficult and often impossible to decipher them. On August 14th I visited the office of the cable company with the Postmaster-General, and witnessed the disturbances. The then terminus of the tramways was almost immediately opposite the window of the cable company's office, and the movement of the driver's hand on the controller of the car could be watched while standing by the siphon recorder. We waited until a car started, and simultaneously with the movement of the handle of the controller, a "kick" was recorded on the slip. In a report to the Postmaster-General, dated August 16th, 1896, I suggested that the disturbance might be due to the direct leakage of current, owing to the use of earth as a return by the tramway, or to induction owing to the proximity of the cable; and that the first might be removed by the use of an earth at a considerable distance, and the second by an equal and opposite induction. I proposed a series of systematic experiments.

I am informed that, with the co-operation of the Post Office, a number of earths were tried. A telephone line to the top of Signal Hill, a telegraph wire run down to the sea at Sea Point and terminating in an earth plate in the sea, an earth at Observatory (three and a half miles from Cape Town), an earth plate in the Alfred Docks, an earth at Durban Road (12 miles from Cape Town along the main line of the railway), an earth plate near the cable hut, an earth in the garden of the Standard Bank, and the rails of the tramway, were all tried alone, and in parallel with the sheath of the cable. No reduction of the disturbance was found, and, on the whole, the ordinary earth on the sheath gave the best results.

On August 26th I was invited to take part in the experiments by Mr. T. Cassidy, superintendent of the cable company at Cape

Town. Before dealing with the experiments I will briefly describe the nature of the disturbances. Mr. Trotter.

The cable was sometimes connected to Mossamedes. I am informed that the length is 1,383·526 knots, and the resistance 8,820 ohms. It was sometimes put through to Loanda, 530 knots up the coast. It is worked with 35 cells and a condenser of 60 microfarads. The resistance of the coil of the recorder is 500 ohms. In sending, it is shunted with 1 ohm. No disturbances are perceived during sending. In receiving it is sometimes shunted with 6,000 ohms.

As this is a matter which concerns tramway engineers as much as cable men, and as the former have little opportunity of acquainting themselves with the work of the latter, it may be worth remarking that such a cable as this one is always worked through condensers, for the purpose of cutting off earth currents. A paper tape 16 mm. wide is drawn at about 10 mm. per second under a siphon which is kept in motion by an electric vibrator. It is worth noting, for examination of the details of the disturbances, that the vibrator in the present case gave about 32 dots per second.

The "kicks" are of three kinds. The first—the starting and stopping kicks—are shown in Fig. 2. A starting kick for a car between the works and Cape Town resembles the signal for an N. The first swing is below the line, and falls to about 2·5 to 3 mm. in about 0·2 of a second. This delay is due to the inertia of the working parts of the instrument. This swing is followed by a swing above the line. This is simply a mechanical overshoot of the instrument. It is generally about three-fourths of the amplitude of the swing. A third and even fourth swing is sometimes seen. Such a kick is due to the switching on of about 20 amperes by putting a controller on a car to the first notch. A stopping kick is reversed, and resembles the signal for an A. Its amplitude depends on the current which happened to be passing at the moment of breaking.

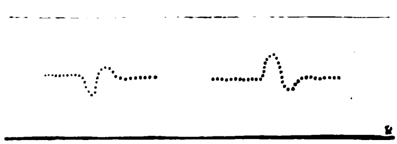


Fig. 2.—Starting and Stopping Kicks.

Trotter.

A second kind of disturbance is shown in Fig. 3, and may be called a "splash." The siphon is often thrown right off the paper, but the instrument has never been damaged. A disturbance of this kind is comparatively rare. So far as I have observed, it always begins downwards. It is therefore probable that it is due to the sudden switching on of a considerable current. It is more or less sharp-pointed, and the rebound is sometimes greater than the first swing; both of these features are, I believe, mechanical.

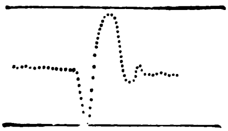


FIG. 3.—"Splash."

The third kind of disturbance is a comparatively small and irregular quiver of the line, and is probably due to the jumping of the trolley at insulators and points, and perhaps to sand on the rails.

In order to form some idea of the magnitude of the disturbing currents in the cable, the siphon recorder was, at my request, disconnected entirely from the lines, condensers, and instruments. I then connected a Clark cell through 100,000 ohms and applied it direct to the instrument, under its ordinary conditions of shunt and control. Quickly snapping the contact gave a kick closely resembling a starting kick. The first swing was 2 mm. Holding down the contact, the line settled down, after one or two small oscillations, with a permanent displacement of 1-7th mm. (Fig. 4).

As the various earths had failed to reduce the disturbances, and as there was no cable ship at Cape Town to lay an earth out to sea, I attempted to apply an equal and opposite induction. I arranged with the Post Office for the use of a private telephone line from Cape Town to Mowbray, running alongside the tramway for two and a half miles, and a distance of about one and a quarter miles beyond the terminus. This line was almost useless to the subscriber, on account of the disturbances caused by the tramway. On August 27th this line was earthed at Mowbray, and was connected through the usual condenser of 60 microfarads with the siphon recorder at the cable office, and was earthed on the sheath of the cable. At the same time another recorder at the cable hut was connected, also through the usual condensers,

to the core and sheath of the cable. The instruments were run simultaneously, and the time was marked at every 10 minutes on each slip, and these were afterwards compared. Every disturbance on the cable was clearly reproduced on the telephone line, with an amplitude about five times greater. Not a single kick on the cable failed to be represented also on the telephone line; but many of the smaller ones on the telephone line were not visible on the cable slip, on account of the small amplitude (Fig. 5). A marked difference between the kicks produced by the telephone line and those produced by the cable is worth notice. It may be seen in Fig. 5 that the telephone kicks are always sharp-pointed; a simple kick never has any sign of a rounded top, and there is seldom any trace of a rebound. I had no opportunity of examining this more fully. It did not strike me until later that the absence of any overshoot may be due to the relations of the inductance, capacity, and resistance of the line. At first I thought that the rounded top of the ordinary kick (Fig. 2) was due to the capacity of the cable; but Fig. 4, in which no condenser was employed, shows that this is not the case. The discharge was evidently instantaneous, but the fact that the line does not drop to zero until 0·2 to 0·3 second does not indicate that the current died away at the same rate. If the current died away twice as fast, or even three times as fast, it would probably check the swing in this manner. The metallic resistance of the telephone line, calculated from the gauge of the wire (No. 18 bronze), is 280 ohms, and I understand that the resistance of the whole circuit may be taken at about 800 ohms. I fail to understand how the conditions of the telephone line could produce any appreciable prolongation of the discharge; but it is possible that the overshoot of the kicks on the cable may be due to the fact that the resistance of that part of the cable which is being affected is small, and that the length affected is short. There is no material difference between the recorders. Mr. Trotter

The next day—August 28th—was wet, but no difference was observed in the disturbances. An attempt was made to earth the cable through the telephone line, but, owing to some mistake about the connections of the condenser, the zero wandered. On

Mr. Trotter. August 29th the experiment was repeated, the connections being made as in Fig. 6. The siphon recorder, protected on each side by condensers of 60 microfarads, was connected to the sheath of the cable and to the telephone line, this line being earthed at the far end. The telephone line gave kicks as on the previous occasion. The instrument was then connected to the core of the cable through a condenser in the usual way, and earthed to the sheath. The cable was through to Mossamedes, and the usual kicks were observed. Fig. 7 shows the connections.

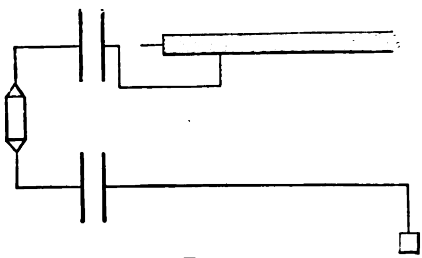


FIG. 6.

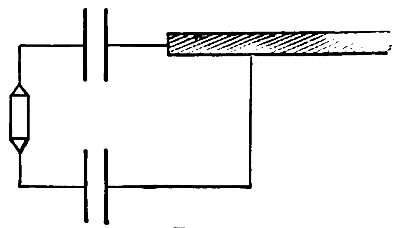


FIG. 7.

An auxiliary and disturbed earth was then connected by putting the telephone wire to the sheath through a resistance. A rheostat was also placed in circuit with the cable, and these resistances were varied (Fig. 8). In order that the kicks might be comparable, signals were made from the window of the cable office to the driver of any car that happened to be standing at the terminus. The kicks were not materially reduced in amplitude until 100,000 ohms were introduced into the telephone line, or 8,000 into the cable circuit; the latter being, of course, an impracticable condition for working.

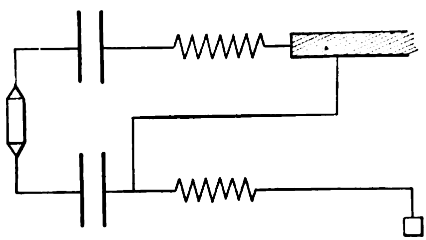


FIG. 8.

In order to study more closely the nature of the disturbances, the tramway companies arranged for a night test, and the programme which was suggested in my report to the Postmaster-

General of August 16th was carried out. On August 31st, at 11.45 p.m., the cable being put through to receive from Loanda, and the tramway service being stopped for the night, a tram-car started from the works to Mowbray. According to a previous arrangement it stopped at Woodstock at 11.55, at Great Moore Street at midnight, and arrived at Mowbray Hill at 12.10. It then returned, and stopped at Salt River at 12.30, and Castle Bridge at 12.40, arriving at the end of Darling Street a few minutes later. The time was marked on the slip at every 10 minutes, and the stoppings and startings were clearly recognisable. Two points were clear—first, that the amplitude of the kicks was in proportion to the distance of the car from the works, but rather greater on the Cape Town side; and, second, that the direction was reversed when the car passed the works. Mr. Trotter.

An important experiment, suggested by Mr. W. B. Rommel, was carried out after this run. A feeder buried in the street was connected to the negative pole of the dynamo at the works, and a branch from this feeder was brought into the cable office. A rheostat was connected, and currents varying from 40 to 100 amperes (at 500 volts) were sent through the completely insulated circuit formed by the feeder and the trolley wire. No kicks whatever were visible, and the line remained perfectly straight. The cable was through to receive from Loanda with the usual connections.

A line was then run out to the rails in the street, and currents were sent by means of the rheostat. They produced violent kicks, a current of 40 amperes sending the siphon off the slip

On September 3rd, as a supplement to Mr. Rommel's experiment, the siphon recorder was, at my request, connected direct to the core and sheath of the cable, without condensers. The cable was through to Mossamedes. The character of the disturbances was somewhat altered; the return was, as might be expected, less prompt, and there was no mechanical rebound. Owing to the inertia of the instrument, the maximum deflection was reached in about 0·3 of a second, and the deflection of 3 mm. died away in about 0·7 second and returned to the zero line (Fig. 9). The zero line without condensers was by no means straight, but it was

Mr. Trotter. sufficiently so to indicate that the disturbances were in all probability due to induction, and not to direct leakage. If time had permitted, experiments without condensers would probably have thrown some light upon the real nature of the disturbances.

On September 5th a telephone line to Salt River, a distance of $2\frac{1}{4}$ miles, and at an average distance of 200 yards from the railway (nowhere less than 100 yards) and about 350 yards from the tramway, was used for an earth. Besides the usual tramway kicks, a continual vibration due to the railway and post office telegraph lines (especially the quadruplex), which run along the railway, was recorded on the tape, but not nearly so remarkable as in the case of the Durban Road earth already alluded to (Fig. 10). I regret that this experiment was not repeated with the condensers removed, in order that the effect of earth currents might be distinguished from induction.

Another attempt was made on September 15th to neutralise the disturbances by means of an equal and opposite induction. Mr. Shaw, of the staff of the "Great Northern" cable ship, was present. The telephone line to Mowbray and the sheath of the cable and the siphon recorder were connected up through a rheostat so that resistance taken out of one earth could be added to the other (Fig. 11). It was expected that a point would be found at which the disturbances would be reduced to zero, and that beyond this they would be reversed, but neither reduction nor reversal could be obtained.

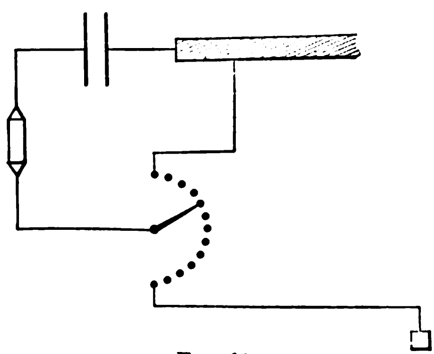


Fig. 11.

On September 15th I made a report to the Postmaster-General upon the results of the experiments, and summarised the five courses which might be adopted. 1st. Make a remote earth connection by means of a telegraph line. (This has been so far unsuccessful.) 2nd. Neutralise. (This has also been unsuccessful.) 3rd. Prevent

all leakage. (This is impracticable.) 4th. Reduce abruptness of change of current. Recommended if Nos. 1 and 2 fail. 5th. A last resort, but a sure one: lay a few miles of new two-core cable across the bay. Mr. Trotter.

Mr. N. Shaw continued to make experiments, at which I was not present. I understand they were not successful. On October 1st the tramway was opened to Sea Point (see map), and the increased traffic resulted in a marked increase in the number and in the amplitude of the disturbances. The operators had begun to read the signals in spite of the kicks, but after this date the signals were almost always illegible while the trams were running. In order to try a still more remote earth, a military telegraph line was run, under the superintendence of Captain O'Meara, along the shore of the bay to the point marked A on Fig. 1, and was terminated there with a copper plate in the sea. The result is shown in Fig. 12. On October 3rd the disturbances appeared to be reduced, but on October 5th—a public holiday in Cape Town—they were as bad as ever. It has been noticed that the amplitude of the kicks is much greater on some days than on others; but this variation has not been traced to the changes of weather, and may be due to nothing more than heavy traffic. Other experiments were made with a differential choking coil by the cable company's staff, and earths at the point A, and at Durban Road (12 miles from Cape Town along the railway), were tried, both separately and in shunt to the sheath, but without any promise of success. A sample of the slip taken at one of these experiments is shown in Fig. 13.

Early in October the cable was duplexed at the station; but no appreciable difference was noticed until, as I am informed, at the suggestion of Mr. H. A. C. Saunders, on the 24th December, it was put on duplex connections with the artificial line at the cable house, as shown in Fig. 13A, from which date until the 14th January the whole of the traffic was carried in a satisfactory manner during the interruption of the alternative route on the East Coast. On the restoration of the East Coast route the company discontinued this plan of working, as it was only a makeshift and very inconvenient. The cable was not actually under

Mr. Trotter. duplex conditions—that is, the company could not work duplex—the artificial line being only used as an adjustment to counterbalance the disturbance on the cable.

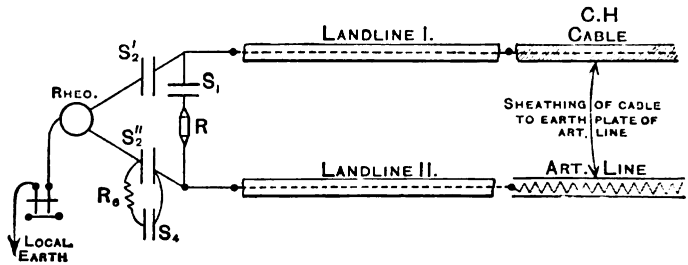


FIG. 13A.

The only remaining method was to lay a length of new cable. Three ways of employing such a cable present themselves. First, the old cable might be picked up and landed at a hut on the other side of the bay, and two cables or a two-core cable laid from that hut to Cape Town. Second, a two-core cable might be jointed to the old cable at some 5 or 6 knots distant from Cape Town, one core being spliced to the core of the old cable, and the other being connected to the sheath. The third way is simply to lay a cable over the old one, terminating in an earth plate or joining the core to the sheath.

No steps could be taken until the "Great Northern" arrived at Cape Town. The cable was laid under the superintendence of Mr. Harold W. Ansell on January 20th, 1897. The end had been sealed. The sheath and core were connected to the instrument at the cable hut, and I am informed that kicks in all respects similar to those of the old cable were recorded. An attempt was made to neutralise or reverse the disturbances in the old cable by means of those in the new one, but without success. On January 22nd an earth plate was attached to the end of the new cable, and, to the great satisfaction of those who were present, a nearly continuous line was drawn by the siphon (Fig. 14). A few kicks of an amplitude of rather less than 1 millimetre were visible. Later in the day the earth plate was removed and the core was soldered to the sheath, and the end was sealed to keep the sea water from

the soldered joint. This arrangement is shown in Fig. 14A. The length of the new cable is about 5 knots, and it was laid as Mr. Trotter.

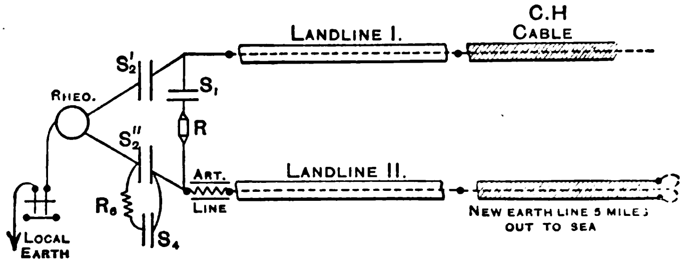


FIG. 14A.

nearly as possible over the old one. Since that date the traffic has been resumed, and no appreciable disturbances have been noticed.

At the first series of experiments Mr. T. Cassidy, Mr. H. Spetch, and one or more of the Cape Town staff of the cable company were present; together with Mr. W. B. Rommel, engineer of the Cape Town tramway companies; Mr. E. G. Jones, late electrical engineer of the Cape Town Corporation, and now resident engineer of the tramway companies; Mr. B. Bayly, Metropolitan District engineer and surveyor of the Post Office; and myself. On the last occasion Mr. A. Worswick, engineer of the Port Elizabeth tramway, was present, Mr. Rommel being away.

In the course of these experiments enough theories have been propounded, and cures proposed, to fill a volume. To non-cable electricians it is not easy to see why such disturbances should not be cancelled by means of an equal and opposite induction. There appears to be no difficulty in getting an opposite one, and it would seem that by shunts and resistances it could be made equal. The difficulty seems to have been that, while it was possible to produce differences of potential, there was no point within several miles as a *point d'appui* where the potential was fixed. The distributed character of the induction is such that no useful effect could be produced by a small current shunted from the main current leaving the station. The sensitiveness of the

Mr. Trotter. siphon recorder to neighbouring telegraph currents introduced further difficulties when a good earth was sought. Many other very interesting investigations might have been made had time permitted, such as the distribution of the current which caused the induction, especially out at sea.

Surprise has been expressed in Cape Town that the tramway companies should "allow so much current to run to waste into "the sea that some of it gets into the submarine cable." A sufficient answer to this is that the current causing the disturbance is about a twenty-millionth part of the average current generated at the works. But, seriously, the magnitude of the primary current which induces the secondary current in the cable is very difficult to estimate. The 2 or 3 amperes that pass through the earth plates when about 200 amperes are being generated probably play only a small part.

Although at the present date, owing to the increased traffic, the fall of volts on the rails from Sea Point to the works is rather serious, at the time of the first experiments when only five or six cars were running it was well under 1 volt; and nothing more could be asked from a tramway company using their rails as a return. At one time I thought that, if the starting current could be switched on "by some efficient method of gradually varying "resistance," say in five steps of 4 amperes, succeeding each other at intervals of not less than one-fifth of a second, the five little kicks would be too small to interfere with the signals, and each would have time to die away before the next began. A worm-wheel on the controller would prevent the driver switching on too fast. But in mounting a hill a well-loaded car may take from 80 to 100 amperes, and to break this in 4-ampere steps at intervals of a fifth of a second would occupy from two to two and a quarter seconds. Since it is necessary that the reversing switch of a controller should be locked until all the current is cut off, such an arrangement would materially delay an emergency stop. If the current to be broken were always the same, it could be switched off from the motor to a resistance and then gradually reduced by the controller. But the varying current renders this impracticable. It is not worth while to discuss other details of

methods of preventing the disturbances, since it would be always more satisfactory, and generally cheaper for a tramway company, to lay a short length of cable, as has been done at Cape Town. Mr. Trotter.

The causes of the disturbances which have been experienced in this case are very different from those which affect telephones. The attention of the Joint Committee of the House of Lords and of the House of Commons which was appointed in 1893 to consider and report whether the grant of statutory powers to use electricity ought to be qualified by any prohibition or restriction as to earth return currents, or by any provisions as to leakage, induction, or similar matters, was confined to telephone and railway signal interferences. It is hardly necessary to remark that the rapidly fluctuating character of the current produced by the commutator of the motor, which is the chief cause of the trouble with telephones, would have no effect on a siphon recorder; nor would a strong continuous current, such as those which often get astray in Cape Town, dropping the indicators at the telephone exchange a dozen or two at a time, affect an instrument protected by a condenser. The sparking of the trolley at insulators is sometimes audible in a telephone, and the roar, as distinguished from the hum, is probably due to a dirty track. These are matters of small moment as far as a submarine cable is concerned. I have carefully listened for a click or other sudden sound in a telephone at the moment a hum begins or ends. The click, if any, at the beginning is very insignificant, and I have never heard any click whatever when the hum stops.

Figures 15, 16, and 17 are specimens of slip. Fig. 15 was recorded before the new cable was laid; the signals are unreadable owing to the tramway disturbances. Fig. 16 gives the words "Limited Cape Town Station," the recorder being earthed to the sheath of the old cable. The disturbances happen to be very slight. Fig. 17 gives the same words with the recorder earthed on the new cable.

In conclusion, I have to thank the Eastern and South African Telegraph Company for inviting me to take part in the experiments. I hope that this paper will be supplemented by an account of the experiments at which I was not present.

The PRESIDENT: At this late hour it will be impossible to commence the discussion on this important paper, and it must therefore be postponed until the next meeting, on the 20th inst.

I have to announce that the scrutineers report the following candidates to have been duly elected, viz.:—

Members:

William Worby Beaumont. | Edm. Arthur Norman Pochin.

Associates:

Arthur James Brooks. | George William Cooper.
Arthur William Higgs.

The Three Hundred and Third Ordinary General Meeting of the Institution was held at the Society of Arts, John Street, Adelphi, W.C., on Thursday evening, May 20th, 1897—Sir HENRY MANCE, C.I.E., President, in the Chair.

The minutes of the Ordinary General Meeting held on Thursday, May 13th, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Students to that of Associates—

Frederick Simmons Grogan. | Edgar Julius Sander.
Martin Jeffries Scears.

Mr. A. T. Snell and Mr. Raphael were appointed scrutineers of the ballot for new members.

The PRESIDENT: We will now commence the discussion on Mr. Trotter's paper read at the last meeting.

Mr. CHAS. BRIGHT: I think we must all agree in feeling very much indebted to Mr. Trotter for this paper, which cannot fail to be equally valuable both to the cable man and to the electric traction engineer. It seems to me that the paper is more especially praiseworthy and useful in giving us a wide range of experiments, the majority of which—though hopeful in character—more or less failed to produce the desired result. The publication of successful experiments is common enough, but those which do not happen to succeed are rarely made known, though perhaps of equal, if not greater, interest and value. Mr. Bright.

It seems abundantly evident that the cause of the kicks on the siphon recorder in this case was due, not to direct leakage,

Mr. Bright. but rather to the difference in the inductive effects produced by the primary current from the tramway circuit upon the two parallel conductors running between the cable hut and station This being so, it appears to me almost too much to expect that any particularly useful result could be attained by varying the position of the earth—the latter being common to both ;* in fact, I venture to think that the only really effective cure would naturally be of a differential, or balancing, character.

I am sorry to see that the attempt shown in Fig. 13A of balancing the extra induction in the one underground cable by adding an artificial line to the other, was not capable of being turned to successful account in practice by perfection of duplex balance at the cable hut. Of course, in theory, this was absolutely perfect, for it was precisely the same electrically as extending the cable to which it was connected. But in practice one can easily see that there would be considerable difficulty in altering the adjustment at a point so far from the recorder station. It is also to be regretted that alternative artificial methods other than the somewhat brutal and costly plan of establishing a metallic circuit could not have either been discovered or found successful.

However, from the cable company's point of view, no doubt the means by which the desired end was attained was immaterial, for it is assumed that the tramway people would have to pay the bill.

The final resort adopted with success may be looked upon, I presume, as a modification of a complete metallic circuit, in that it was a case of laying a cable to such a distance as was found in practice to sufficiently balance the inductive effect on the other line.

The moral of the paper seems to be that people proposing to

* In reference to the introduction of induced currents in the case of earth circuits, it may be interesting to note that in the year 1856 the late Mr. Samuel Statham and Dr. Wildman Whitehouse actually secured provisional protection (Specification No. 1726 of that year) for the use of a return wire, though this was the original custom until Steinheil put Dr. Watson's "earth return" into practice! No doubt Messrs. Statham and Whitehouse had these inductive influences (as well as earth currents) in mind when they proposed abandoning the earth as a "return."

establish tramway systems would be well advised in giving cable stations plenty of elbow-room – especially in view of the increasing application of high-speed automatic working, for it is here, of course, that these inductive effects, with small signals, become most serious. Mr. Bright.

I notice, by the way, that Mr. Trotter speaks of working condensers as being used "for the purpose of cutting off earth "currents." Perhaps it would be well also to remember that an additional service rendered by them is that of sharpening the signals, thus permitting of an increased speed.

Papers of this sort are particularly rare from people holding official positions in the Colonies, and I think we ought to be all the more grateful to Mr. Trotter for having given us this paper, as well as to the Eastern and South African Telegraph Company and staff for offering facilities for the experiments, and for their publication. This paper may, indeed, have the salutary effect of inducing others in various cable stations at all quarters of the globe to give us their experiences.

Mr. W. H. PREECE: Papers of this kind are very difficult to discuss, but they are of extreme value to the Institution, because they give us what we so very rarely get—the experience of those who are practically at the other side of the world. One reason why we have had so few papers of this class is that in the past the rivalry of competing companies instilled a policy of silence or secrecy amongst the powers that be, preventing their staff from sending papers to the technical Press, and also to this Institution. While we certainly are very much indebted to Mr. Trotter for the paper itself, we are also indebted to the managers of the South African Cable Company, who have allowed the facts to be published in our Proceedings. We cannot do too much to encourage such papers. We all know that we suffer from disturbances of this kind very much in England. In the Post Office we suffer more than most people; and it is not very long ago—I think the last meeting but one—when I had to recount to the Institution an experience of our own in the South of London. We have had similar disturbances in Liverpool and in Leeds, and, in fact, wherever electrical tramways, telephones, and telegraphs, submarine or overland, exist together. Mr. Preece.

Mr. Preece. In such cases, unless the lessons of experience are followed, we are bound to experience a certain amount of inconvenience. But there is this great comfort—that, however serious these disturbances may be, they are remediable. If there is a disturbance, it is due to some defect existing in the circuit, or some error of the judgment of those who laid down these different circuits. The difficulty is to determine, as Mr. Bright has suggested, where induction comes in and where leakage is found. There is, however, never any difficulty in determining what is induction and what is leakage; for the effects of induction are only momentary, while those of leakage are continuous. The principal disturbance we have in London is this continuous disturbance due to leakage; and now, with the experience in America, we have found the modes by which the troubles of leakage are practically limited. In this particular instance it is practically impossible for the tramway company to eliminate the cause of disturbance, and therefore it remained for the cable company to adopt the plan described by Mr. Trotter. One was glad to learn that the results were satisfactory. That, however, is not the only instance. Mr. Luke has just returned from India, and he will be able to give us his experience of a similar defect in Madras.

Mr. Luke. Mr. P. V. LUKE: I have not had the advantage of hearing Mr. Trotter's paper, but I judge from the diagram, Fig. G, which I see on the wall, that he has made some reference to the Madras case. That diagram exactly shows what happened at Madras. The cable is represented in red.

Mr. GADSBY: I may mention that that is a diagram which I have prepared, and I intend to say something about it.

Mr. LUKE: I have not much to say upon the subject, except that there was a disturbance felt on the Madras-Penang cables, and that various means were tried to obviate it. First of all a line terminating in an earth plate was run out into the sea at right angles to the line from the cable house to the telegraph office, and next an earth line was run up along the shore in continuation of the line from the cable house to the office. These means were ineffectual, and it was found that nothing really removed the disturbance until a highly insulated line was made

from the telegraph office to the cable house, and there connected to the cable guards—thus making a metallic circuit over the section affected by the disturbing currents. The main point which may be of interest to many here, for I think it is rather a controversial matter, is that the tramway company had to reimburse the Eastern Extension Cable Company for the cost of making this alteration. The telephone lines also were affected by the tramway, and the tramway company have had to pay the cost of certain alterations which the telephone company had to make to their lines in order to avoid the inductive disturbances from the tramway currents. I do not think there is anything of interest that I can add, as I did not hear Mr. Trotter's paper. Mr. Luke.

The PRESIDENT; How far did they carry out the earth plate to sea?

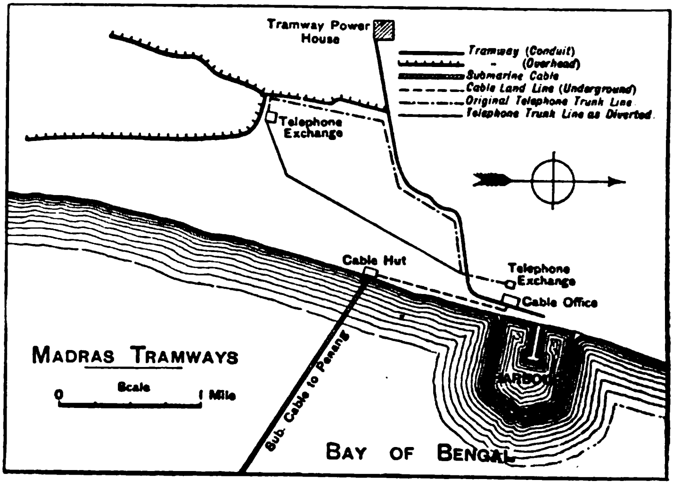
Mr. LUKE: They carried it out as far as they could go within the limits of the harbour—probably half a mile.

The PRESIDENT: Was it on an insulated core?

Mr. LUKE: Yes, by an insulated core which terminated in a large copper earth plate. The insulated line along the coast was about 15 miles long, with an earth plate buried at the end. Neither of these remedial measures was effectual.

Mr. C. H. GADSBY: I have been very much interested in Mr. Trotter's paper, on account of having had similar experience myself in Madras. Whilst I was in charge of the Madras electric tramways, from December, 1894, to April, 1896, we had considerable trouble both with the telephones and with the submarine cable which is landed at Madras, and belongs to the Eastern Extension and Australasian Company, coming across from Penang, and which is, I think, about 1,500 miles long. We carried out a series of experiments there, and I thought we had determined that the interference with the telephones was due to induction, and that in the case of the submarine cable it was due to leakage. I thought the difficulty had been overcome by carrying an insulated wire from the siphon recorder to the hut where the cable is landed—about a mile along the shore—and making the earth on the sheath of the cable. This was suggested before I left Madras, and it was anticipated that it would overcome Mr. Gadsby.

Mr. Gadsby. the difficulty. After I left the work was carried out, and I understood that it had been successful; but before coming here this evening I saw Mr. Saunders, of the Eastern Extension Telegraph Company, in order to make sure of my facts, and he tells me that further disturbances have quite recently been reported; so that the difficulty cannot be regarded as having been overcome. The diagram shows the configuration of the Madras tramways, which explains itself, and indicates how a diversion of the telephone trunk line got over the difficulty with



the telephones. A part of the tramway was a conduit system, with a single conductor with an iron-lined conduit, with the rails as a return. The other portions were on the overhead system, with a side trolley wire and the rail return. I will not trouble you with any details of the experiments. I have read Mr. Trotter's paper through several times, and have tried to formulate a theory on the basis of the experiments there recorded; but they appear to be of a very conflicting nature. One seems to show conclusively that the interference was due to induction; the next one appears to show almost as conclusively that it was due to direct leakage. I venture to suggest that it was not due strictly to either of these, but that it is a condenser effect. In the early days of electric traction we regarded the earth as a conductor

with ample conductivity for returning the current to the power station. We soon found that we were wrong in that respect. Cable electricians, and others familiar with the operation of very small currents only, have assumed that all points of the earth's surface are at zero-potential. I venture to think it is possible that, by sending into the earth's surface at any point a large charge of electricity at a few volts higher potential than the earth's surface, the potential of the surface at that point may be raised instantaneously, and that on any neighbouring cable we may get condenser effects in the following manner:—The part of the sheath of the cable in the locality of the disturbance will have its potential slightly raised, with the result that there will be set up a wave of redistribution of the charge on the cable core, that end within the part of the sheath whose potential is raised falling below the normal zero-potential. The attached condenser will increase this effect, and there will be a wave of current through the siphon recorder and a kick. This, I think, is borne out by a rather important fact mentioned in Mr. Trotter's paper. At one part of the paper we are told that when the new cable was laid out in the bay the end was sealed, and the siphon recorder was put between the shore ends of the new sheath and core, and that whilst the end at sea was sealed, and had the core still insulated from the sheath, disturbances were taking place through the siphon recorder, due to the tramway working. This can be explained either on the basis I have just mentioned, or on the basis of an insulated conductor partly enclosed in a conducting sheath—that is, with the one end in and the other out. On this latter basis, if the sheath were raised in potential, there would be a redistribution of the charge on the partially enclosed conductor, and the part in the sheath would be lowered in potential whilst the part outside the sheath would be raised in potential, and there would be a similar flow of current through the siphon recorder. I make this suggestion although I can see that there are some of the observed results which are not easily explained on this basis, but I hope somebody may be able to throw more light on the view of this matter. Mr. Gadsby.

Professor AYRTON: With reference to the remark made by Prof. Ayrton.

Prof. Ayrton.

the last speaker, I think he will find that the explanation he has given really resolves itself only into another way of expressing leakage. For what do we mean by a disturbance being produced in a telegraph line by leakage as distinct from induction? Why, that a current has been caused to flow in the telegraph line, not by an action, apparently at a distance, between it and some other wire, but by an action which is analogous with that caused by the ordinary signalling battery. Now in a submarine cable worked with condensers the ordinary signal is produced by the signalling battery causing an alteration in the potential of the near coating of the condenser; hence, if the disturbance due to the tramway arises from the potential of the earth at the cable station being altered by the tramway, such an effect, if we wish to be consistent, must be classed under a leakage disturbance.

I do not think people realise the extent of the magnetic disturbance which may be produced by an electric tramway. Mr. Preece has said that the greatest difficulty in this country arising from electric lines was leakage. I have put on the table a number of curves recording the results of a magnetic survey that was made by Professors Rücker, Boys, and myself in the early part of 1893 over the whole neighbourhood of the City and South London Electric Railway, which runs underground between London Bridge and Stockwell. It was not merely along the line, but for distances up to two hundred yards on each side. All these great variations are undoubtedly of an inductive character: that is to say, our instrument was simply a suspended magnet; it was not a galvanometer, and had no wires connected with the earth. There was no question, therefore, of electricity leaking into the instrument itself. It was simply a suspended magnet—a magnetometer, in fact—used to determine the disturbances of the earth's magnetic field throughout the whole region in the neighbourhood of the City and South London line, which were caused either by magnets or masses of iron in the passing trains, or by currents passing through the earth. These particular curves have not been shown before, and I thought you might be interested in seeing them.

I select the following three diagrams as typical of the rest, for

Prof. Ayrton.

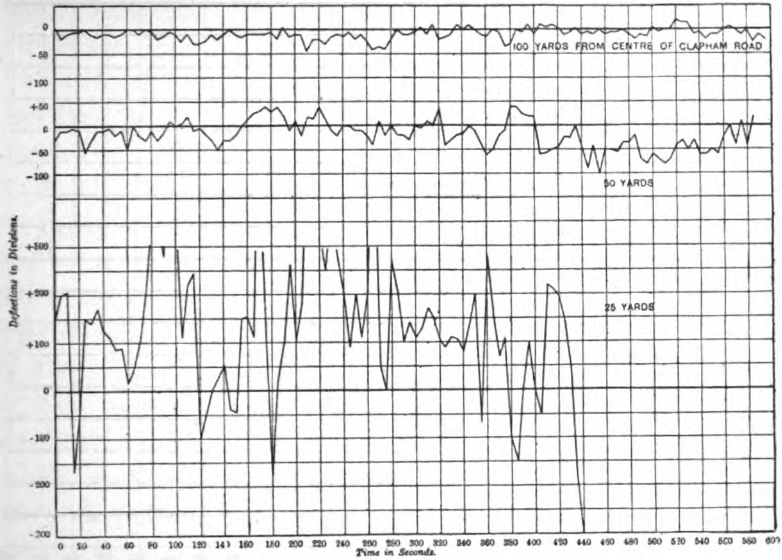


DIAGRAM 1.—Observation taken in Portland Place, Clapham. March 8th, 1893. Magnetometer, E. and W.

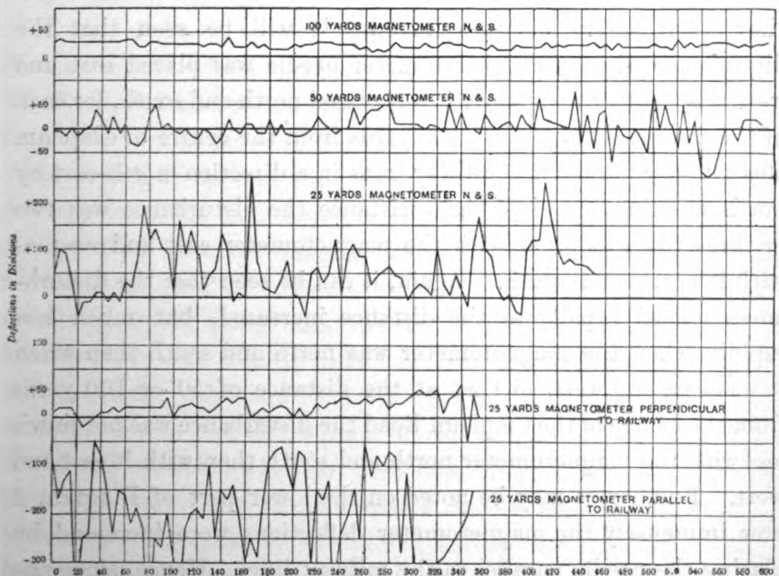


DIAGRAM 2.—Portland Place, Clapham. Distance taken from centre of Clapham Road. March 8th, 1893.

Prof. Ayrton.

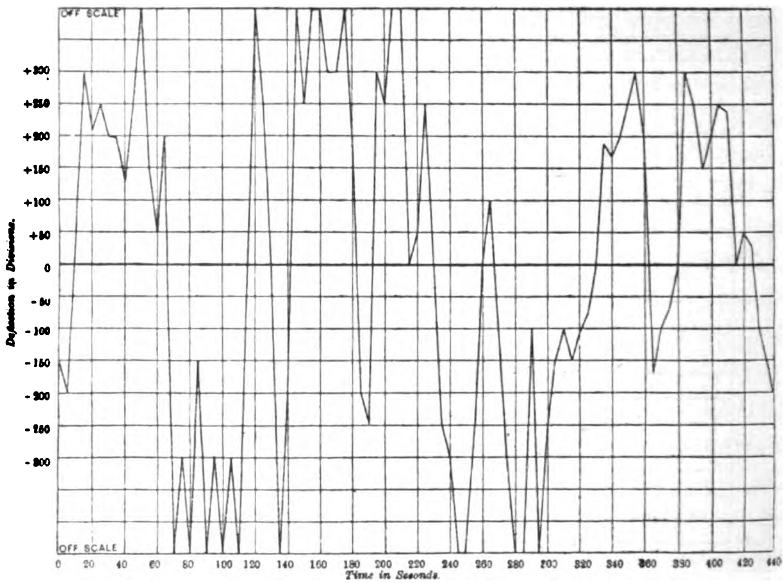


Diagram 3.—Taken in Princes Square, 25 yards from the centre of Kennington Park Road. March 3rd, 1893. Magnetometer, E. and W.

they show several things. Firstly, it will be seen that the disturbance when the magnetometer needle was placed east and west was greater than when it was placed north and south, for each of the distances 25, 50, and 100 yards from the centre of Clapham Road, under which the railway runs in a direction north-east by north, and that at the 25 yards distance the disturbance was two or three times as great with the magnetometer east and west as with it north and south. Again, it can be seen that the disturbances fell off rapidly as the distance increased, but much less rapidly when the magnetometer was north and south than when it was east and west, so that at the distance of 50 or 100 yards from the centre of the Clapham Road the disturbance was not much less with the magnetometer north and south than with it east and west. Further, it may be noted on the lower part of Diagram 2 how immensely the magnetometer deflections were increased by placing the needle parallel to the railway line. Hence, since the railway runs north of north-east, it might be expected that the disturbance when the needle was north and south would have

been greater than when it was east and west. The apparent discrepancy is explained, however, by the fact that "magnetometer "north and south" means that the needle was in the magnetic meridian, which is some 17 degrees west of the geographical north. Prof. Ayrton.

Lastly, by comparing the curves for the 25 feet distance on Diagrams 1 and 2 with that on Diagram 3, we see that the magnetic effects were totally different in magnitude at different parts of the line, being many times as great in Princes Square, for example, at 25 yards from the centre of the road as they were in Portland Place at about the same distance from the line of route.

Now as to the cause of these magnetic disturbances? The rapidity of the change in the deflection of the magnetometer needle, combined with the great difference in the magnitude of the effects at different places about equi-distant from the railway, would seem to indicate that the disturbance was not due to the direct action of the passing trains, but to the currents returning through the earth under the magnetometer and being diverted by the gas and water pipes.

It was partly on the strength of these experiments that the electric railway which it was proposed should run under Exhibition Road did not obtain the consent of Parliament, in spite of the very powerful support that it received in high quarters. Of course it might be said that the instrument which we employed was very sensitive, and therefore that it would detect anything. As a matter of fact, the magnetometer which we used was taken about in a cab, and, although we did not make observations while the cab was going along, because there would have been too much vibration, we were able to take readings very shortly after each stoppage of the cab; and to prove that these deflections of the needle were not due to vibration of the ground, I may mention that on one occasion, when we were observing, there was a period of absolute rest: the magnetometer needle became absolutely stationary. Of course we instantly recorded the time when this happened, as well as the minute when, later on, the needle started deflecting again from one side of the scale to the other. We had assistants at the railway station timing the passage of the electric

Prof. Ayrton.

trains, and, on comparing these records, it turned out that the period in question was exactly the duration of a stoppage in the traffic. We were able, in fact, with our magnetometer to time with perfect exactness, from a distance, the stoppage of the traffic on the City and South London line. There is no question, therefore, that all these disturbances were purely magnetic, produced partly, it might be, by passing trains, but mainly, I think, by the fact that the return current was wandering all about the neighbourhood, selecting, no doubt, gas pipes and water pipes for its path.

At that time electric railway promoters thought me a very objectionable person, but I am happy to say that they have now come round, and that they are beginning to realise the fact that a person who points out defects may really be their greatest friend. What does the experience at Cape Town mean, which forms the subject of Mr. Trotter's paper? Why, that a submarine telegraph company which was carrying on its daily occupation was suddenly disturbed and interfered with by somebody at a distance starting to carry on its daily occupation; and then the former company was told, "Oh, you must discover some means "for preventing the operations of the new electric tramway "company interfering with you, electrically or magnetically." But surely the law ought to be that the new-comers should not be allowed to interfere with those who were there before.

If you consider what happened at the Cape of Good Hope, you will find that it meant that the earth in the cable station—the ground under the cable station—was not the submarine cable company's own electrically, but belonged to the tramway company, and the cable company was compelled to go five miles out to sea to find an earth, instead of using the one underneath their own premises. I wonder, for instance, if the ground underneath this building really belongs to the Society of Arts, or whether some day it will electrically belong to some "Charing Cross, Temple Bar, and "Bank Electric Traction Corporation," or to some other future electric railway company. The equitable course, obviously, is for a new electric railway not to interfere with the operations of those who were on the spot before, and that a magnetic

interference should be dealt with like an interference with "ancient lights." Prof. Ayrton.

Now, can the magnetic interference of an electric railway be prevented at a cost which is reasonable? and can the means taken to prevent it be rendered otherwise beneficial to the traction company? No electric lighting company in this country, as far as I am aware, employs an uninsulated return. I am not speaking of using the earth itself as the return—I do not mean that the company does not bury two copper plates in the ground and trust to the current going back by the earth—I mean that no electric light company uses a wholly uninsulated metallic conductor for its return. Is it not a fact that all the electric light companies in this country insulate both the positive and negative mains, whether they employ a direct-current or an alternating-current system? One conductor, of course, may be better insulated than the other in the case, for example, of a concentric cable, and when high pressures are used the outer conductor may have its potential kept at zero by an earth connection somewhere, but even then both the conductors are more or less well insulated along their whole length.

It is not, however, with concentric cables that we are mainly concerned in electric traction, since the fact that the moving car must, of necessity, make continuous electric contact with the going and return conductors along the line prevents the use of concentric conductors, except for feeders. In electric traction, then, we have to deal with two separate conductors, and in such a case a completely insulated system is greatly to the advantage of the company. In the first place, the stress on the insulating material if both the conductors are insulated is only half of what it is if one is insulated. Again, if you have both conductors entirely insulated and an earth fault comes on one of them, you do not have a breakdown, for you are then no worse off than you would be if you had an uninsulated return. By having both conductors insulated the danger from shock is also very much lessened. Further, you know that the Board of Trade limits the drop in pressure to 7 volts with an *un*insulated return. So that, if you have an *un*insulated return,

Prof. Ayrton.

if you use the rails of your electric railway or tramway for your return, you are not allowed to have more than 7 volts drop. That would mean with the sort of electric railways we are going to have under London—a three-minute service, with trains each carrying some 300 persons, as on the District line, for example—that it would be necessary to employ very stout rails, very good electric bonds between the rails, and very thick feeders going to many points of these rails; it would mean, in other words, that the return conductor must have an extremely large cross section. Now it is merely a question of calculation of pounds, shillings, and pence to prove to the proposed electric railway companies that on the whole they will gain by having an insulated return, which may have a much smaller cross section, since the regulation about the 7 volts drop would not then have to be considered.

I am happy to say that the three important companies that have had Bills before Parliament this spring, for obtaining sanction to construct the "City and West End" line, to run between Hammersmith and the City, the "Brompton and Piccadilly Circus" line, which is to run from South Kensington to Piccadilly Circus, and the Bill for boring a deep tunnel under the present District Railway, through which express trains will be run electrically, as well as for converting into an electric railway that portion of the Inner Circle that belongs to the District Railway Company,—I am happy to say that the engineers of these three companies have been sufficiently convinced for the promoters in each case to have given a written undertaking that a completely insulated system shall be employed; so that, whether these lines be worked on the two- or the three-wire system, whether the current be direct or alternating, there are clauses in the Bills guaranteeing that the return shall be insulated throughout to the satisfaction of the City and Guilds of London Institute, and of the Science and Art Department. That, I think, is a great step in advance, not merely in the interests of science, but in the interests of the electric railways themselves.

Of course it is very difficult, when you represent a scientific college, to make people understand that you may be just

as interested—not necessarily in the pecuniary sense—but really just as interested in the success of electric railways and tramways as you are in the success of the experiments carried out by students in your laboratories. But whether, as I sincerely trust, I have succeeded in convincing the engineers of these proposed underground electric railways that, as a Professor of Electrical Engineering, the interests of the railways and of my students are really identical, the fact remains that the electric system to be carried out is one that will free the companies from trouble following from interference with telegraph lines, interference with telephone lines, damage to pipes through electrolysis, will diminish the risk of breakdowns, the risk of *employés* receiving shocks, and last—and I hope you will think by no means least—will free the companies from the onus of stopping the work now carried out in electrical laboratories even when 100 or 200 yards from the proposed electric railway, and thus debarring students, some of whom will devote their lives to electric traction, from obtaining that scientific training which is essential for their professional success. Prof. Ayrton.

Mr. GADSBY: I should like to mention one point I omitted, viz., that the telephone system in Madras is worked with an earth return The diversion of the line overcame the difficulty, so that the leakage would not seem to have so great an effect as Professor Ayrton seems to suppose. Mr. Gadsby.

Professor GEORGE FORBES [*communicated*]: I am surprised that the author has not made reference to what has been successfully accomplished in a similar case at Coney Island, New York, by Mr. Cuttriss, of the Commercial Cable Company. I suggest that a copy of the paper should be sent to Mr. Cuttriss, with a request that he should add a note containing his own experiences and his method for overcoming the difficulty. Prof. Forbes.

In the case referred to the cable from Canso, Newfoundland, is landed at Coney Island, but not earthed. An overhead wire leads to the New York office and back to Coney Island, is connected to a submarine cable, and earthed about a mile out at sea.

It was found when electric tramways were introduced that so soon as a car crossed the bridge on to Coney Island the marks on

Prof. Forbes. the recorder became confused or unintelligible. It was this defect which was overcome by Mr. Cuttriss.*

Mr. Winter. Mr. CHAS. E. WINTER [*communicated*]: I think it will be interesting in connection with this paper to state that some 28 years ago the cable instruments belonging to the French Atlantic Telegraph Cable Company at the island of St. Pierre (which is of a very rocky nature) were so much interrupted by the working of an ordinary Morse telegraph instrument, belonging to another telegraph company in that island, that the French Atlantic Company were obliged to make an independent earth at sea, after which the working of the instruments was quite satisfactory. Before this independent sea earth was made, I believe the French Atlantic Company were able to decipher on their own instruments the messages sent by the other telegraph company. I happened to be doing some electrical work in connection with the French Atlantic cable about this time, under the direction of the late Mr. Hockin, and well remember the circumstance.

The President. The PRESIDENT: As the author of the paper is at the Cape, we are deprived of the pleasure of listening to his reply to the remarks that have been made. I appreciate the value of Mr. Trotter's contribution to our stock of knowledge on this subject, as I had officially before me the particulars of the disturbances which were caused not long ago by the electric tramways in Madras.

Professor Ayrton has expressed himself to the effect that the tramways have no right to interfere with the telegraphs; now, although I am a telegraph man myself, and although to some

* Acting on Professor Forbes's suggestion, the Editor applied at once to Mr. Cuttriss, who replied that pressure of business would render it impossible for him to furnish the information asked for in time for insertion in this number of the *Journal*, but kindly promised to send later on a description of what was done to overcome the trouble referred to. He states that "the methods employed are "radically different from those treated of in Mr. Trotter's paper, as we have to "deal with fluctuations of current-strength far beyond anything that he speaks "of. The rails are within four feet of our cable for a distance of some three "miles, and in winter time the sleet forms on the trolley wire and adheres to "the rails, so that full fluctuations of the current from zero to its maximum "occur very rapidly."—Ed.

The President.

extent I agree with Professor Ayrton, I do not hold quite such decided views. The submarine telegraph has practically a monopoly of the ocean, and I think it is a little too much to ask that they should also have the monopoly of the land. The roads were made for traffic, and I think each party should take reasonable precautions to protect themselves. The conditions at Madras were somewhat different from those at the Cape, and it might easily follow that the disturbances were due to different causes. At Madras the underground cables connecting the telegraph office and cable house were laid parallel with, and adjacent to, the tramway for a distance of more than a mile. When the two spare underground cables were used for the purpose of obtaining sea earths the interference ceased. The disturbances in this case were, in my opinion, due entirely to induction, the effect of which was counterbalanced by using the underground cables as earth connection. I am more inclined to this opinion as we are told a sea earth 700 yards distant, in front of the office, was tried, without effecting any improvement; and I do not think there would be any material difference between an earth obtained at the cable house a mile distant, and one obtained in the sea 700 yards away in another direction. Mention has been made of an attempt to solve the difficulty by the use of an inland earth 10 miles distant; but this experiment is not at all conclusive, as if that earth had the usual amount of resistance in it the recorder instrument might easily be affected by the working of instruments at some of the land-line stations, or it is quite possible, by means of a second wire, if any existed, that indirect communication may have been established back to the earth plate at Madras. I have often observed a delicate testing galvanometer disturbed by currents which must have originated 100 miles away.

The interference with the working at the Cape appears to me to have been due to inefficient earth—that is to say, one which might have been good enough for ordinary purposes, but not sufficiently distant to escape the stray currents from the tramway. The plan of obtaining an earth some miles out to sea was recommended by Cromwell Varley nearly 40 years ago; there is,

The President. therefore, nothing unreasonable in asking a cable company to adopt this precaution when necessary, though I should have thought a less distance would have sufficed.

The splash in one of the specimen slips exactly resembles the effect which is sometimes noticed after a flash of lightning. I have frequently observed similar disturbances during a thunderstorm, although the latter may have been many miles distant from the station.

As we have another paper to read to-night, I will not detain you with any further remarks, but, in conclusion, ask you to pass a hearty vote of thanks to Mr. Trotter.

The vote was unanimously agreed to.

The following paper was then read:—

ON DYNAMOES.

By W. M. MORDEY, Member.

Mr. Mordey. 1. The development of the dynamo has been very rapid. The subject from the first proved attractive to engineers and scientific men, as well as to commercial people. After quantitative methods were introduced, a few years' work sufficed to bring the machine up to a high degree of excellence, as changes were easily made and their effects readily observed. Rapid progress on sound lines was greatly facilitated by the remarkable paper read by Drs. J. and E. Hopkinson in 1886 before the Royal Society. The paper read in 1893 by W. B. Sayers before this Institution indicated possibilities of progress in a new direction. In spite of this suggestive paper, practice appears to have settled on certain regular lines. For some years little or no advance has taken place, and the dynamo has come to be looked upon as having substantially reached its full development—except, perhaps, as to size—present practice and present proportions being regarded as very unlikely to be departed from.

2. Does it deserve its high reputation? As a working machine it certainly does. The results it gives are excellent. It is cool, reliable, durable, highly efficient, has good regulating qualities, and good collection, even in some cases with fixed brushes and varying load. But these excellent qualities, possessed, at any

rate, by the best machines, are obtained at a high price, by the lavish use of costly materials—at the cost of a large air gap, of excessive excitation, and magnets larger both in length and section than the armature should require. Mr. Mordey.

If designs which require a lavish expenditure of material to attain a desired result are unsatisfactory, then the best modern dynamoes leave plenty of room for improvement. Instead of being regarded as a finished product, they should be considered crude, imperfect, and unsatisfactory.

3. To establish this contention, a statement of one simple fact should suffice. One-tenth of the present exciting power would amply suffice to magnetise the iron of the magnetic system (and a gap sufficient for clearance) to its required value; the other nine-tenths may be looked upon as spent directly or indirectly in overcoming armature reaction.

In attempting to improve dynamoes, our aim should be, while retaining a high standard of practical working, to get more work than at present from a given value of material and labour.

4. Output is limited by sparking, by heating, by questions of regulation, and by strength of the working parts. It is better that the limit should be heating rather than sparking, in order that overloading for short periods may be possible. If a machine sparks much when overloaded, then its normal output cannot well be exceeded; but if it does not spark it may be overloaded for a short time, as it only heats slowly. Too much attention can scarcely be given to this subject, for on cool working the life of the machine mainly depends. It is to be feared that attention has often been given to preventing heat losses by aiming at a high efficiency, and not sufficient attention to getting rid of the heat that is generated. This paper deals especially with conditions bearing on the reduction of material and labour by minimising sparking and armature reaction. But it must not be overlooked that, the more we may succeed in reducing these, the more important becomes the question of cooling—of ventilation, radiation, and conduction—in order that we may safely avail ourselves of any possible gain of output.

5. It may be thought that in the present paper references to

Mr. Mordey. sparking should be condensed, after the fashion of the famous treatise on Snakes in Ireland, into—"There is no sparking in "modern dynamoes." But a natural historian, in explaining the absence of snakes, might perhaps have argued that there were incipient or suppressed or potential snakes, and that their outward and visible manifestation was only prevented by equal and opposite snakes, or equivalent devices. So with dynamoes. Good dynamoes do not spark, or the sparking is negligibly small, or only troublesome under varying loads with fixed brushes. But sparking is kept under by expensive means.

6. A large part of the present waste is surely preventible. That the above estimate of this waste is not excessive may be judged from the consideration of the following imaginary case:—Consider a dynamo designed, so far as the armature is concerned, on ordinary lines, and with a field, in the first place, sufficient to enable full volts to be obtained, but no current; the armature conductors being, however, large enough to carry the full current. Such a machine might have a toothed armature, an air gap only sufficient for clearance,* and a short magnetic circuit. It would require very little excitation. Now attempt to load it—reaction sets in, and to check this the gap is increased; then we must add field winding, add length to the magnet in order to provide space for the increased winding, add section to the field magnet in order to convey the leakage magnetism which results from the increased gap, and so on, cumulatively, till finally we have increased the excitation 30 to 50 times, and made the magnet much larger (in length, in section, and in copper) than when we started. Practically the whole of this increase is a direct result of armature reaction.

* As showing how small an air gap suffices for mechanical purposes, the practice in the case of polyphase motors may be referred to. In these machines large "armature reaction" is essential to their success. The following are the clearances adopted by one maker:—

H.P. of Motor.	Clearance in Millimetres.
1-24th	0·3
5	0·5
30	0·7
100	1·5
300	2·5

7. Let us examine the excitation data of any well-known dynamoes. For example, let us take the Edison-Hopkinson dynamo, fully described in the Hopkinsons' 1886 Royal Society paper, as an example of a smooth core, and the 150-kilowatt "Railway Generator" of the General Electric Company of America, fully described in the 1896 edition of Prof. Thompson's "Dynamo-electric Machinery,"* as a very recent example of a toothed-core machine, based on an extensive experience. Mr. Mordey.

Comparison of Distribution of Excitation.

——	"Railway Generator" 150-K.W. (1896). (6 poles.)		Edison-Hopkinson 34-K.W. (1886). (2 poles.)	
	Per Cent.		Per Cent.	
Air gap	44·39	88·15	80·2	89·8
Teeth	3·06		...	
Compounding	40·70		9·6	
Armature core	0·71	... ...	0·68	
Field cores	4·58	11·04	6·61	9·53
Yoke	6·56		2·92	
	100·00		100·00	
Ampere-turns, total ...	19,630	..	20,156	

The machines are taken as examples mainly because the full information necessary for the present purpose is available. They are admirable machines.

8. An examination of the table reveals some interesting resemblances. If any part of the magnetic circuit of a dynamo may be considered more essential than any other part, the armature core is probably entitled to that distinction.† In the two cases this essential part absorbs respectively 0·68 and 0·71 per

* P. 434, plate xi.

† The armature will work when surrounded by an existing coil, even if the whole of the field-magnet iron is removed.

Mr. Mordey. cent. of the excitation; the other 99·3 per cent. is taken up in getting the magnetism through the rest of the circuit.

And when we compare the initial and the full excitation we see that, although the former is small in the toothed machine, the ultimate result in the two cases is about the same, the large reaction in one being balanced by the large air gap in the other. Peter is robbed to pay Paul.

The inference must not be drawn that there is little to be said for the use of teeth in this instance. On the contrary, the teeth have very great practical advantages, even if the excitation were the same in both cases.

The above comparison is perhaps a little unfair to the English machine, which was a rather small one, and was brought out ten years ago. No doubt the designer would improve somewhat on it now, but it is given exactly as published, and serves very well to show that Dr. Hopkinson raised the dynamo in 1886 to so high a level that subsequent improvement has been comparatively slight, and very difficult; and it must not be forgotten that he started from chaos.

"The Magnetising Space."

Before proceeding further, attention is drawn to the subject of diagrams.

9. The consideration of excitation and armature reaction is simplified by clear diagrams. Instead of showing helical windings disposed about the field magnet, it seems better to show merely a flux of current through a space bounded by the armature core, the pole-tips, and the part of the magnet joining the pole-tips. This space may conveniently be termed "the magnetising space;" it is shown in several of the figures used for the illustration of this paper. Bearing in mind that the primary object is, or should be, to magnetise the armature, the diagram shows clearly the essentials of the magnetic circuit. The winding is shown in section by circles which are respectively black or dotted, to indicate the two directions of the current. Clearly it is not necessary, so far as the direct influence on the armature is concerned, to show the

completion of the field turns; they may be completed by going round the armature, or round any part of the field, or, for the matter of that, they need not be completed at all so far as the armature cares. This kind of diagram reduces all forms of field magnet to a common basis, and illustrates the essentials of the magnetic circuit. And it helps to the understanding of the problem of self-exciting armatures by showing how the armature conductors supply the "magnetising space" with current, in the magnetising direction or the reverse. Mr. Mordey.

10. In earlier days we often debated the weighty question, What part of a dynamo or motor should be the heavier? We are now probably all too wise to spend time in such discussions. Our point of view has altered, and we wish only to make each part as light, or, at least, as inexpensive, as we can. A simple diagram of this sort helps to the appreciation of what seems to be the true function of the field magnet. A field magnet should be a mass of iron so arranged as to complete the magnetic circuit between one side of the armature and the other, in the shortest possible distance, and with as small a cross section as is consistent with the economical conveying of the magnetism of the armature.

Field magnets seldom accord very closely with this definition.

Comparison of Types.

11. Of the two classes* of dynamoes—smooth-cored and toothed—the former is preferred in this country, principally because the collection is so good; the latter is preferred on the Continent and in America. In the United States the 500-volt railway generators are almost always toothed; but for low-tension lighting work, where large currents have to be collected, smooth cores are often used.

In comparing the two types, we may say of the smooth-

* Dynamoes with "coreless" armatures are not important enough to form a class. The reasons which led to the use of "coreless" alternators do not apply to dynamoes: in the latter the periodicity is not high enough to justify the omission of the iron.

r. Mordey. cored type that in practical work it gives excellent results; that its collection is excellent, but that it is expensive in material, and in some other respects unsatisfactory.

12. The toothed kind does not collect well, and is not a good regulator, but it has so many good qualities that it is of the greatest importance to remove what is objectionable in it. What may be termed its latent advantages—mechanical, electrical, and magnetic—as compared with the smooth-core type—are:—

1. Small excitation.
2. Small magnetic leakage.
3. Absence of eddies in armature conductors.
4. Armature conductors solid instead of laminated.
5. Drag on armature conductors small.
6. Armature conductors easily supported.
7. Cooling qualities very good.
8. It is cheaper to make.

13. These are termed latent advantages, because they are not at present all realised. Referring to the various points—

(1) *Small Excitation.*—The excitation is usually smaller than in smooth-cored machines, but not nearly so small as it might be made. The reasons are, that to check armature reaction and to keep sparking within limits, the gap is usually made a good deal larger than would suffice for mechanical clearance, and the teethed are often worked at a very high density, giving the effect of a still larger gap.

(2) *Magnetic Leakage.*—As leakage depends principally on the difference of magnetic potential across the gap, it follows from (1) that it is larger than it ought to be. Some general observations on this matter will be found in another section of this paper.

(3) and (4) *Absence of Eddies in Armature Conductors.*—This advantage is fairly realised. Except when the teeth are saturated there is very little magnetism in the slots, and the conductors need not be laminated.

This saves power, reduces cost, and makes the conductors more mechanical and easier to wind.* Mr. Mordey.

(5) and (6) *Drag on Armature Conductors.*—As this is practically absent, the conductors require only sufficient support to drive them, without reference to the load. Even if this were not true (and the question is considered in another part of the paper),† the mechanical support afforded by the teeth would offer a very great advantage.

(7) *Cooling.*—This is very important. In a smooth-cored armature the core is covered by insulating materials, which, unfortunately, insulate heat nearly as well as electricity. In a toothed armature the exposure of the bare metal surface assists very materially in getting rid of the heat. The value of this feature can hardly be over-stated.

(8) *Cheapness.*—The toothed type for any given output is, even now, cheaper than the smooth-cored.

14. If the above statements are correct, the toothed type has everything in its favour except sparking and armature reaction. Advocates of smooth-core construction may assert that the drawbacks to their favourite type are more apparent than real—that when once set in the bearings the armature gives no trouble.

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* An experiment made some years ago by the author may be referred to as illustrating the absence of eddies in conductors surrounded by iron. A transformer of the closed magnetic circuit type was wound with one winding only, the space usually occupied by the second winding being left empty. The winding was connected to a suitable source of alternating E.M.F., and the power absorbed was measured by a very sensitive wattmeter readily capable of showing a small increase in the excitation. Solid bars of copper were then inserted in the vacant space. There was no increase in the excitation. If any eddies had been set up they would have acted as a load, and the wattmeter would have shown the amount. The object of the experiment was to ascertain whether it was necessary to laminate large secondary conductors in transformers; in closed-circuit transformers it was not necessary. This case is perhaps not on all-fours with sunk windings, but they are closely related.

† Pars 43 to 47, pp. 564 to 566

Mr. Mordey. But this is no answer to the really inherent objections to the type, and we all know that in building and handling it requires much more care and skill than the toothed kind.

15. And, on the other hand, advocates of toothed armatures may say that it is rather late in the day to insist on the advantages of the machines they have been building and using for years; that the machines are excellent, do not spark at all, and so on. All which may be quite true. Some of the machines are excellent. But there are two extremes (and a great many intermediate varieties) in the class of toothed-armature dynamoes—

(i.) Those with a small gap reluctance and bad collection; and

(ii.) Those with a large gap reluctance and a better collection.

In neither sort are the full potential advantages of the type realised. The large-gap variety ought perhaps to be considered intermediate between the smooth-cored and the toothed kinds. The teeth support and drive the conductors, but the excitation is not small and the armature reaction is very great in spite of the gap (as is seen by the comparison quoted above); and to the extent that the magnetism is not carried by the teeth, so are the full benefits and economies of the toothed type lost: the direct drag is felt by the conductors; they have eddies generated in them, and, if they are large, require to be laminated; the magnetic leakage is considerable, and the machines are not very cheap to build.

Magnetic Leakage.

16. Magnetic leakage is a much more serious evil than is generally admitted. In most dynamoes it is enormous, even in machines which enjoy the reputation of being nearly perfect examples of design—and which, indeed, are nearly perfect, judged by present standards. For example, the ordinary two-pole drum dynamo only utilises about 70 per cent. of the magnetism produced. The leakage coefficient (v) given in the Royal Society paper already referred to was 1·32—which means that out of every

132 lines of force produced in the field magnet only 100 pass through the armature; the other 32 leak past the armature and are wasted. This coefficient was obtained from an unloaded machine; at full load v would probably be 1·4, or more. Values of v for six varieties of machines given by Professor Thompson* range from 1·25 to 1·49—average, 1·34.† The proportions of all dynamoes have to be seriously modified to allow for this waste. The section of the magnet is increased from 25 to 50 per cent., or, if not increased in section, it must be increased in length to carry the greatly increased field winding. The incidental disadvantages connected with leakage of magnetism are well known—disturbance of compasses on board ship, and of watches, the use of non-magnetic distance pieces, and so forth—and there is no incidental advantage of any sort. It is sheer waste. Mr. Mordey.

17. The magnetic system of a dynamo is usually considered from what appears to be a misleading point of view. It seems to be a mistake to begin by magnetising the field magnet when the real object is to get the armature magnetised. It may be answered to this, that there is no beginning,—that the whole magnetic circuit has to be magnetised, that the armature is only one part—and a very small part—of it, and that it comes to the same thing however or wherever we magnetise the circuit. But there is a difference, as was recognised long ago by Forbes,‡ and realised in practice by Eickemeyer, who placed the exciting coil directly round the armature, where its effect was primarily wanted, instead of round the field core.

The full significance of this construction has perhaps not been generally recognised. Its effect on magnetic leakage is very important, for it makes the leakage negative. In other words, as the armature carries more magnetism than the field, instead of less, the leakage becomes a friend instead of a foe; for practically all the magnetism generated is now useful, because it traverses

* "Dynamo-electric Machinery" (1896, p. 151).

† All light-load values; for full load they should be considerably increased.

‡ British Patent 4120, 1885.

Mr. Mordey. the armature.* It is, of course, not to be inferred from this that the leakage should be encouraged by skimping the iron.

18. The practical objection to this construction is that it is not so simple to dispose the field winding round the ends of the armature as round the core of the field magnet, and that there is no gain in copper or energy, as the length of the turn round the armature is usually more than round the field. But the absence of external stray field and shortness of the magnetic circuit are points of much practical importance, worth some effort to obtain. The inconvenience of inside windings will become less as we learn how to reduce their amount. And what would help materially would be the reduction of the cost, and the improvement of the quality, of steel castings. This country is behind others in this important trade, and it behoves our steel founders to give it their attention. Magnetic steel castings are being produced in the United States in large quantities, with ribs and other thin parts clean and thin as if of cast iron.

19. The considerations affecting leakage, as connected with the disposition of the field magnet, become of diminishing importance as the air gap is reduced. If there were no gap there would practically be no leakage—a further reason for using toothed armatures.

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20. What we want is the sparkless collection and small armature reaction associated with smooth cores, together with the advantages of economy and of energy which are inherent in toothed armatures, but which are at present not fully realised in practice.

21. It is a very singular thing that the windings originally introduced have continued in use practically without change ever since. For instance, in ring armatures the ordinary endless helix, coiled round an iron core, with connections taken at regular intervals to a collector, is in use to-day just as it was introduced by Gramme and Pacinotti about 27 years ago.

* That the leakage may be useful, instead of detrimental, is evident from the fact that an armature will work if surrounded only by an exciting coil, the iron of the magnet being removed. In such a case all the magnetism would be "leakage."

22. With this, as with all ordinary windings, we have the condition of things shown in the three diagrams in Fig. 1. (*a*) Mr. Mordey.

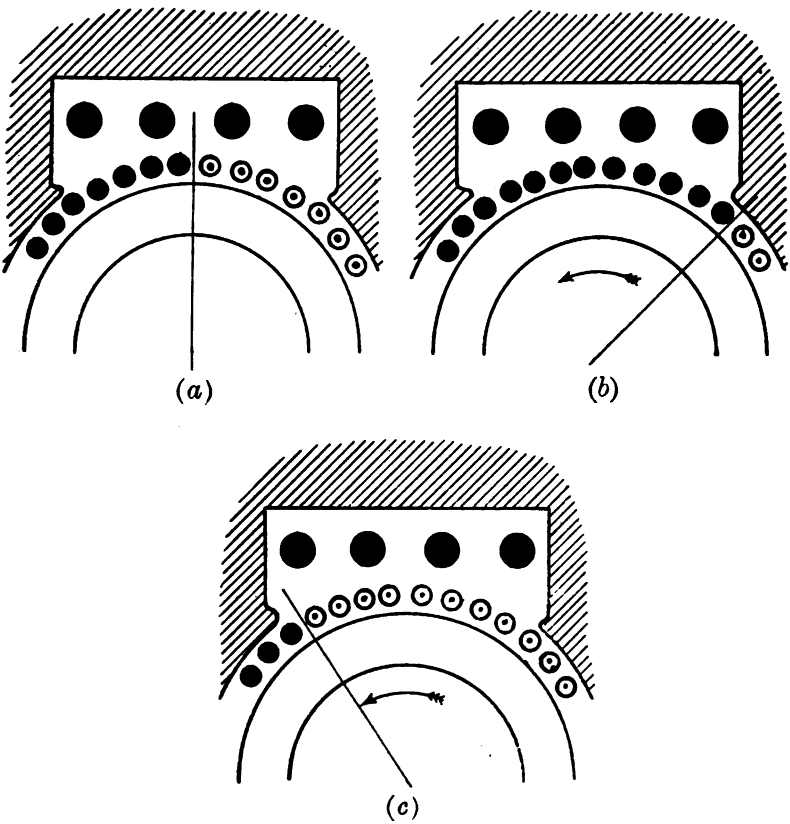

FIG. 1.

With no lead, the positive and negative directions balance. (*b*) With brushes lagging* to the backward pole-tip the armature current helps the field; with brushes leading to the forward tip (*c*) it opposes the field. (*c*) is the usual collecting position necessary to secure good collection. There are other well-known aspects of armature reaction, but they do not come within the scope of this

* The expressions "forward lead" and "backward lead," although sanctioned by misuse, are not happy ones. "Positive lead" and "negative lead" are better. "Lead" and "lag" are shorter terms.

Mr. Mordey. paper. We should use some arrangement such that, without loss of E.M.F., the armature conductors in the "magnetising space" shall produce no weakening of the magnetising force, or, preferably, shall add to the magnetising force in that space. If we can collect with a lagging brush, the armature current will help the field winding. Or if we can collect in a midway position the armature currents will be equal and opposite, and there will be little or no counteracting effect. The two latter conditions are ordinarily prevented by sparking, as the coils under commutation have an active E.M.F. in them, and a large current is generated in the short-circuit.

Sayers Winding.

23. Sayers indicated the right direction to work. The investigations of Hopkinson, Swinburne,* and others† paved the way for Sayers, who set himself a double problem—to prevent or reduce armature reaction by collecting in a position where the armature current should assist the field, and to short-circuit active coils without sparking; and he obtained a solution of that difficult problem. By means of auxiliary coils connected between the ordinary coils and the commutator, but which were idle except during the moment of commutation, he injected into the armature coils under commutation, an E.M.F. counter to the mischievous spark-creating E.M.F., and so succeeded in collecting in the desirable backward position.

RING ARMATURES.

24. Let us consider the ring armature, and inquire whether we have exhausted all simple expedients.

Let us vary the monotony of the ordinary Gramme winding, and find whether, by a little rearrangement, it does not give

* *Journal:* "The Theory of Armature Reactions in Dynamos and Motors;" vol. xix., p. 90 (1890).

† Reference must not be omitted to Ryan's work, "Method of Balancing "Armature Reactions" (*Sibley Journal of Eng.*, Oct., 1892). See also Ryan and M. E. Thompson, *Am. I.E.E.*, 1895—a most complete and interesting solution. An equal and opposite circle of currents is applied round the armature, counteracting the armature reaction. See also pp. 391-2, S. P. Thompson's book (1896.)

what is wanted. Consider such a winding having, say, four turns to each sector of the commutator, as in Fig. 2 (i.). Call this an "element" of the winding. Now arrange that element as in Fig. 2 (ii.), disposing the four turns, not all together, but two and two, with a gap between them about equal to the gap between the pole-tips, and dispose all the rest of the winding in the same order, filling in the space between the parts of each element by parts of other elements. Join all the elements in series, not exactly as in the ordinary Gramme winding, but so that the current passes along each side of the armature in a series following a zigzag course, instead of the usual one. It will be observed that in each side of the armature the parts of each element, as well as the whole elements, are in series, adding their E.M.F.'s exactly as in an ordinary Gramme armature; but that when any element passes through the region between the poles (where in an ordinary winding it would be adding nothing useful to the circuit), it becomes the seat of two opposing E.M.F.'s, and that when those opposing E.M.F.'s are equal the element may be short-circuited without any current being generated in it, although in each part there is a strong E.M.F. Mr. Mordey.

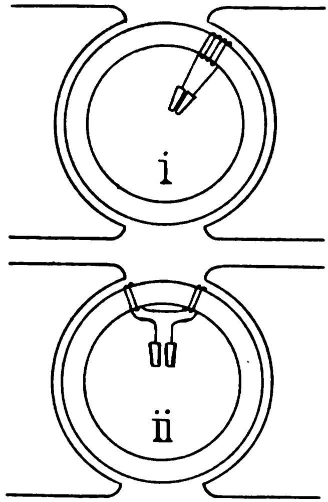

FIG. 2.

25. Fig. 3 shows five successive positions of an element in its passage from one side of the field to the other.

Fig. 4 shows more clearly the winding of a single element and parts of others; Fig. 5 is a diagram showing a complete armature; and Fig. 6 is a diagram of an extended portion of an armature, showing the connections of the elements, and the directions of the current in them. To distinguish it, the element under commutation is shown thicker than the others. The field winding is shown by large black circles, and the two directions of the armature current are indicated respectively by small black

Mr. Mordey. circles and by dotted circles. It is important that the armature coils should show black circles rather than dotted circles, because

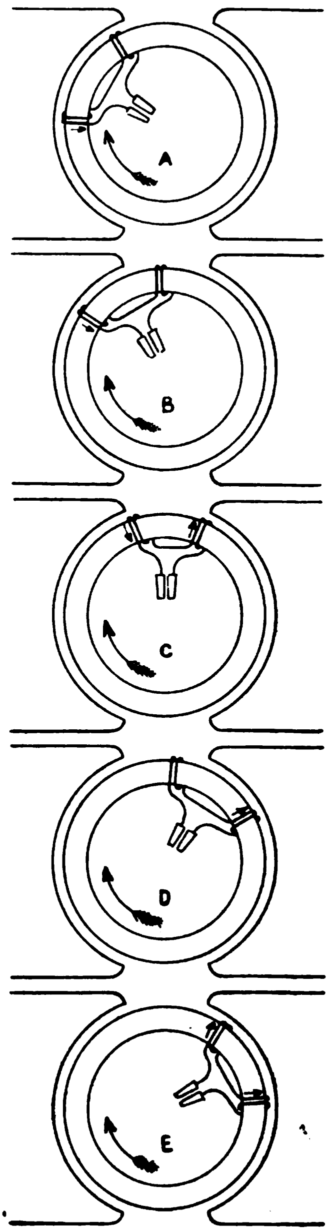

FIG. 3.

the black circles are what are wanted in the magnetising space. Mr. Mordey.
On the other side of the armature the conditions would, of course,

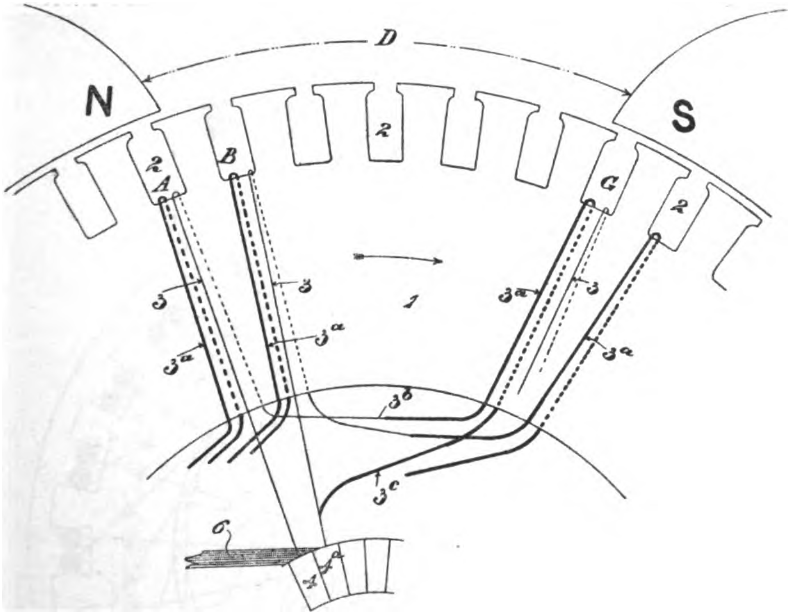

FIG. 4.*

be reversed—the magnetising space containing dotted circles, and the armature conductors should show as dotted circles. In practice there is not so great a gain with this particular arrangement as the diagram shows, because of the cross induction and the slight movement for the reversal of the current. The relative value of this gain depends, of course, upon the initial value of the excitation. A reference to the results, quoted further on, shows that it may amount to a saving of about half the ordinary field winding, and that a further saving may arise from the possible lowering of the initial excitation.

26. Figs. 5 and 6 show that this winding is quite symmetrical: the armature may be run in either direction, exactly as with the ordinary winding. Within the span of the element under

* This figure is from a forthcoming publication; it is not necessary to explain the letters of reference here.

Mr. Mordey. commutation the armature conductors carry currents which alternate in direction, so that from this part of the winding there is no reaction on the field. It may be supposed there is

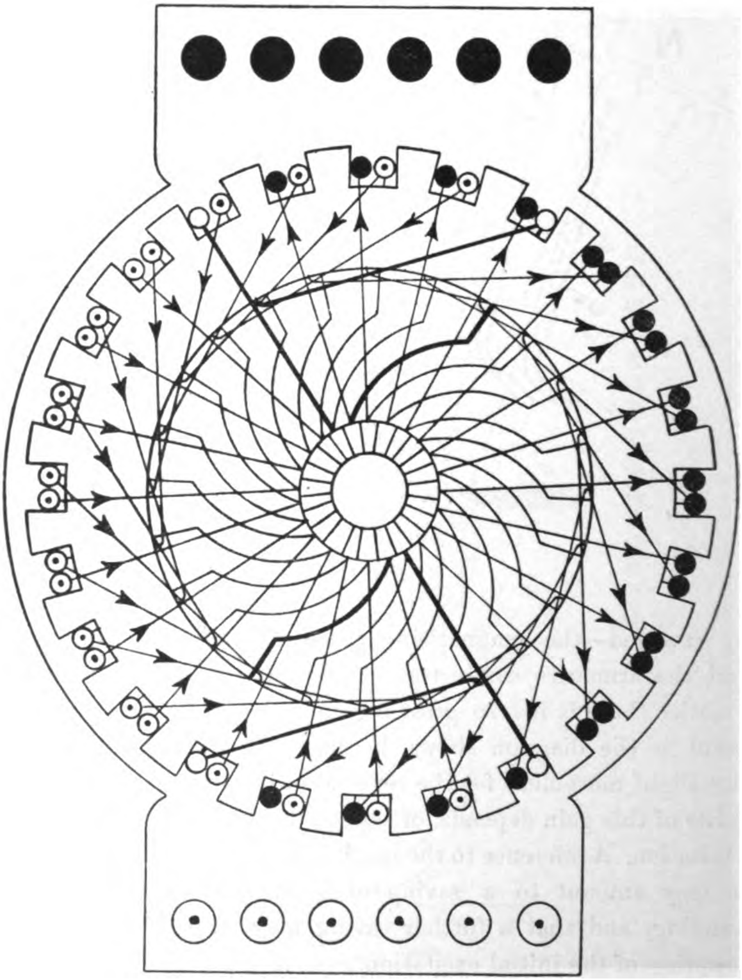

FIG. 5.

some loss of volts, as the volts and current in the conductors comprised within this span are alternately in the same and in opposite direction to the E.M.F.; but this loss is not appreciable at no load, as might be expected from the small E M.F. near

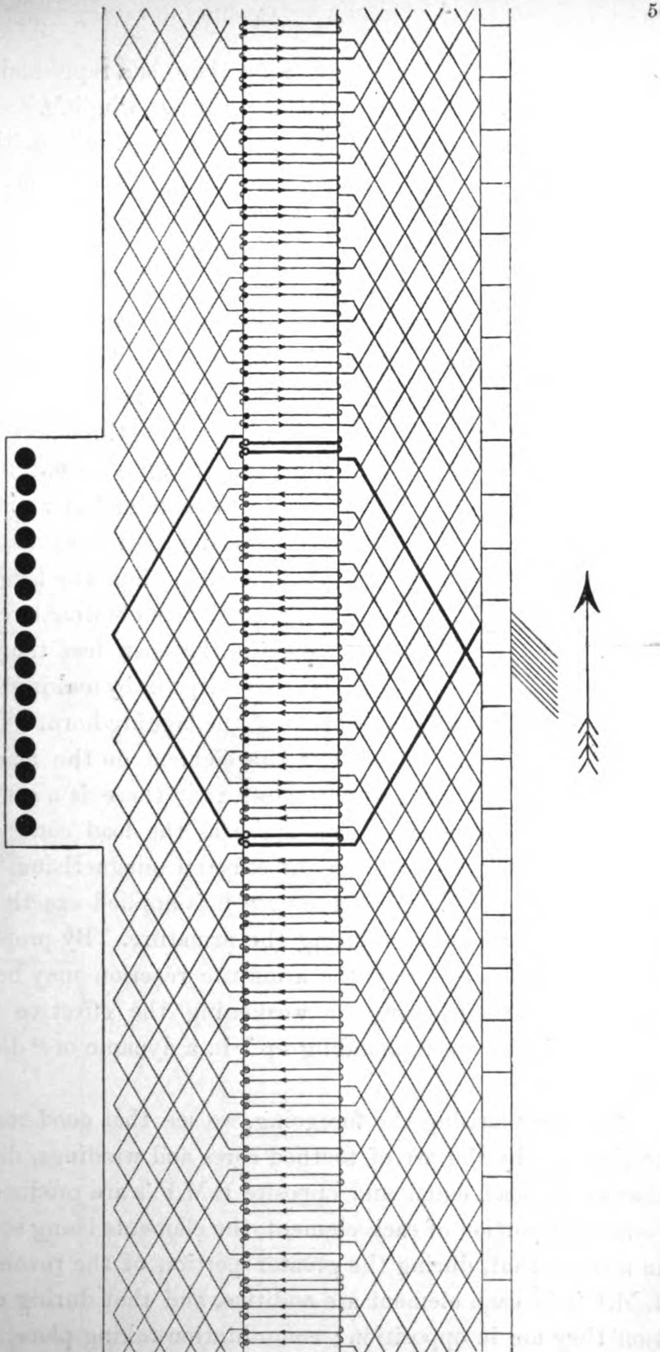

Fig. 6.

Mr. Mordey. the neutral line. As the load rises there is a rapid and marked gain in E.M.F. as compared with an ordinary winding.

It may be supposed that with this winding the breadth of the neutral line must be narrow, but this will not necessarily be the case if the slope of the field is slight in the regions occupied by the parts of the element under commutation.

27. Other ways of arranging the element will suggest themselves, both as to the distribution of its parts and the span. Fig. 7 shows an element of four ~~horns~~ turns divided into two unequal portions—one and three, instead of two and two, the three ~~horns~~ turns leading. With such an element a balance would be obtained at no load when the leading part was in a much weaker field than the lagging part —a condition obtainable either by making the span less than that of the pole-tips, or by making the clearance less at the leading horn than at the lagging horn. The effect of such an unequal division of the element on the magnetising space is seen in Fig. 8, which shows that there is a considerable strengthening reaction in that space as the load comes on. As compared with the usual loss of effective magnetising force this may be very useful. And of course it is applied exactly where it is most effective in magnetising the armature. By proportioning the element in this way the armature reaction may be utilised either for strengthening or weakening the effective field—as, for example, by "compounding up" in a dynamo or "down" in a motor.

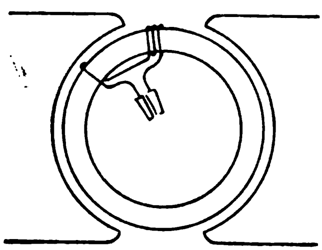

FIG. 7.

28. Summarising the foregoing, we see that good results may be obtained by the use of toothed cores and windings, disposed so that at no load equal and opposite E.M.F.'s are produced in the connected portion of each element, the elements being so arranged in a series that, during the greater portion of the revolution, the E.M.F.'s in each element are additive, and that during commutation they are in opposition; commutation taking place, not when any coil is on or near the neutral line, but when it is away from

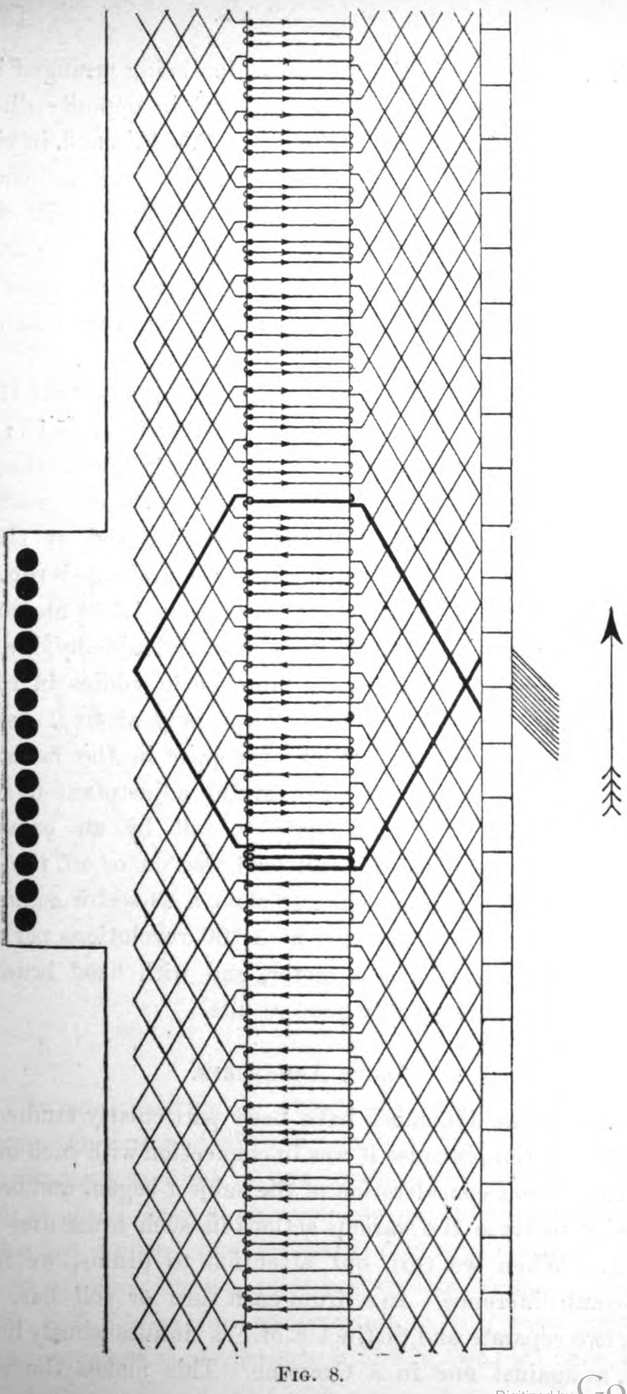

FIG. 8.

Mr. Mordey. that line. The coils in the neutral region being arranged in such an order that successive coils are traversed in opposite directions by the current, irrespective of their own E.M.F.'s, and, in virtue of which alternation of direction, these coils cease to exert any influence in the "magnetising space." Or, by a modification in the proportions of the element, we have seen how the armature current may, as desired, be caused either to increase or to diminish the magnetising force acting in the magnetising space.

29. Results have been obtained by testing a toothed Gramme machine of "Manchester" type, wound as in Figs. 5 and 6: it gave an output, with good collection, about one-third more than could have been satisfactorily taken from a smooth-core machine of this type. It was a 15-kilowatt machine, and at this load attained a rise of only 30° Fahr. after a prolonged run. The current-density in the armature was about 1,500 amperes per square inch; this low temperature was undoubtedly due to the exposure of the core, and to the absence of eddies in the conductor. The full-load excitation was only about 1 per cent., without an extravagant amount of copper in the fields. The reaction was so slight that by careful adjustment of suitable brushes it was found impossible to tell by an examination of the brushes whether the full load was on or off, the brushes being fixed. The armature had 48 slots, a 48-sector commutator, four turns in each slot, and ran at 1,100 revolutions per minute. The machine was run as a motor, and with fixed brushes and varying load it gave equally good results.

Drum Armatures.

30. Gramme dynamoes have been particularly studied in the foregoing section, because it was in connection with such machines that the present consideration of the subject began, and because it is easier to trace the various actions in such armatures than in drums. When we turn our attention to drums, we find one important difference. In a drum each turn or coil has, or may have, two separate and distinct E.M.F.'s simultaneously impressed on it, as against one in a Gramme. This makes the study of

drums somewhat less easy than rings. But, bearing this difference in mind, it is obvious that all the main considerations put forward apply equally to the two kinds of armature. It is only necessary to so arrange the winding that the circle of conductors seen in section, surrounding the core, shall present the same order in the two cases. Mr. Mordey.

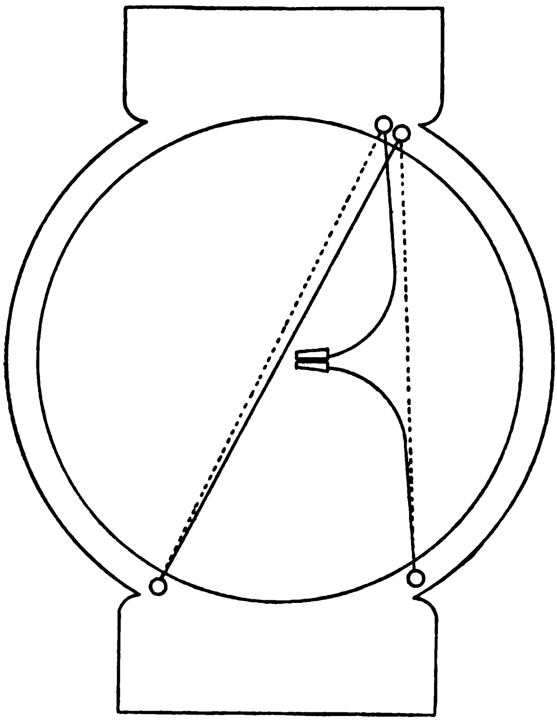

Fig. 10a.

31. In endeavouring to obtain the same effect in a drum armature, the first and obvious course is to form an element, as in the Gramme winding, of two turns joined in series, each turn wound, as usual, over a diameter, or an approximate diameter, but the two crossing one another at the required angle to give them the same relative positions on the core, or the same relative E.M.F.'s as in the Gramme. But, since each turn has two separate and distinct E.M.F.'s, we at once observe

Mr. Mordey. that this procedure may be simplified by so disposing the two halves of each turn that the parts of a single turn will occupy the required relative positions and form a complete element; that is to say, we may wind the single turn, not across a diameter,

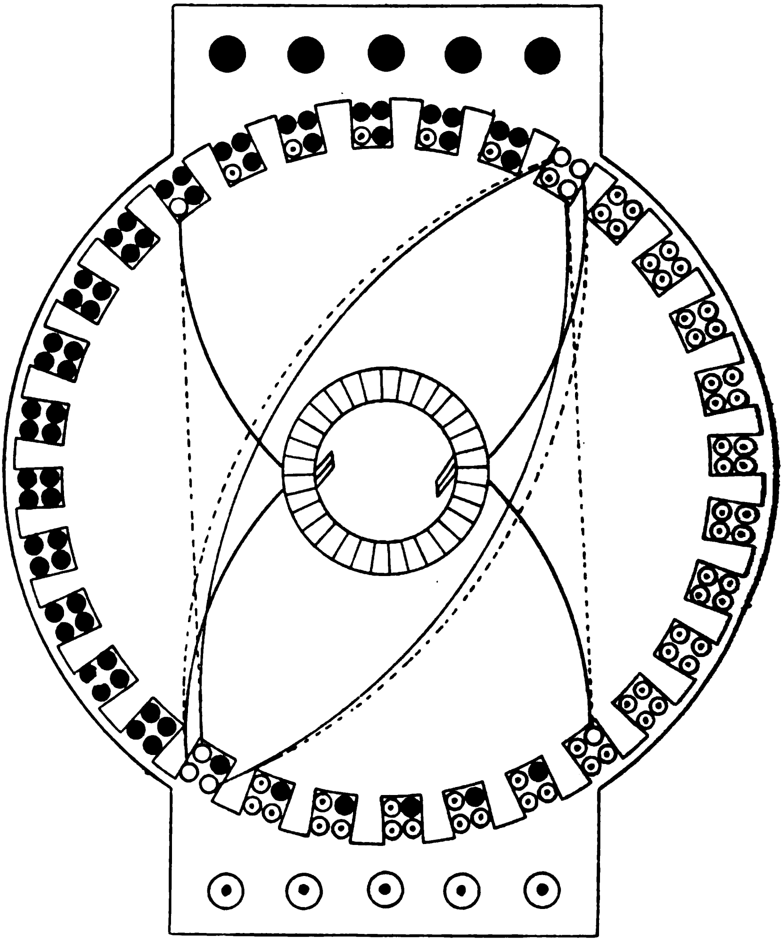

FIG. 10B.

as usual, but across such a chord that its two portions comply with the conditions stated in the preceding paragraph. And by so winding and so connecting the turns in the required order we get nearly all that is desired. Such a winding will give us

conditions equivalent to those shown in Figs. 5 and 6. And if we wish to go further and to obtain conditions equivalent to Fig. 8, and we cannot find a chord capable of giving what we want, we may use a double turn of any combination we desire. Fig. 10A shows such an "element," consisting of a short chord and a long one, or a diameter. This element consists of four bars. The portion of the winding in the "magnetising space" exhibits an improved reaction, the current in three bars out of four being in the assisting direction. This is illustrated by Fig. 10B, the two elements under commutation being shown in full, and the directions of current in the whole winding being given. Fig. 10C is a developed diagram of this winding, showing the whole of the connections An armature wound in this way gave excellent results. Mr. Mordey.

32. But it may be said that chord windings have often been proposed,—that Swinburne went into them pretty fully a good many years ago,—that the very first elementary diagram of a drum in Thompson's book* shows a sort of chord winding; and, further, that toothed drums are well known and have long been used,—that toothed armatures are indeed as old as the magneto machine. All this is granted. And it may be said that there can be no latent or undeveloped possibilities in the suitable combination of these two things, since the severe commercial competition of the past few years would have surely brought them to light. But it is possible that the complicated nature of the disease has prevented a simple remedy being sought for.

33. This section may conclude by an account of results obtained by modifying a toothed drum dynamo on the lines explained. The machine was a new one, just turned out by a well-known firm of Continental manufacturers. It was supplied to do 15 kilowatts at 1,050 revolutions.

As received it gave the curve A, Fig. 9, which shows the excitation at various loads, with constant volts and constant speed. The collection was not good even at small loads; it had no sparkless position, and the volts varied very much when the brushes

* "Dynamo-electric Machinery," p. 39 (1896).

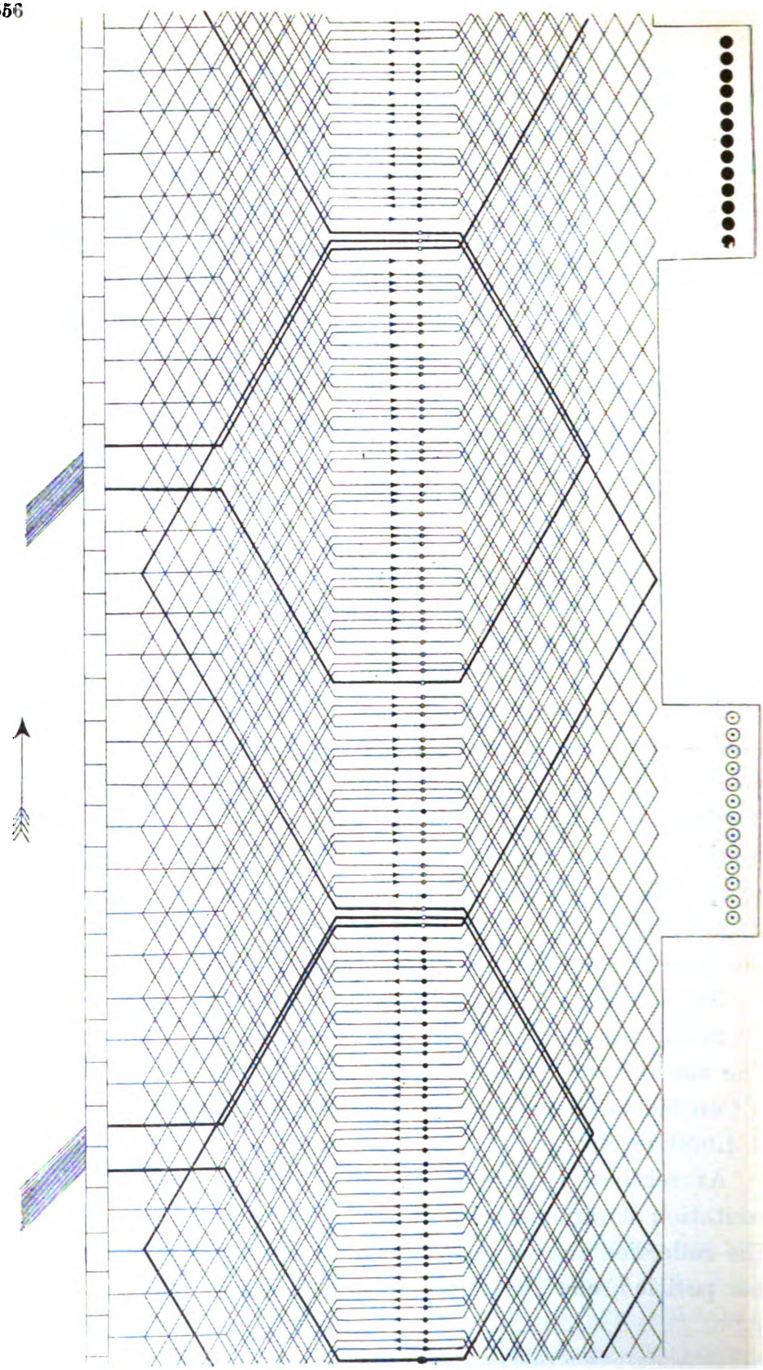

Fig. 10c.

Mr. Mordey.

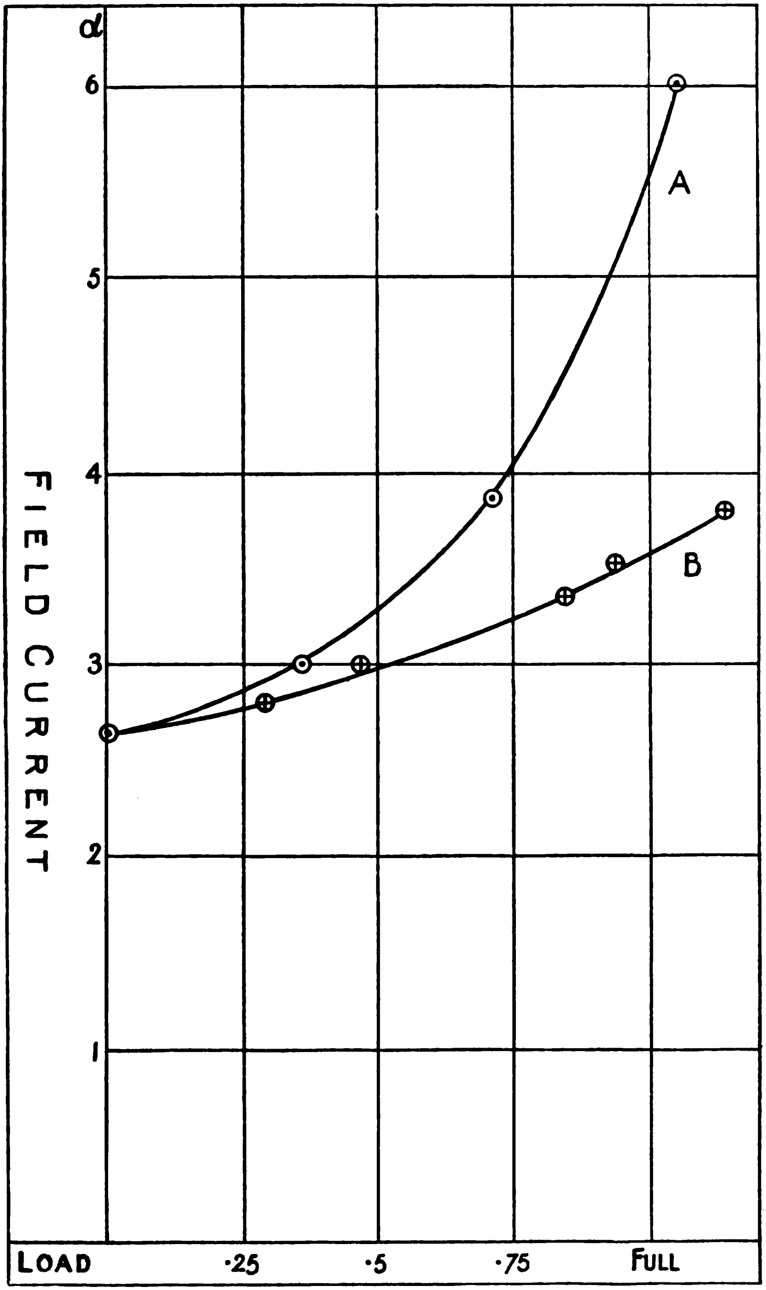

Fig. 9.

Mr. Mordey. were moved slightly. It was then rewound with a conductor of the same cross section as before, and with the same number of turns. There were 32 slots and 32 sectors, 4 conductors in each slot. The actual winding diagram is given in Fig. 10, which is

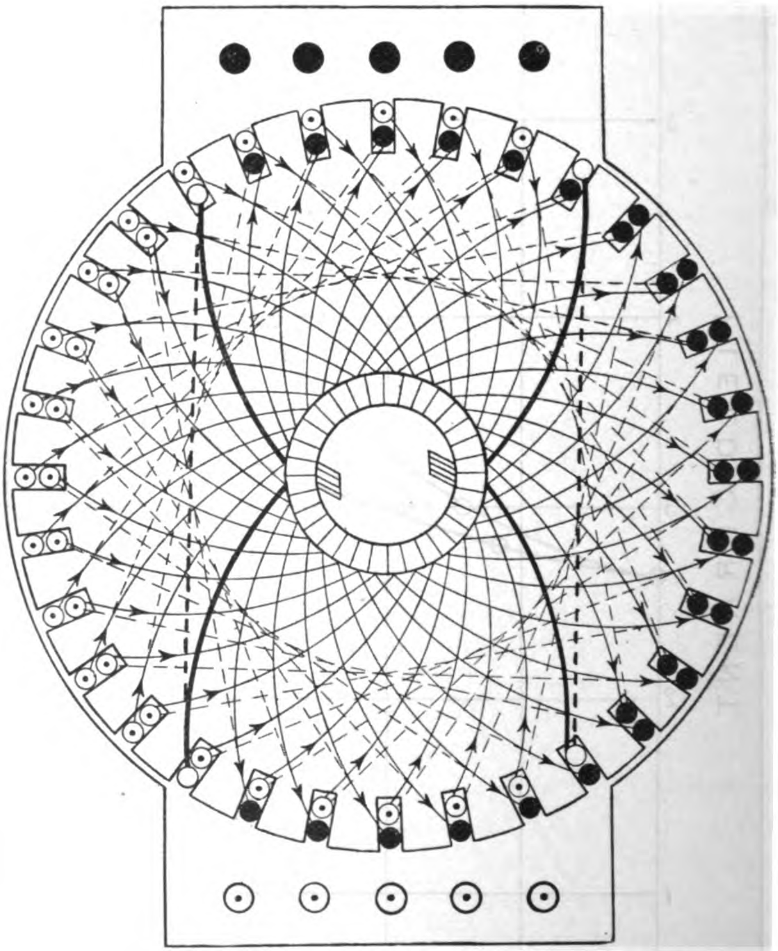

Fig. 10.

obviously a much more convenient and simple winding than the ordinary diametrical winding. Keeping the gap the same, it was re-tested, and gave curve B, Fig. 9. The collection was now excellent; the excitation at full load was 72·5 per cent. in amperes, and 44 per cent. in watts, of the old excitation. The variation of

volts for a given movement of the brushes was a quarter of what it was before; the full-load lead, which was originally 67·5°, was now 12·5°. The machine would now probably pass muster as a satisfactory one in every respect; previously it certainly could not have been considered a first-class machine, nor even a good example of an ordinary toothed-armature dynamo. Copper gauze brushes were used in both tests. Mr. Mordey.

It may be thought that these windings are not as simple as the ordinary diametrical windings. They are actually simpler and cheaper. For instance, the winding shown in Fig. 10 was found to be considerably shorter than the winding it replaced. It was much easier to put on, and the end connections occupied only half the original end space. It was a wire-wound machine. A further advantage, apparent from the figure, is that a central space is left clear for ventilating purposes.

The winding in Figs. 10A, 10B, 10C, is intermediate as regards length and convenience between the ordinary diametrical winding and that shown in Fig. 10.

Brushes and Collection.

34. The importance of good collection makes it worth while to carefully study, not only the main conditions, but any minor effects that bear on it. Dynamoes may be designed so liberally that they are almost incapable of sparking under any circumstances. But this is expensive, and we should try to develop less costly methods, and, if possible, find how to check tendencies to sparking. Prevention is not always better than cure. Many inexpensive dynamoes, if cured of sparking, would be quite good and useful machines.

35. There are few matters of which our knowledge is less definite than the exact actions in the region of the neutral line, or even the precise causes of the movement of that line. This line of best collection is usually assumed to be definitely determined by conditions depending on the armature-turns and current, the strength of the field, and so forth—that in a given machine, with a given load, the angle of lead is quite definite, and only to be altered by structural modifications of the machine,

Mr. Mordey. such as increase of gap or change of polar surface. But, as a matter of fact, it is quite possible to shift the collecting line without any structural alterations—not merely to collect further back, but to get the *best* collection further back, and good collection no longer possible at the forward position, which previously was the best one.

36. Among the numerous and complicated actions that take place under the brush, there is one that has an important influence on the angle of lead—that is, the amount of current flowing in the short-circuit. As the coils go successively under the brush and are short-circuited, the pre-existing direction of current is continued, then it falls to zero, and then, as the coil passes out from under the brush, it rises again in the opposite direction. It will be clear from Fig. 11 that this short-circuit current may quite perceptibly increase the armature reaction by adding to the demagnetising effect in the "magnetising space." The amount of this current may vary, in different dynamoes, from very small values up to something considerably more than the working current; that is to say, when the coil goes under the brush the current may either fall quickly or slowly to zero, or it may rise to several times the working current before falling to zero. The large circles in Fig. 11 indicate the large short-circuit current, and show clearly how it may add to the demagnetising effect. Experiment has shown that by checking this current it is possible to considerably reduce the lead angle.*

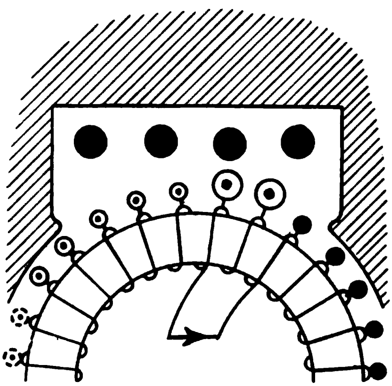

Fig. 11.

37. Even in a dynamo which works sparklessly and well, the

* In explaining this effect, a preference may be felt for regarding the gain as due to the smaller reversing field required (and therefore smaller lead), resulting from the more rapid reduction of the existing current; this comes to pretty much the same thing.

short-circuit current may be serious in amount. It is harmful Mr. Mordey. in many ways. It causes waste of power both in the short-circuit itself, and in the field winding, which must be increased to compensate for it. It reduces the output by lowering the sparking limit, by increasing the lead, by increasing the drop of volts,—by requiring larger brushes and commutators than would otherwise be used.

This short-circuit current may be checked by an opposing E.M.F. or by a resistance; the former requires structural alterations.

Resistance in Commutator Leads.

38. The obvious way to check such short-circuit currents is by the insertion of resistance in the portion of the circuit under commutation, as, for example, between the commutator and the collector. This is easy; and, as the resistance is only in circuit as the coil passes the brush, and at no other time, an amount may be added large in proportion to the resistance of the coil short-circuited, but small as compared with the whole armature. But this plan, although it possibly has some effect on armature reaction, has no marked effect on sparking, for a very simple reason. To break a circuit without sparking, a resistance should be gradually introduced, and the circuit broken when the resistance is a maximum and the current a minimum. Unfortunately, this is not what happens in the case under consideration. The resistance is introduced and removed suddenly.

Experiment has confirmed this explanation. Resistance was added to the commutator connections of an armature without any noticeable effect on the sparking. The amount was varied from something quite small to sufficient to increase the C R drop about 20 per cent.

Resistance in Brushes.

39. By a simple expedient the brushes themselves may easily be made to do what is wanted. An ordinary brush connects the adjacent sections by a path of very low resistance, allowing currents to circulate freely, checked by little more than the "contact "resistance."

Mr. Mordey. The study of reactions leads naturally to the perception of the qualities a brush should possess. It should have a low resistance, so that it may easily carry large currents; and it should have a high resistance, to prevent the circulation of large currents. Fortunately, this combination of opposite qualities is attainable by lamination across the path of the circulating currents. If a brush is slit or divided by insulating divisions (as in Fig. 12),

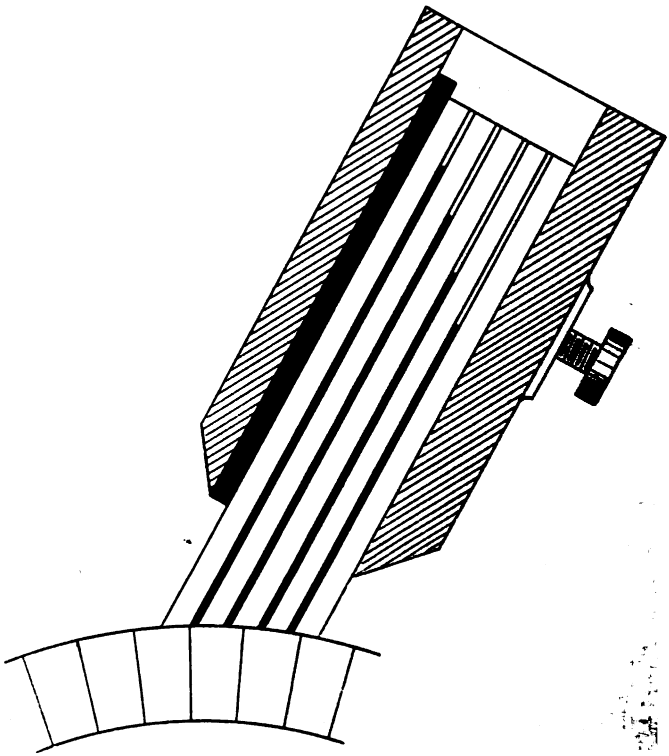

FIG. 12.

although it is just as effective as before for the main current, it opposes a much longer path than usual to a short-circuit current, which, in consequence, is much reduced.

40. And the graduation of the resistance so that it is removed gradually from the circuit is not difficult. A little thought will make it clear that, as sparking does not occur when a

contact is made, but does occur when it is broken, we should encourage the current to enter the brush at the back, where the brush is making contact with the sectors as they successively pass under it; but we should discourage it from entering by the front, where the brush is breaking contact with the sectors as they successively pass away from it. Mr. Mordey.

Such a distribution of current is facilitated by making a good contact at the side of the brush for which the current should have a preference, and a bad contact, or none at all, at the front side, where it is desired that only a small part of the current should enter. Fig. 12 shows this arrangement.

41. Brushes of this sort are easily made—for example, by placing together a number of thin plates or layers of carbon, or metal gauze or foil, with an insulator between the layers except near the end where they are brought into contact. With carbon plates a good plan is to stick the plates together with an insulating cement except at one end, where tinfoil is put between them to ensure contact: they then form solid blocks like ordinary carbon brushes. Such brushes applied in a particular case gave a better collection, and only half the lead of ordinary copper gauze brushes; they were distinctly better than ordinary undivided carbon brushes

42. Carbon brushes were originally proposed by Forbes.* Their good effect was at one time attributed to small friction against the commutator, and to some peculiar property by virtue of which the current passed to them more easily than to better conductors. Their success probably is owing simply to their high resistance, by which they are enabled to check reactions after the manner described.

One objection to carbon brushes is that, on account of their high resistance, they require to be of larger section than other brushes; a density of 75 to 100 amperes per square inch being as high as they should be worked. If this density is exceeded they heat very much at the contact surface. Possibly they may be made to carry larger working currents when made so that the

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* British Patent No. 1288, 1885.

Mr. Mordey. circulation of current through them is reduced. What the actual current-density is at the contact face in any given case cannot easily be determined, as the circular effect so disturbs the distribution of the current at the face, by increasing it at one part and opposing it at another. The action of heat in changing the conditions, by varying the resistance, may also be worth a passing notice.

Drag on Buried Conductors.

43. Whether the mechanical pull is exerted directly on or by the conductors of armatures having sunk windings, or whether it is borne by the iron, has been repeatedly discussed. It has, of course, been freely acknowledged that, in slotted armatures, the support given to the conductors by the teeth makes such armatures mechanically superior to those having smooth cores. But many able persons hold that the advantage goes no further, —that the teeth act only as so many "driving horns," the torque coming on the conductors, which are actually driven by the teeth through the insulation. If this is the case, the conductors ought to be as securely fixed in the slots as possible, in order that the racking and hammering of the insulating material may not affect the life of the windings.

44. There is considerable interest in this question, both theoretical and practical. The practical considerations are of the greatest possible importance, for on the answer to this question many vital things depend affecting the engineering aspects of electrical applications. Constructors of electrical machinery have laboured (or have often believed they laboured) under a very serious disability—one which often seemed to present an effectual bar to the thoroughly satisfactory solution of many electrical engineering problems. This disadvantage has been the use of very unmechanical materials for mechanical purposes. It is probable that every electrical engineer has respectfully watched a locomotive take a heavy train up an incline, or the passing of an express train at full speed, and has thought of what it would involve if this work had to be done by the medium of a pull exerted on cotton-covered copper conductors tied by piano wire to

the surface of a smooth drum covered with shellaced paper and mica. The unmechanical character of insulators has always seemed to stand in the way. Mr. Mordey.

45. But if there is no drag on a buried conductor it changes all this. We need then no longer envy the mechanical engineer, for we, too, can put metal to metal and make sound mechanical

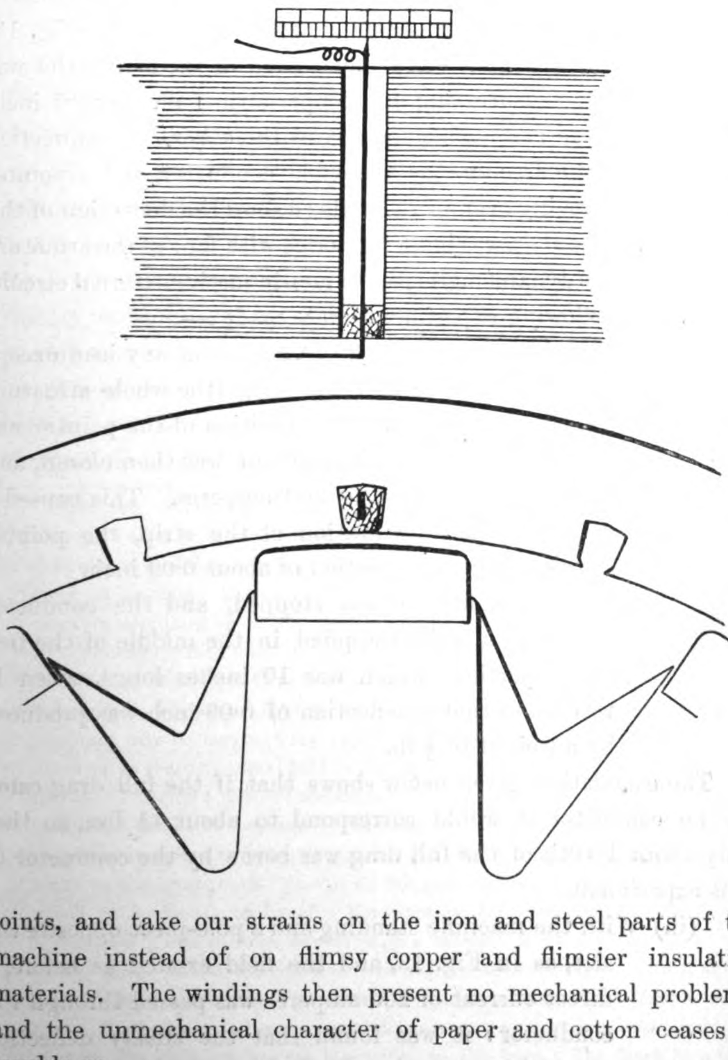


FIG. 13.

joints, and take our strains on the iron and steel parts of the machine instead of on flimsy copper and flimsier insulation materials. The windings then present no mechanical problems, and the unmechanical character of paper and cotton ceases to trouble us.

Mr. Mordey. 46. It may be thought that this point has long been settled, but that there is still much uncertainty about it is evident from quite recent utterances. This arises, no doubt, from the absence of experimental proofs—definite trials do not seem to have been made. An account of some trials is therefore given here :—

An inductor alternator was used having a stationary slotted armature, the polar projection of the rotor being of one polarity only. The arrangement is shown diagrammatically by Fig. 13. One of the armature coils was removed, and in the empty slot was placed a conductor consisting of a copper strip 1 inch by 0·1 inch, rigidly supported at one end, and free at the other. A connection was made to the free end by a flexible conductor, and a pointer was attached moving over a fixed scale to show the deflection of the conductor. The strip was joined in series with four of the armature coils, a Siemens dynamometer, and a non-inductive external circuit.

47. The following experiments were made :—

(i.) The machine was run at 25 $\sim$ without any load except on the four coils and the strip (the whole armature contained 20 coils). The position of the pointer was noted on the scale. The circuit was then closed, and the current adjusted to 200 amperes. This caused a very perceptible vibration of the strip, the pointer showing a mean deflection of about 0·09 inch.

The machine was stopped, and the conductor loaded by a weight applied in the middle of the free active portion (which was 10 inches long), when it was found that a deflection of 0·09 inch was produced by a weight of ½ lb.

The calculation given below shows that if the full drag came on the conductor it would correspond to about 4¾ lbs., so that only about 1-10th of the full drag was borne by the conductor in this experiment.

(ii.) With the machine standing and a pole-piece opposite the slot, as in Fig. 13, and the field excited as before, a direct current of 200 amperes was passed through the conductor: it was found that the steady deflection was rather more than before, viz., 0·12 inch.

From the data given by the tests of this alternator, 10 inches of active conductor (the length used in the experiment) would give 1·38 volts at the periodicity used, viz., 25 ∿. Mr. Morde

Assuming no phase displacement,

200 amperes × 1·38 volts = 276 watts = 12,170 ft.-lbs. per minute.

Surface speed at gap, 2,590 feet per minute.

Hence, if the full drag came on the conductor,

$$\text{Drag} = \frac{12,170}{2,590} = 4{\cdot}7 \text{ lbs.}$$

The comparison as to the effect of the drag and the weight is based on the assumption that the conductor was rigid and straight, instead of being slightly curved, as it would actually be; but the error produced by this assumption is unimportant.

It may be explained that in actual work the winding is such that each conductor carries about 80 amperes, and the ampere-turns in each slot are about 12 times those produced by the single conductor with the current used in the tests.*

These experiments appear entirely to support the view that the drag on the armature is borne by the teeth and not by the conductors in slotted armatures.

* On this subject the following interesting observations by Mr. Swinburne appeared in *Industries and Iron* of February 2nd, 1894:—"Some electricians have "difficulty in realising that in dynamos with notched armatures there is little or no "force on the coils. They argue that as the conductors produce their full electrical "pressure they must be cutting the field, and as they move uniformly and cut the "field uniformly, they must be in a strong field, and must therefore have the same "force acting on them as if they were on the outside of the armature. There is a "fallacy here, however. The pressure depends on the rate of cutting lines, and "the lines may be moving very rapidly in the opposite direction while they cut "the wire, so that the actual field in which the wire finds itself may be very weak "indeed. Imagine a notched armature and field. The lines will crowd into the "teeth, and avoid the notches. Take any one line: as the armature moves, it "cannot be imagined as being dragged round with it; it must therefore cross the "notch to the next tooth. It will do this very quickly. To get an idea of what "would be necessary to imagine the lines of induction, we can think of the "appearance of iron railings seen through a carriage window with a knot or lump "on the glass. There are practically no railings seen through the knot, and they "are crowded on each side of it, and as the carriage moves they jump across the "knot. The same number per second passes the knot as any other part of the "window, but they are always less dense in the knot. The lines in a hole or "notch-wound machine must be mentally pictured in this way"

Mr. Mordey. It is a little paradoxical that when teeth are not used the conductors need support, but have none; when teeth are provided the conductors do not need their support, as there is then nothing to drive.

Armature Excitation.

48. With leading brushes the armature current opposes the field current. But with no load, leading brushes assist the field current under suitable conditions. That is to say, the effect with leading brushes is at no load the reverse of what it is at full load.

Consider a dynamo running without load. If the brushes are given a slight lead, as in Fig. 14 A, a current flows through the

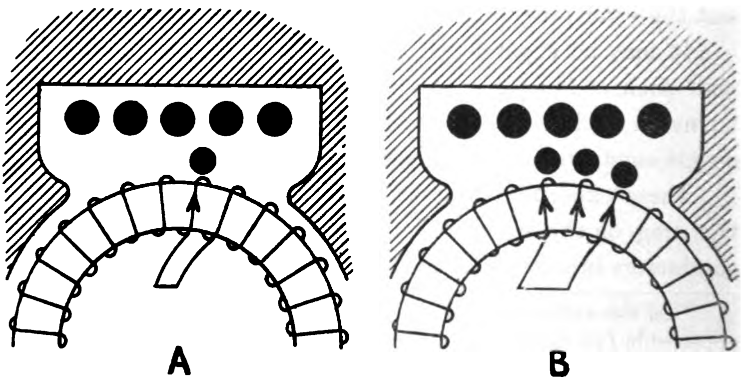


Fig. 14.

short-circuited coils in such a direction as to assist the field. Further, if a brush is made to span a number of sectors, as in Fig. 14 B, this assisting short-circuit current may be made to flow not only through the coils in advance of the neutral line, but through some of the coils behind the neutral line. To get this latter result it is only necessary that the short-circuit should include a greater portion of the winding in front of the neutral line than behind it: then the E.M.F. of the forward coils will overcome that of the backward coils, and a current will be forced through the latter contrary to their E.M.F. It will also be obvious that if the coils behind the neutral line are short-circuited by a brush a demagnetising current will flow, which in a similar way may be made to extend to the front of the neutral line. The

increase of excitation obtainable in this way is considerable. It was found quite easy to raise the volts of an ordinary dynamo 20 or 25 per cent. A much greater increase could be obtained momentarily, but excessive sparking made it undesirable to continue the experiment. Mr Mordey.

49. This effect is exactly the reverse of the short-circuit effect under the brushes referred to in the section of this paper headed "Brushes and Collection." But the two effects are not contradictory. In one case it is the working current in the armature behind the brush, and the continuance of the same direction of current under the brush, that opposes the field winding. The direction of current in front of the brush is always assisting; it is in this latter direction that the short-circuit current is set up under the conditions named, and the result is, naturally, to strengthen the field.

50. A rather interesting difference between the conditions of the magnetic circuit on the two sides of a dynamo may be referred to here. It was found, by experiment with a single-magnet Gramme dynamo, that, although the increase of volts was considerable when a wide brush was placed on the upper side, no further increase occurred when a similar brush was put on the lower side. Fig. 15 shows a single and a double horse-shoe form of magnet. In the former, the armature current in the magnetising space on the upper side is acting on a magnetic circuit, shown by the line L, which passes mostly through iron. On the lower side, however, the line L′

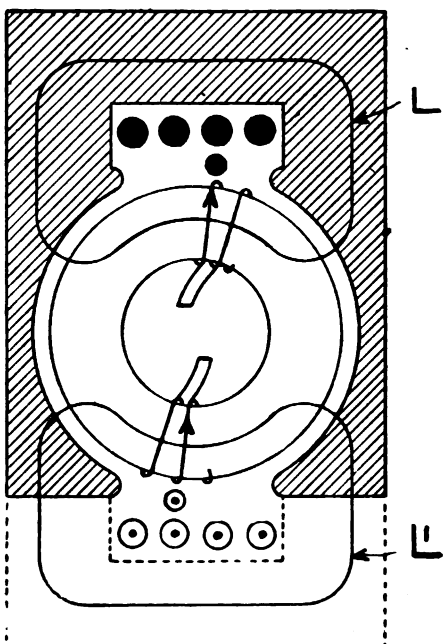


Fig. 15.

Mordey. passes mainly through air, and consequently the effect of the current is not so great. This difference will, of course, be most marked in small air space dynamoes.

In a double horse-shoe magnet, as shown dotted, the conditions on the two sides are similar.

Effects of this kind are best studied with a Gramme armature, as the short-circuit caused by the brush is confined to the coils on a definite portion of the ring. In a drum, the short-circuited coils pass round the armature, and consequently the separate effects in the two magnetising spaces cannot be so well observed.

This subject is referred to only as illustrating armature reaction, and not for any immediate purpose of practical utility. It may, however, suggest a useful result to some ingenious person.

Internal Fields.

51. The consideration of armature windings which reduce the amount of field winding required, and a recognition of the proper function of the field magnet, lead naturally to a revived interest in internal field magnets such as were introduced and advocated long ago by Professors Ayrton and Perry.* There is now much to be said in favour of such arrangements. In those machines the armature and commutator were fixed, while the brushes and the internal shuttle-shaped magnet revolved. One only shrinks from recommending a return to such constructions because we have become accustomed to stationary brushes capable of adjustment while in use. But it would be unwise to sacrifice too much to retain this convenience. Our dislike to revolving brushes is to a great extent a legacy from the spark-age. If machines can be made to work sparklessly with brushes in a fixed position relatively to the field, then the main objection to revolving the magnet and the brushes is removed. The objection, even now, has not much force, as is evident from the common practice, as in tram-cars, of running motors where they cannot, or do not, get any attention whatever when running.

52. Internal field magnets have been largely used on the

* *Journal*, xii., p. 335, May 10th, 1883.

Continent, the field magnets being fixed. Siemens & Halske and others have made very fine machines of this kind. Usually the outside of the armature is made to serve as commutator, but sometimes a commutator of ordinary form is used. As a type it is at present more especially suited to large sizes, because of the difficulty with small sizes of getting sufficient internal space for the magnet; but this difficulty disappears to some extent if we can reduce the amount of field winding and the cross section of the field core. Mr. Mordey.

If this paper has seemed needlessly long, indulgence is asked on the ground that the object has been to make the explanations clear, not to experts only, but to all who take an interest in and have a general knowledge of the subject.

My thanks are due, amongst others, to Mr. L. Wilson and to Dr. Fleischmann, and in particular to Mr. A. G. Hansard, whose very able and willing assistance has been of great service to me in the preparation of this paper.

The PRESIDENT: The discussion on Mr. Mordey's paper will be commenced at our next meeting, on Thursday, the 28th inst. That will be our last meeting for the session, and the whole of the evening will be given up to the discussion of this paper.

The PRESIDENT announced that the scrutineers reported the following candidates to have been duly elected:—

Associates:

John S. Bowman.	Walter Riggs.
William J. Dale.	Thomas M. F. Tamblyn-Watts.
Harry Philip Gaze.	Albert Edward Whitaker.
George C. Laird.	Walter H. Whichello.

Students:

Robert William Hodges.	John S. Martin.

Henry Kirby Rodwell.

The Three Hundred and Fourth Ordinary General Meeting of the Institution was held at the Society of Arts, John Street, Adelphi, on Thursday evening, May 27th, 1897—Sir HENRY MANCE, C.E., President, in the Chair.

The minutes of the Ordinary General Meeting held on May 20th, 1897, were read and approved.

The names of new candidates for election into the Institution were announced, and, this being the last meeting before the recess, it was, on the motion of the PRESIDENT, agreed unanimously that the candidates should be balloted for that evening.

The following transfers were announced as having been approved by the Council:—

From the class of Students to that of Associates—

A. S. Cubitt.	Frank Johnston.
William Fennell.	George F. Malden.
Frank T. Hamilton.	Alfred Lovell Phillips.

Frederick S. Spiers.

The PRESIDENT: I will now call upon the Secretary to read the terms of the Address of Congratulation from the Institution to Her Majesty the Queen on the attainment of the Sixtieth year of her reign. The Address, illuminated on vellum, is on the table before you.

The SECRETARY read the Address, as follows:—

TO

HER MOST GRACIOUS MAJESTY

VICTORIA,

QUEEN OF GREAT BRITAIN AND IRELAND,

EMPRESS OF INDIA.

MAY IT PLEASE YOUR MAJESTY:

We, Your Majesty's loyal and faithful subjects, the President, Council, and Members of the Institution of Electrical Engineers, desire humbly to offer to Your Majesty our heart-felt congratulations on the happy and

auspicious event of Your Majesty's attainment of the Sixtieth year of Your Majesty's reign.

We gratefully acknowledge the many blessings which, in common with all Your Majesty's subjects, we have been permitted to enjoy under Your Majesty's beneficent sway; and especially do we bear in mind how, under Your Majesty's encouragement, and that of His Royal Highness the late deeply lamented Prince Consort, the Arts, Sciences, and Manufactures have developed and flourished during Your Majesty's reign.

In no other branch of Applied Science has greater progress been made during the last sixty years than in that with which we are specially connected, and of which this Institution is the recognised representative Society in this country.

The Electric Telegraph, the practicability of which was first established by Cooke and Wheatstone in the year of Your Majesty's accession to the Throne, has now, by means of submarine cables, been extended to every part of Your Majesty's vast dominions, and has been the means of bringing into close communication with the Parent Country and with each other Your Majesty's subjects resident in the most distant parts of the Empire.

The Telephone, the Microphone, Electric Lighting, Electric Traction, and the Electric Transmission of Power are among the other chief applications of Electrical Science which have all been created and developed during Your Majesty's reign, and have largely promoted the happiness and convenience of Your Majesty's subjects.

We earnestly pray that the Almighty may vouchsafe to Your Majesty Life and Health for many years to come, whereby Your Majesty's subjects may continue to enjoy the inestimable blessing of Your Majesty's beneficent rule.

By Order.

HENRY MANCE, *President.*

June, 1897. F. H. WEBB, *Secretary.*

The meeting unanimously signified their approval, and the Address was accordingly adopted.

The PRESIDENT: We will now commence the discussion on Mr. Mordey's paper on "Dynamoes."

Prof. Thompson.

Professor SILVANUS P. THOMPSON: This Institution is fortunate in having brought before it, even at so late a period of the session, a paper of the practical importance which Mr. Mordey's paper possesses. One has had to deplore, during the two years, the fact that important contributions to the progress of electrical engineering were made known in other Institutions rather than this, indicating, I fear, a want of patriotism on the part of some of the members of this Institution. Happily, Mr. Mordey has been faithful to us, and has given us an opportunity of learning first of this advance which he has made, and of discussing the value of that advance. It has often been said that the magnetism in a dynamo is required for the armature; and that the field magnet is only something which we, unfortunately, cannot do without, and which is used for driving magnetic lines to the place where they are wanted. This idea led to the design of iron-clad machines having the winding directly in the place where it was most advantageous. This idea dominates the greater part of the illustrations which Mr. Mordey has given us. Many a time and oft Mr. Mordey and I discussed very closely the question of what we were then calling "cross-"compounding." We discussed a compound winding—that is to say, a thick wire winding in series with the armature to take the main current, wound, not upon the limbs of the field magnet merely to strengthen the field, but wound across from one limb to the other over the armature; compounding, not in the direction of the magnetic lines, but across, so as to counteract the cross-magnetising reaction; in fact, to combat cross-magnetisation by a counter cross-magnetisation. This notion of cross-compounding and of over-cross-compounding we discussed very deeply. But before our ideas could be made of any value there came out the suggestion of Professor Ryan, of using a cross-compound winding sunk in channels in the pole-pieces. With a cross-compound winding it is quite possible to fix the

brushes once for all, and, instead of altering the lead of the brushes for variations of load between no load and full load, to alter electrically the amount of cross-compounding; in fact, instead of shifting the brushes fore or aft, shift the resultant lines of magnetisation by merely shunting, more or less, the current going round the cross-compound circuit, thus substituting electrical regulation for mechanical regulation. But on the top of Professor Ryan's methods there came those two papers given to this Institution two and three years ago by Mr. Sayers, in which he made public the method of compensating for distortions due to the armature current by employing a second winding on the armature—the method, in fact, of compounding the machine by special commuting coils on the armature, instead of compounding it across on the field magnet. I ventured to characterise the first of the two papers which Mr. Sayers read as the most important contribution that there had been for many years to the theory of the continuous-current dynamo. Now we have Mr. Mordey following on in the same order of ideas, operating, not on the field magnet, but on the armature itself, with its own currents, to restore the disturbed equilibrium, so as to introduce such compensation as will enable you to avoid the variable lead that has been hitherto necessary to give to the brushes in order to secure sparkless collection. It is a most interesting thing to see with what an extremely small and simple alteration so much can be attained. Take the case of the modification of the Gramme ring. That modification is a mere spacing-out of each group of winding in two lots, as in the lower Fig. 2, as compared with the upper Fig. 2—a change so excessively simple that it might have been done any time since the year 1870, when Gramme rings were first made public. It is a thing that any fool might have thought of, but that no fool did think of till the year 1897. If the test of a great invention is its simplicity, then the fact that this invention is so exceedingly simple is a proof of its greatness; it further proves that it requires a wise man to think out that which no fool has thought of. It is very easy now to be wise after the event. It is exceedingly difficult when one has once seen this simple solution of many difficulties to say, "Why didn't

Prof. Thompson.

Prof. Thompson.

"we see that before?" It is so exceedingly obvious that we did not see it. How many of us have been trying for years and years to solve this problem of attaining sparkless collection with the invariable position of the brushes! and how many of us have tried and failed! Here is a solution so simple that you might have in your hands an armature wound in this way, and would not know that it was wound in any different way from that in which armatures have hitherto been wound since 1870. Incidentally, there is a point of very great interest in this paper—in Mr. Mordey's little experimental investigation to prove that there is no drag* on a conductor carrying a current when that conductor is sunk deep between teeth, or put in slots, so that it does not actually lie in the magnetic field. In such cases the drag comes on the teeth instead of on the conductor. I have said much about that in past years, and I have been told that I was hopelessly wrong; that a conductor could not possibly go round between the teeth of an armature and do any useful work for cutting magnetic lines, unless it was itself in a magnetic field, and therefore subject to a drag. I satisfied myself years ago that such was the case—not by special experiment, but by trying the effect in coils situated as they are in transformers. The drag that comes on a coil when it is unprotected by teeth, is a totally different thing from the comparatively small drag that comes upon it when the teeth take the majority of the magnetic lines. In Mr. Mordey's case the drag is reduced to 1-10th by sinking the coil in a slot between the teeth. This must not be taken as proof that under no circumstances is there absence of drag. What it proves is that the coil in which there was a drag of about 1-10th of the full amount, was to that extent not completely protected by iron; it was only protected to the extent of 9-10ths. Had this protection been more complete—the slot being narrower or deeper, or the iron better or less saturated—that drag would have been reduced to a still smaller amount. But as there are some here who still hold the view that it is impossible to protect from drag the windings of an armature by sinking them between teeth, I

* See "Dynamo-electric Machinery," 5th edition, p. 99.

shall be very curious to hear whether that opinion is going to be upheld, now that Mr. Mordey has so distinctly given experimental reasons to the contrary. This has a bearing, obviously, upon the construction of many forms of machines—not the least important on the construction of alternators. But there are very few alternators now built in which the armature coils are not sunk between teeth, or in actual holes in the core discs. The fact that sunk windings are now so universal is in itself proof that the constructors have at last realised that the winding when not sunk is exposed to forces which do not occur when it is protected by these slots. Prof. Thompson.

Mr. G. ADAMS: I must congratulate Mr. Mordey upon having produced so instructive and valuable a paper on dynamos. I do not quite agree, however, with Mr. Mordey when he states that the modern dynamo is a crude and unsatisfactory machine. Machines as made at present certainly are well-finished products, and frequently give excellent results. But, considered as an apparatus for producing current by the most economic disposition of materials, they cannot be said to have reached perfection. Broadly speaking, Mr. Mordey's paper is a condemnation of the smooth-cored armature, and he argues in a very convincing way that the machine with smooth-cored armature has not a limb to stand upon. On the main points of his arguments most electrical engineers will agree. Since these arguments have been brought forward by one so practically conversant with the design of dynamos as Mr. Mordey, we can scarcely do otherwise than accept most of his conclusions; and the question may well be asked, Why are dynamos with smooth-cored armatures made in England in such large numbers at the present time, notwithstanding such improvements as suggested by Mr. Mordey have been put forward? I think the answer will be, Because of the difficulty of obtaining good steel castings for field magnets. The use of tooth-cored armatures generally necessitates a more complicated form of field magnet than can be constructed of rectangular slabs of wrought iron. So long, therefore, as we are restricted to the use of wrought iron there can be very little improvement in the design of dynamos and motors. Electrical Mr. Adams

Mr Adams. engineers and steel-makers ought, therefore, to unite in overcoming this difficulty. Mr. Mordey brings out in a very forcible way the advantages to be gained by the use of tooth-cored armatures, especially with regard to economy in materials. In this connection I venture to draw the attention of the meeting to the diagram which I have fixed on the wall, which shows a tooth-cored Sayers dynamo side by side with a modern smooth-cored dynamo having an armature of approximately the same size. The smaller quantity of material required for the Sayers machine is made very apparent on this diagram. On page 533 Mr. Mordey states that the more we succeed in reducing sparking and armature reaction, the more important becomes the question of cooling and ventilation. Ought not the matter of cooling and ventilation to be of less importance with the types of machine which Mr. Mordey has so ably described? We are able, in such machines, to use smaller field windings, so that the $C^2 R$ losses would be less; tooth-cored armatures are also much more efficient radiators of heat than smooth-cored machines. The question of ventilation would therefore appear to be less serious than is the case with ordinary dynamos. On page 534 Mr. Mordey states that when a tooth-cored machine is loaded up the air gap must be increased in order to check the armature reaction, with the effect of increasing the reluctance of the magnetic circuit. This statement is apparently borne out by the comparative figures given by the author. I am sure it would be interesting if Mr. Mordey, or any other gentleman present, could give the reason for the increased armature reaction which is stated by Mr. Mordey to be the result of diminishing the air gap. In the comparative table quoted on page 535, the ratio of ampere-turns to output is about 133 to 1 for the railway generator, and about 600 to 1 for the Edison-Hopkinson machine—a result which indicates in a striking manner the superiority of the tooth-cored machine. The difficulty of sparking having been so completely overcome in the ordinary dynamo, it appears probable that the important desideratum of sparkless collection will also be obtained with tooth-cored armatures when more experience has been obtained in their design and construction.

Mr. W. B. SAYERS: I wish to join in thanking Mr. Mordey for having brought this subject before us to-night. I will concentrate my attention on the main point, which is the new winding. Considering the very generous and pleasant way in which my own efforts in this direction were received, both by Mr. Mordey and many others, I feel in a somewhat unenviable position in being rather out of accord with Mr. Mordey in this connection. The fact is, I have studied this arrangement of Mr. Mordey's, and I am quite unable to see that it can possibly produce any advantage—at any rate, for anything but very small machines. I quite think that the study of the particular machine which Mr. Mordey has experimented upon would reveal the reason why, in that particular case, some advantage was shown. It is a matter of common knowledge to many of us that a certain amount of reversing electro-motive force is necessary if we are to get sparkless collection. Mr. Mordey says that by dividing his coil into two parts, as shown in Fig. 4 (Mordey), he gets opposing electro-motive forces in the one coil, and, secondly, he gets no sparking. So long as there is no current in the armature, that is perfectly true. The electro-motive force in two trailing turns, for instance, exactly balances the electro-motive force in the two leading turns, provided the brushes are set exactly between the two poles; and when the current is practically zero, as it is on light loads, there will obviously be no sparking. But the moment you get a load on—assuming that the armature is running clockwise—the left-hand horn (see Fig. 4, Mordey) is strengthened, and the right-hand horn weakened. Now, unfortunately for Mr. Mordey's arrangement, the right-hand, or "leading," horn is the one which he must reverse with. It is a matter of impossibility, so far as I am able to see, to commutate with the "trailing" horn. The electro-motive force is in the wrong direction, and no arrangement of windings or anything else will make it right. If, for instance, you put one turn in one direction and one in another, then you balance out your electromotive forces and the machine does not work. If you divide your coils into two equal parts, the result is you have a balance in the wrong direction, the "trailing" horn is strengthened, and your Mr. Sayers.

Mr. Sayers. reversing horn is weakened, and consequently your electromotive forces do not balance out. Instead of having a positive E.M.F., you have a slight negative E.M.F. If you shift the brushes forward in order to get your coils away from the "trailing" horn, and work with what E.M F. you have under the "leading" horn, I admit you can commutate a coil, but what you have done is to halve your commutating power. If you had wound the whole of your coils in the same plane, then you would have four coils under the reversing horn instead of two. Now, with regard to Fig. 10B, we see there a winding so arranged that there is a direct magnetising power due to the armature. One black dot just balances out one circle with a dot in it, and the other two, taking the top side, are magnetising coils. But the expense at which that arrangement is obtained is this—so far as commutation is concerned: If the machine is running in a "counter-clockwise" direction, the only bar available for commutation is the one under the "leading" pole; you have three bars against one as regards commutation. I speak with the knowledge that humanity is fallible, but I feel perfectly confident that that arrangement would not work with a machine of any size as machines go nowadays. Now I pass to another point, and that is, Mr. Mordey says that 1-10th of the present exciting power would amply suffice to magnetise the iron of the magnetic system, and the gap sufficient for clearance. I think Mr. Mordey has gone a long way too far. In my experience, about 50 per cent. to even 70 per cent. of the magnetising force is required for an air gap. I have found that, from mechanical and other reasons which I will explain, it is impracticable—undesirable, that is—to work with very short air gaps. Fig. 1 (Sayers) refers to a machine having the shortest air gap which I have ever used. The particular machine was made for the Midland Railway Company. It has been described before this Society. The air gap was 1 mm., but the machine was a motor generator—a "booster," in fact—and the field was comparatively weak. I do not know that that machine would have run for any time if the fields had been very strong, for the simple reason that a slight difference in the air gap on the two sides causes an enormous

magnetic strain to one side; and, in fact, unless everything is excessively rigid, the force gathers up so tremendously that the Mr. Sayers.

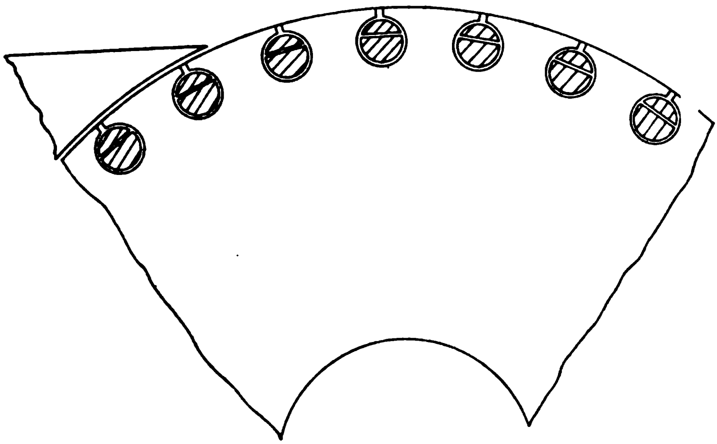


FIG. 1.

armature may go over to one side and stick. I have found that an air gap of about 0·15 of an inch for moderate-sized machines, up to 0·25, 0·375, 0·5 inch, are workable. For instance, Fig. 2 (Sayers)

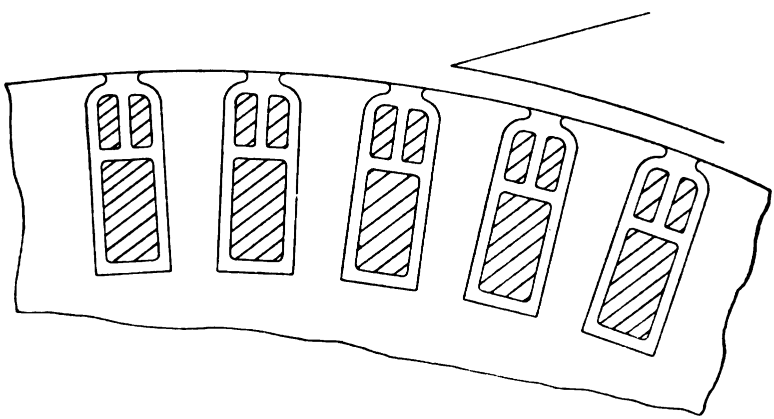


FIG. 2.

represents a 270-kilowatt machine which is now working. The air gap is 0·375 inch. I am using 0·5 inch on some 380-kilowatt slow-speed bipolar machines upon which I am now engaged. Another important point is that the air gap must be in a definite

Mr. Sayers. relation to the width at the top of the slot. That is a point to which I have drawn attention before. If the opening at the top of the slot is too wide, eddy-currents are generated in the field poles; therefore, if it is wished to use the very narrow air gap, the fields must be laminated—which is out of the question for commercial reasons, as a rule—or, as is shown in Fig. 1 (Sayers), the iron must be almost continuous round the armature conductor. Now, making the iron continuous round the armature conductor has this objection: the position of the brushes must be rather nicely adjusted. A slight error of adjustment upsets the arrangement, and more or less E.M.F. is generated than is required, consequently sparking occurs. Fig. 2, I may say, refers to a 270-kilowatt machine which is doing electrolytic work very satisfactorily. Some are at work, and others building by Messrs. P. R. Jackson & Co., of Manchester. I remarked that the closing of the top of the slot increases the self-induction. I have found it better to make the slots parallel when possible. In the 270-kilowatt machine the slots were contracted at the top in order to comply with the air space which we desired to use. I have recently adopted the arrangement shown in Figs. 3 and 4

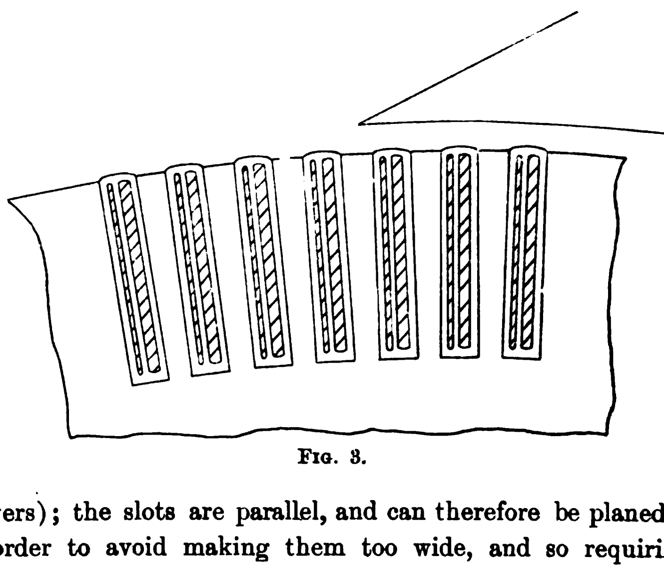


Fig. 3.

(Sayers); the slots are parallel, and can therefore be planed out. In order to avoid making them too wide, and so requiring a greater air gap, I put my commutating coils into separate slots.

The two bars in separate slots are main coils; the two thinner ones together in one slot are bars forming commutating coils. Mr. Sayers.

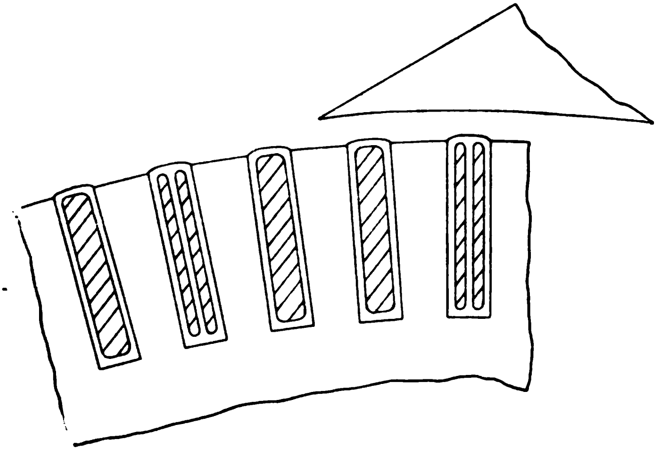


Fig. 4.

Another advantage gained by this arrangement is that the bar

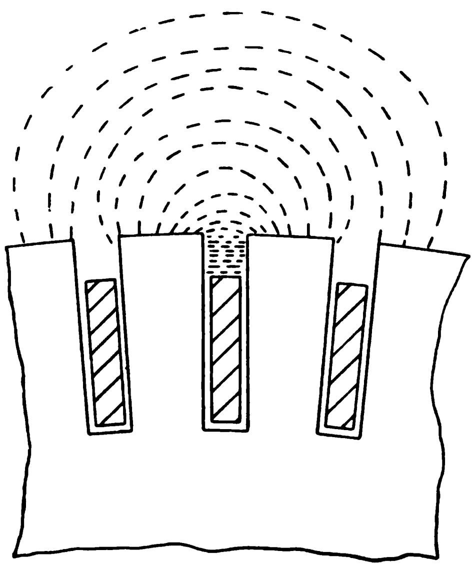


Fig. 5.

occupies the whole depth of the slot. The back E.M.F. of self-induction varies very greatly with the proportions of the slot and

Mr. Sayers. the number and arrangement of the bars in it which are undergoing reversal at the same time. Other things being equal, it is more difficult to commutate an armature with a number of bars per slot than one having only one bar per slot. The back E.M.F. of self-induction varies nearly as the square of the number of bars per slot undergoing simultaneous reversal. Looking at Fig. 5 (Sayers), the field generated (which, with the time element added, is the self-induction) by the middle conductor is represented by dotted lines. Magnetic lines would also be generated through the bar itself as indicated in Fig. 6 (Sayers); but those

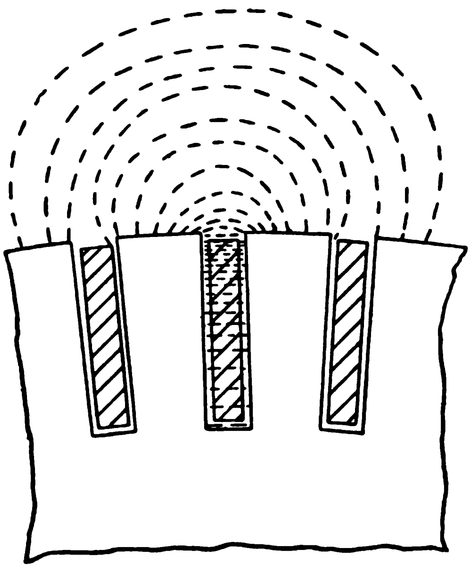


Fig. 6.

passing through the bar do not produce any E.M.F. along the length of the bar, but only a swirl or eddy in the bar itself, the energy represented by which, however, is quite negligible. Consequently, it is advantageous to bring the bar right to the top of the slot, as shown in Figs. 3, 4, and 6 (Sayers). I agree thoroughly with Mr. Mordey in the points which he brings forward as to the advantages of tooth-core machines. With regard to what he says about the drag on armature conductors in slots, I thoroughly agree. Mr. Swinburne drew my attention to it some

years ago. What really happens is that the armature conductors Mr. Sayers. are moving in a very weak field—a very attenuated field; that field is flying across the slots in the opposite direction to which the armature conductors are travelling: it is just as though the armature were running at 10 times the speed in a very weak field. I am very glad to see that Mr. Mordey has come round to my way of thinking with regard to cheapness. When I read my paper before this Institution, I urged very strongly that my machines with short air space and slotted cores were cheaper to build than the ordinary smooth core type, and I see Mr. Mordey has come round to that view. With regard to machines with a number of bars per slot, I have said these are more difficult to commutate than those with one bar per slot. As regards the bottom conductor, the self-induction is represented by the field generated in the portion of the slot above it, plus that over the surface; in the second conductor, by that in the portion above it, plus that over the surface; and so on. The self-induction of the bottom bar may thus be several times as great as that of the top one, and this cannot be avoided; but, seeing that the effective self-induction is almost entirely due to the field generated in the paths I have referred to, the square factor may be eliminated, by distributing the several bars or wires forming a section of the winding over a number of slots. Figs. 7 and 8 represent such a winding.

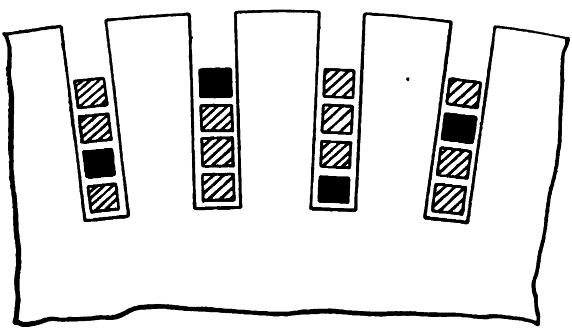


FIG. 7.

Referring to Fig. 7, the bars shown black in section are those which undergo reversal at the same time under the respective brushes. Fig. 8 is a diagram of a winding in which this result

Mr. Sayers.

NOTE.—The — —— — —— lines indicate the sections of the winding in which the reversal of the current is about to take place under the respective brushes. The thick lines indicate the sections in which the reversal has just been completed. If the direction of motion were opposite to that indicated by the large arrow, the thick lines would indicate the sections in which the reversal was about to take place, and the — —— — —— lines those in which it was completed. Thus the zone of reversal is always that embraced by the large ⁀‿ in whichever direction the machine may be run, and whether as generator or motor.

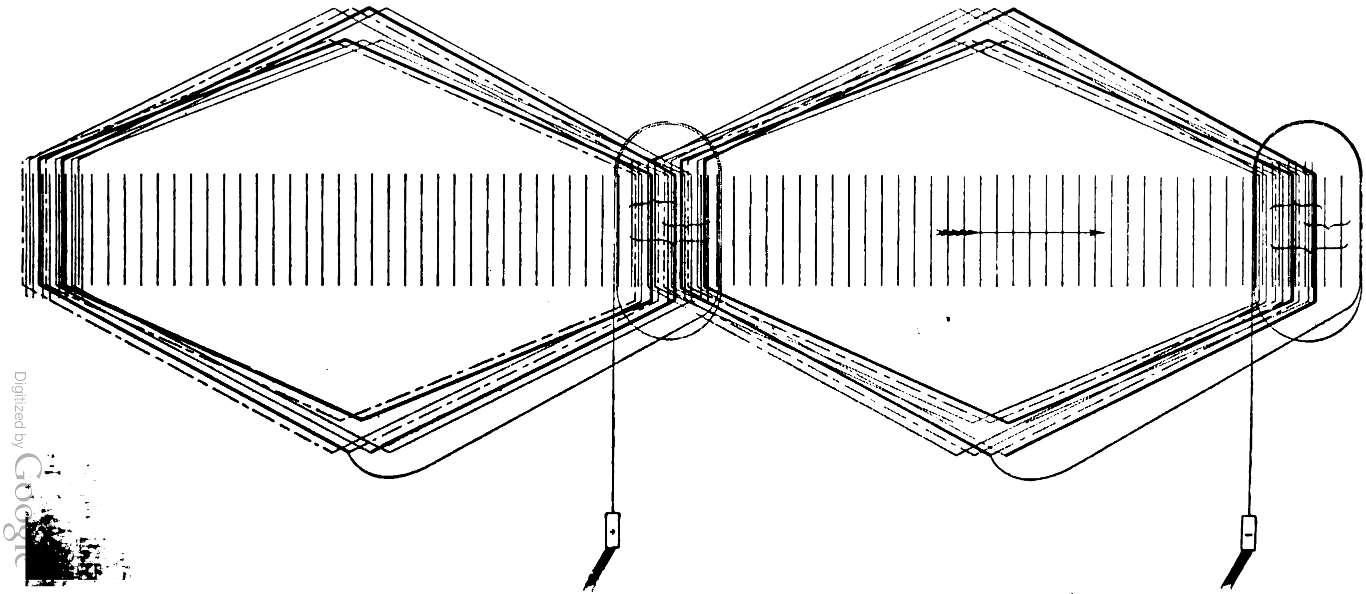


FIG. 8.

is accomplished. It is really a sort of chord winding. On page 12 Mr. Mordey makes some remarks—which I will not refer to in detail—on the current generated in the short-circuited section under the brush. I had gone into that very carefully at the time that I wrote my last paper, and I came to the conclusion that you cannot have an abnormal current in the short-circuited coils under the brushes in a machine which is not sparking. If the brushes are so far back that the current in the coil increases when it becomes short-circuited, there is no chance of reversing inductively; it simply runs out from under the brush, with an abnormal current flowing in it, and sparking occurs. That is what happens when the brushes are too far back. On the other hand, if the brushes are too far forward, a current is generated in the right direction, but it is bigger than the normal current; you get reversal overdone, and that must spark also. I believe, of the two, running out of circuit with the commutation overdone is the less damaging. I do not know whether I am misrepresenting what Mr. Mordey intended. It appears to me to be absolutely impossible for any big current to be generated in the section under the brush, unless the machine is sparking. With regard to Fig. 12, I would like to point out to Mr. Mordey that the copper brushes, which are supposed to be separated by carbon slips, do not really interpose a resistance in the way in which one would at first suppose, for the simple reason that the copper brushes themselves short-circuit the section. It is an intermittent affair. The resistance is in circuit for a moment, and then the brush bridges over the mica, and the section is short-circuited, just as though there were no resistance there at all. Mr. Sayers.

[*Added 24th June.*]—Since the discussion on Mr. Mordey's paper, it has occurred to me that the explanation of the improved results obtained by Mr. Mordey with his new winding applied to the 15-kilowatt machine is largely contained in my contribution to the discussion. The effective self-induction of a section being proportional to the square of the number of turns in one slot undergoing simultaneous commutation, it follows that, by dividing the four-turn coil into two equal parts, disposed in slots distant from each other, the self-induction of the coil will be

Mr. Sayers. halved, while at the same time half the conductors will be available for commutating power. On this account, therefore, the ratio commutating power to self-induction remains the same. But the leading horn will be stronger with Mr. Mordey's winding on account of back turns being non-existent, thus giving an advantage in favour of Mr. Mordey's arrangement. When the same reasoning is applied to the case in which he puts only one turn forward and three back, it is found that, instead of halving the self-induction, he only reduces it in the ratio of 16 to 10, while the commutating power is quartered. It is possible with a large gap between the poles that the magnetising turns on the armature should keep the leading horn up to a strength several times as great as would have been the case had all the turns lying between the poles been back turns, and this apparently was the case in the machine upon which Mr. Mordey made his experiments.

I therefore do not agree with Mr. Mavor that the same result would be obtained by bringing the pole-tips very near together, but I see no reason to change my opinion that the arrangement is only applicable to small machines.

As regards fixed brushes with Mr. Mordey's winding, perfect inductive reversal cannot be realised with this condition, because the reversing horn is still weakened, instead of being strengthened, by the armature current. In this connection I may say that I have recently succeeded in devising a perfect form of reversing pole for use in my machines, by means of which the reversing field is strictly proportional to the load on the armature. I cannot at present describe the device, but it has been tested experimentally with complete satisfaction, and a number of large machines are in course of construction in which it is to be applied.—W. B. S.

Mr. Esson. Mr. W. B. Esson: I am very sorry to see that Mr. Mordey has disfigured his paper by the woeful spelling of the title. Why Mr. Mordey wishes to change the recognised spelling I do not know, though it certainly appears from the paper that "dynamoes" spelt with an *e* have sometimes different characteristics to dynamos spelt in the ordinary way. I do not

think Mr. Mordey is quite strong enough for this kind of thing. Mr. Esson.
If he had been a Professor of Philology, no doubt the new spelling would have been hailed with acclamation, and Mr. Mordey would have been recognised as a learned *savant* by his professional brethren; being only an electrical engineer, however, I am afraid that his colleagues will regard him, as far as this matter of spelling is concerned, as only something of a crank. But I must say this for "dynamoes" spelt with an *e*—that they form a very fitting companion for Mr. Mordey's "periodicity," which of all the terms in our electrical vocabulary is perhaps the most malappropriate. He also seems to be unfortunate in his choice of expressions, for I notice in the present paper he uses the term "magnetising space." Now what, in Heaven's name, is magnetising space? Magnetising coil I can understand, also *magnetised* space; but magnetising space I consider wholly unjustifiable.

Coming to the matter of Mr. Mordey's paper, what does it contain? It contains a lot of history—ancient history—some of it very ancient history. If Mr. Mordey will refer to the pages of the *Electrical Review*, he will find that in 1885 I wrote some articles dealing very fully with the advantages and disadvantages of toothed armatures.* I pointed out how they required less magnetising force than ordinary armatures, and also some other things which Mr. Mordey has omitted. Amongst these was the heating of the poles owing to the disturbing action on the polar surface of the teeth—a circumstance which necessitated a somewhat larger air gap than would have been otherwise required, or which to some extent discounted the advantage, which would have been otherwise derived from teeth, in reducing the magnetising force. I will just read the last sentence from my articles in the *Electrical Review*. It reads: "In con-"clusion, then, it appears that, after due allowance has been made "for the prevention of wasteful currents" (that referred to the currents in the pole-pieces), "we have in armatures of the "Pacinotti type, if constructed in accordance with the data here

* "Dynamo Armatures," *Electrical Review*, vol. xvii., 1885.

Mr. Esson. "given" (the data given meant allowing an air gap the dimension of which was fixed with relation to the width of the armature slots),* "the resistance of the air space reduced "about 30 per cent. The clearance, considered simply as "mechanical clearance, is rather excessive, but the reason for "this now appears, and the double advantage of being able to "secure at the same time an excessive mechanical clearance and "a more efficient armature will be recognised. Finally, if we "remark in addition to these advantages the impossibility of the "wires moving on the core, we conclude that, mechanically and "electrically, there is a distinct gain in employing Pacinotti's "armature." You will see, then, that 12 years ago we were keenly alive to the advantages of the toothed armature. Since that date splayed teeth have come into use—teeth expanded on the outer surface, as shown on Mr Mordey's Fig. 4. Also, the method of winding the conductors in channels or holes as shown in Mr. Sayers's diagram has come into force; and these modifications to some extent do away with the necessity for having an extra large air gap, as there is no disturbance of the field over the pole surface. Knowing all about the advantages of the toothed armature at that date, the question is, why could we not continue making it? and the answer is, that it was discontinued on the ground of expense. The machines worked admirably in every way; there was no sparking, and no excessive heating. The slots were numerous and narrow, and there was nothing to choose between these armatures and those of the best present-day machines. On looking up my note-book, I find, strangely enough, that the 13-inch armatures had 48 slots—the exact number in Mr. Mordey's Gramme-wound machine. But there was too much work in them, and they were not cheap to build. I do not say it is not possible to construct a toothed armature machine cheaper than a smooth armature, but I do join issue with Mr. Mordey when he says that, as a class, the toothed armature is cheaper than the other; that depends entirely on the size.

* Mr. Blathy tells me that in Ganz's machines the air gap is equal to half the width of the armature slots. This is for steel. My practice was to make the gap equal 0·35 of the width.

Again, there is considerable disadvantage, in this country at least, in the making for stock machines with toothed armatures. You will find that every customer requires a different output or speed, and the consequence is you cannot stock the carcasses with advantage. If every armature wants a new set of teeth, machines cannot be built economically. Mr. Esson.

I also wish to call Mr. Mordey's attention to two papers which were read in this Institution by Mr. Swinburne and myself in 1890. If he reads those papers through, he will find that almost every matter he has touched upon in his present paper was then discussed. Mr. Swinburne treated of small air gaps and chord winding, noticing the advantages and disadvantages of the latter; also, he discussed sparking, and mentioned many different devices for its prevention. In my paper were discussed the symmetry of the field, the different effects on the field produced by the lead of top and bottom brushes in single-field machines, noting the dissimilarity, and also pointing out the effect on the field, of the current in the short-circuited armature coils, whether the machine was loaded or unloaded, and whether the brushes had a lead or a trail; all this is very old.

Reviewing the past, it has often amused me to note the way in which Mr. Mordey has endeavoured to spring little surprises upon this Institution. One day Mr. Mordey comes down and tries to persuade us that his alternator has no self-induction; another time he will come and tell us that transformers have no hysteresis loss at full load. Now he comes down to try and persuade us that the proper thing is chord winding; that we must have small air gaps; and that we must adopt a tooth armature. I am afraid he will fail in convincing us on these points, as he has failed in convincing us on the others. The whole thing from beginning to end is simply a question of cost.

It is very true that armature reaction is responsible for a good deal of power waste, but it seems to me—and Mr. Mordey admits it in one part of his paper—that the smooth armature and the toothed armature come to about the same thing in the long run, at least for small machines. In large machines the question of cheapness in labour does not come in so much, as the cost of

Mr. Esson. material is the more important thing, but it is otherwise in small machines. The cost of slotting the armature is negligible in large machines, but in small I question whether the toothed-armature type is the cheaper to build. I did not find it so, though for large railway generators or railway motors subjected to great and sudden strains the benefit of teeth is undoubted.

As a matter of fact, the chord winding does not prevent sparking; it simply saves some copper on the fields, and the question is, how much? Well, one can easily calculate it, and there is not a great deal of saving. Again, it may even produce the sparking it is designed to cure, as, instead of having a neutral surface, we are reduced with the chord winding to a neutral line, and have no margin to work on.

We are told that the molecules of the human body are completely displaced once in seven years; so that the Mr. Mordey who addresses us now is not the Mr. Mordey of seven years ago; and, indeed, it is difficult to conceive how he could be the same man. I recollect quite well the discussion which arose when Mr. Swinburne and I read our papers in 1890. Mr. Mordey came down in his best form, prepared to dispute everything. He objected to the small air gap; he objected to the various devices for preventing sparking; he objected to the chord windings; and declared that Messrs. Ayrton and Perry, by their heretical writings, had greatly hindered progress in the particular departments of dynamo and motor design. Mr. Mordey comes down to-night, and what does he tell us? He tells us that we must have toothed armatures, we must have chord windings, we must have short air gaps; and in the last paragraph he pays a tardy compliment to the excellent work which was done by Ayrton and Perry 15 years ago. I do not know how it may be with Mr. Mordey's physical constituents, but this I will say—that seven years exactly marks the time which it takes Mr. Mordey to turn a mental somersault.

In Mr. Mordey's paper there is, however, something original, and that is the part which relates to the drag on buried conductors. I am very glad indeed to find that Mr. Mordey has at last given us some definite information about this. We learn now that the drag on the conductor is not anything like what it

would be if it were on the core surface; nor is it quite *nil*, as some would have us suppose, but that it is something between the two. In conclusion, I think the thanks of the Institution are due to Mr. Mordey for bringing such a large amount of important matter within so small a compass. I have been extremely interested in the paper. It seems like old times at the Institution to hear all these things thrashed out anew; but before I sit down I would like to say to Mr. Mordey that I think there is still more scope for his genius in improving the dynamo than in improving its spelling. Mr. Esson.

[*Added 4th June.*]—I notice in the published reply of Mr. Mordey the statement that I had long endeavoured to make good toothed armatures, but had not succeeded. This statement is not in accordance with fact, and would seem to show that Mr. Mordey's knowledge of what has been done is but co-extensive with his knowledge of what has been written. As a matter of fact, I can show Mr. Mordey some of my toothed armatures which have been running continuously and giving the greatest satisfaction for 10 years. When he can make a similar show we can talk the matter over anew.—W. B. E.

Mr. J. S. RAWORTH: When I read this paper of Mr. Mordey's, it appeared to me that it treated the dynamo with greater lucidity than I had ever seen or heard it treated before. I thought it was quite impossible for anybody, even a mechanical engineer like myself, to misunderstand the drift and meaning of his remarks; but we have discovered that Mr. Esson at least has mistaken them. Mr. Esson says that Mr. Mordey has been discussing ancient history; but I have mentioned this paper to many gentlemen of the very first position in the electrical industry, and they have agreed that it is the best contribution to our knowledge of the science of the manufacture of dynamos that they have come across these last two years. The only reason I know why Mr. Esson has been able to say it contains ancient history is that Mr. Mordey has taken the things that exist, and, by the application of one touch of the master hand, has combined those existing things to better purpose. Therefore it is that Mr. Mordey's dynamo has electro-magnets and a slotted Mr. Raworth.

Mr. Raworth. armature. I saw only last week two or three very large machines made by a no less important firm than the Gramme Company, who are supposed to know a good deal about the Gramme machine. The armatures were such as I see before me in the diagram Fig. 4, with conductors in slots; they had pole-pieces apparently coming down pretty close on to the armature. When I saw this I felt a considerable amount of interest, because I had only lately been saying, on Mr. Baylor's paper, that in all probability the dynamos in America, constructed on that principle, had a considerable amount of sparking. So I examined the machines closely, and found that, at very great trouble, the makers had recessed the pole-pieces to the depth of an inch, or an inch and a quarter, in order to get sparklessness; thus throwing away good material, and throwing away a very strong field which they might have obtained at very small expense. Now, Sir, either the Gramme people are very much behind the times in their knowledge of the art of dynamo building, or Mr. Mordey has not dished up ancient history.

There are one or two other points I wish to mention with regard to Mr. Sayers's argument. I tried to follow it with the small amount of knowledge on these matters that I possess, and it appeared to me that, if his theory were correct from which he concluded that Mr. Mordey in large machines could not get good commutation under the conditions portrayed in Fig. 4, then the argument could not be confined to large machines, but it would follow that the commutation would also be bad in small machines; it appeared to me equally applicable to both. Later on Mr. Sayers said that if a machine had a considerable number of copper conductors wound in each slot, then the difficulty of commutation was increased, as compared with a machine which had only one bar in each slot. Now I, with one or two others, have been privileged to see these machines of Mr. Mordey's actually in operation. I know all about the design of them, and all about their cost; and I have seen a machine of 15 kilowatts with a considerable number of conductors wound in one slot with a very small field magnet, with only a very small portion of that field magnet covered with copper. I have seen that machine give

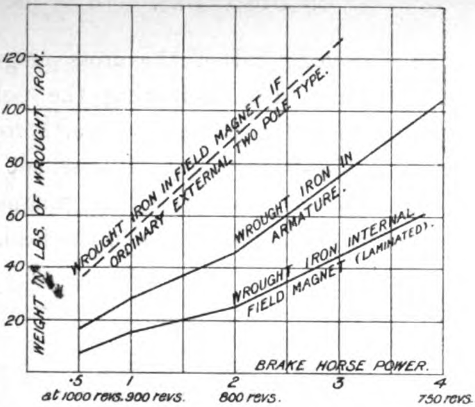
a very much larger output than it could possibly have given with the ordinary winding, and doing it perfectly sparklessly. Now that is a fact which I give for the benefit of the Institution; and therefore, if the argument which Mr. Sayers brought forward is to hold water, he must show that it is not a theoretical objection applicable to all dynamos, but one which applies to large dynamos only, for as a matter of fact it does not apply to small dynamos. So satisfied have the Brush Company been with, first of all, the theoretical considerations which hang around this improved dynamo, and with the performance of the dynamo itself, and with the cost of its production, that they have taken a license under Mr. Mordey's patent, and I hope they will reap very large benefit therefrom. I congratulate Mr. Mordey upon the success of his invention. I think, perhaps, it is a good thing that he put the letter *e* in the word "dynamoes," because it has enabled some of our contributors this evening to explain their views on this question when they had nothing else to explain. Mr. Raworth.

Mr. E. K. Scott: I think it is evident from Figs. 3 and 4 that an armature wound in the particular way Mr. Mordey suggests will have more dead wire than if wound in the ordinary way, and, therefore, that the $C^2 R$ loss would be greater. It would be interesting if the author could give us figures to show how much more it is for any particular case. I do not think it will be serious—probably about 15 per cent. In reading the paper last week, the author suggested that a good method of winding such an armature would be to wind it in the ordinary way as a Gramme ring, and then connect the coils at the ends with short pieces of wire. My experience of the labour costs of armatures is that the labour is practically directly proportional to the number of connections or sweated joints that have to be made. For example, an armature wound with 60 convolutions, and another with the same size core, but wound with, say, 240 convolutions, would not have the same labour cost, because the winder would have in the latter case many more connections and joints to make; therefore, if the particular method of winding hinted at by Mr. Mordey is going to be employed, then I think his labour costs will come out much higher. Mr. Scott.

Mr. Scott. In the last section of the paper reference is made to internal field magnets, and the author was apparently under the impression that nothing has been done in this country in this particular type (suggested by Professors Ayrton and Perry some long time ago). A number of these machines have, however, been built, and the saving of copper and iron in the field is fully recognised by, at any rate, two firms in this country. The curves shown in Fig. A give the relative amounts of iron and copper in armature and field respectively, taken from actual practice (four sizes of the new Mayne rudder motor); and for comparison the dotted lines represent roughly the amount of iron and copper required if the motors had been built in the ordinary way with an external two-pole field. In the larger sizes, with larger currents to commute, the space for field winding begins to get a little bit cut. There is also the question of brush surface on collector rings to be considered, because with an internal revolving field a collector is required in order to get the current into the field winding.

Professor S. P. Thompson long ago suggested the notched pole-piece as one method of attaining the main object which I take it Mr. Mordey has in view, viz., minimising the brush lead and working efficiently with a short air gap. No one seems, however, to have quite grasped the matter in the way it should be done; the Walker Manufacturing Company of America, for instance, laminate the field poles of their large multipole machines, but the thin mild steel plates are cast into the yoke so that they are in the same plane with the armature core laminations. They then cut a long slot across the field pole, from which they claim to get a good result in the direction of minimising the lead on the brushes. One is naturally led to the conclusion that if the pole-plates had been placed so as to be at right angles to the armature core discs, the result would be still more satisfactory, because, instead of only one, there would then be several hundred slots, each helping to prevent armature reaction by giving a more constant line-density in the air gap. To carry the matter a little further, it would not appear to be a very difficult matter to thoroughly laminate the field circuit from pole-face to pole-face, as in Fig. B. This practically amounts to putting the slots in the

Mr. Scott.



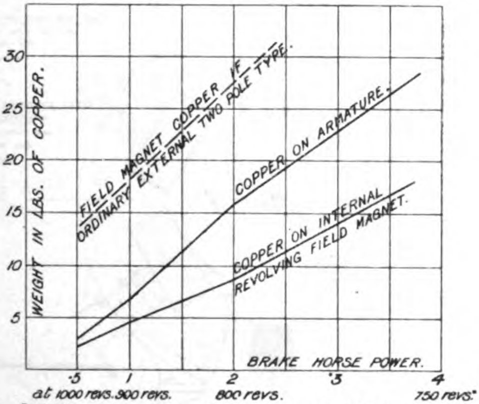


FIG. A. CURVES SHOWING WEIGHTS OF IRON AND COPPER OF SMALL INTERNAL FIELD MOTOR.

NOTE:- Armature is gramme wound and slotted; two pole field is of laminated iron, revolves and has a collector.

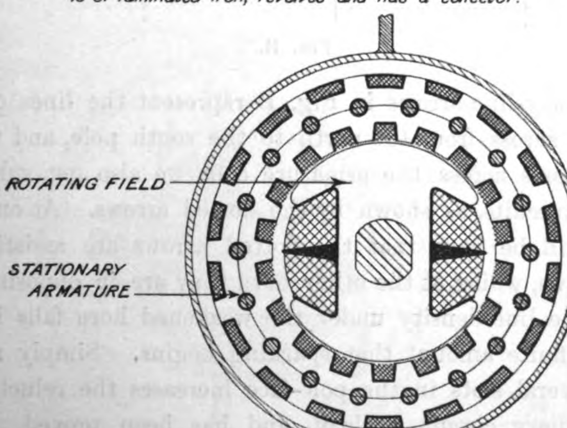


FIG. A.

Mr. Scott. whole way, and thus getting rid of the cross magnetic circuit almost entirely. In any case, laminating the poles of large machines with slotted armatures would be worth doing, because eddy-currents on the pole-faces would be lessened, and in the case of railway and power generators the magnetism would more readily respond to sudden variations in the load.

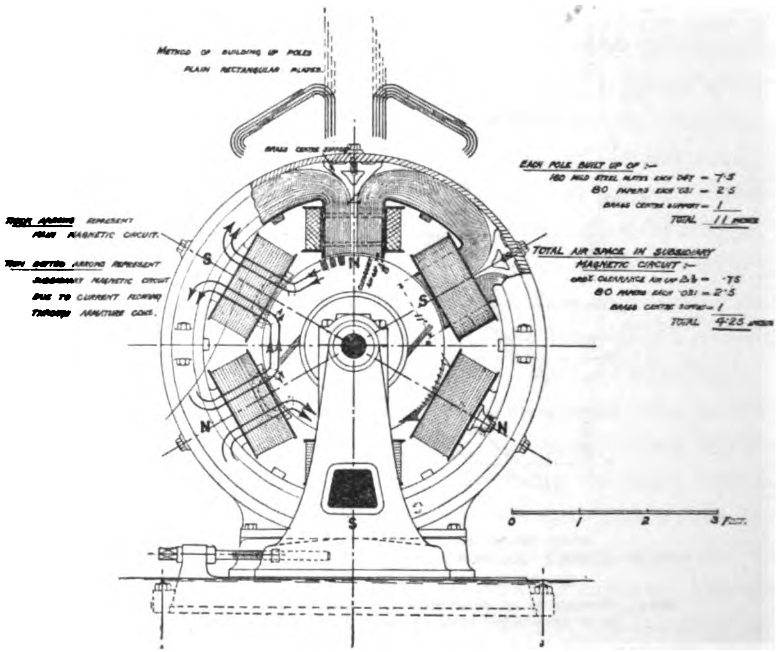


Fig. B.

The heavy-line arrows in Fig. B represent the lines of force streaming across from the north to the south pole, and when a current passes across the armature coils we also get subsidiary magnetic circuits, as shown by the dotted arrows. At one pole-horn it will be seen that the dotted arrows are assisting the heavy arrows, whilst at the other horn they are in opposition; it is when the line-density under the weakened horn falls below a certain definite amount that sparking begins. Simply making one or several slots in the pole-face increases the reluctance of the subsidiary circuit slightly, and has been proved to give beneficial results in actual practice: then why not carry the slots

Mr. Scott.

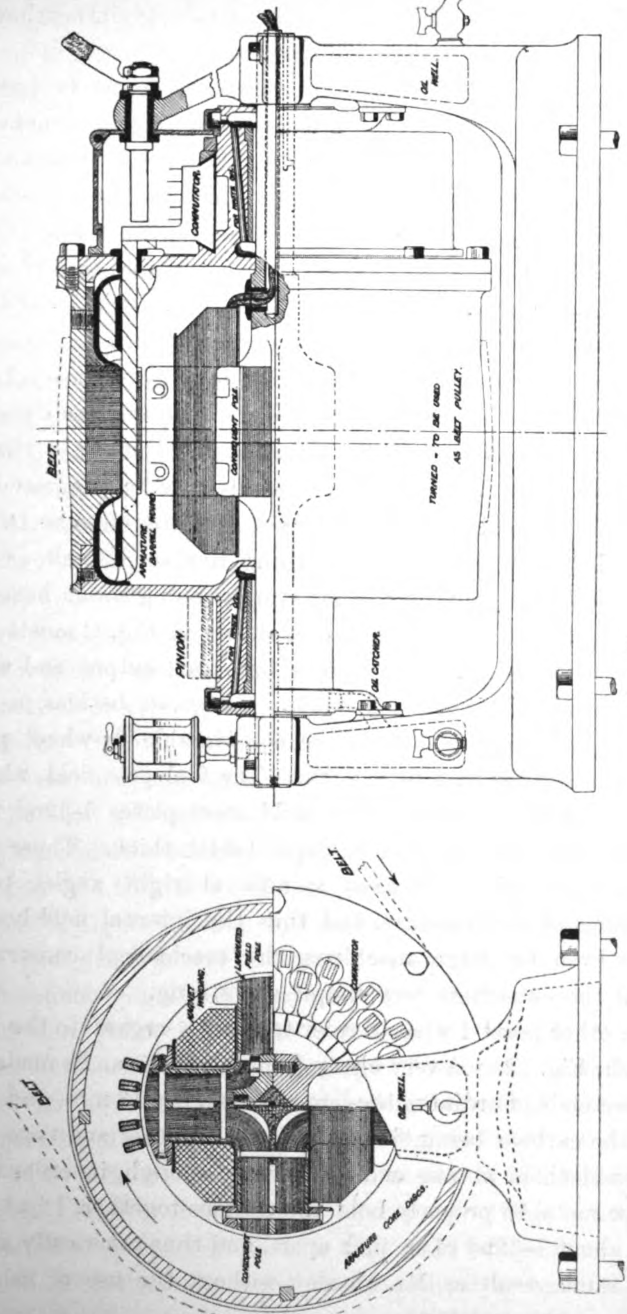


FIG. C.

Mr. Scott. from pole-face to pole-face, or, in other words, go in for thorough lamination, and thus get the full effect?

A certain amount of air gap or reluctance must be put into the magnetic circuit due to the armature coils to make any machine workable, and laminating the field practically means that the distance between the armature core and the pole-face is shortened to a working clearance, and the amount made up by a number of air gaps across the field pole; *e.g.*, a pole-face 10 inches wide will have, say, 80 spaces (160 plates), each 1-32nd inch wide, giving an air gap of $\frac{80}{32} = 2\frac{1}{2}$ inches.

I have gone into this point at some length, because I believe the minimising of the brush lead, and working efficiently and well with slotted armatures, can be attained in other ways than by juggling with the armature windings. It may be suggested that laminating the field would be expensive and troublesome to carry out, but I do not think it is anything like so difficult as some of the methods of altering the armature winding which have been proposed. In any case, the design shown in Fig. C meets these objections very fairly, and I believe for space output and weight output cannot be improved upon. It gives, besides, a large diameter armature, and therefore considerable fly-wheel power, which is desirable in motor work. The four-pole field, which is stationary, is simply built up of mild steel plates 1-32nd thick, insulated from one another by paper 1-64th thick. These plates are firmly bolted to a fixed spindle at right angles to the laminations of the armature, and thus the internal field becomes possible even for large machines, the mechanical construction being at the same time very simple and strong.

The other point I wish to refer to is with regard to the built-up brush (Fig. 12). A very cheap form of brush can be made from the spare ends of ordinary arc-lamp carbon, say 13 mm. and $4\frac{1}{2}$ in. long; the carbons being simply bunched together and type metal cast round them at one end. Curiously enough, in order to get the type metal to properly hold the carbons together, I had them spaced about 1-32nd of an inch apart, and thus practically arrived at the same result as Mr. Mordey without the use of fish glue or other gummy substance.

Mr. H. A. MAVOR: I wish to make an observation with regard to Fig. 4. It seems to me that the object aimed at by Mr. Mordey has already been accomplished by approaching the pole-tips to one another. This method has been largely adopted by German makers, with considerable success. That being so, I do not agree with Mr. Sayers that the machine shown in the diagram would not work. I believe it would, and I believe it would work efficiently and satisfactorily to the same extent as the machines constructed on the principle I have mentioned, viz., those with the pole-tips approximated by decreasing the pole-gap and with the brushes on the neutral line. I think that Mr. Mordey deserves the greatest credit for his new departure, as it undoubtedly is, in adopting another means in the armature itself of arriving at the result which had previously been obtained in the way in which I have described; and I think it may be well worth while to continue investigation in this direction on machines of a larger size, because if a result can be so obtained then he can do what Mr. Sayers does—although I am inclined to think Mr. Sayers's is the better method—and he can also do what he has shown in Fig. 10B, in which he is able to obtain forward magnetising turns on the armature. I doubt very much, however, whether, on the whole, this forward magnetisation has the great advantage that we have been inclined superstitiously to ascribe to it. If a machine has a varying characteristic, and it is driven by an independent engine governed by a throttle governor, the result is that the engine has to run at a higher speed at full load than at light load. I do not know any disadvantage connected with this state of matters which cannot be got rid of by well-known methods. Mr. Mavor.

Another point I wish to mention in connection with Mr. Mordey's remarks as to the advantage of short air space dynamos. I agree with the previous speakers who have held that it is not the sparking alone which limits the dimension of length of air space of slotted-core machines. It is not even the heating of the pole-pieces, nor is it mechanical clearance. The real limiting consideration is that, if there is any want of symmetry in the mechanical construction of the machine, the drag which may take place on the armature may be so great that it is impossible to

Mr. Mavor. make the shaft or the magnets stiff enough to resist it. However accurately a dynamo may be made to begin with, wear takes place in the bearings, and in machines of any considerable size the pull arising from the armature being only 1 mm. nearer one portion of the pole than another can only be realised by those who have witnessed it. The amount of spring in the magnets of single-limbed dynamos is more than one would expect. The magnetic pull on such a magnet will deflect it in quite a surprising way, and, as the pull is inversely as the square of the distance, the evil is cumulative. I wish to mention also that no general arguments as to the advantage of slotted or smooth cores, or long or short air space, can be applied to all dynamos, irrespective of the particular conditions under which the machine is to work. The winding proposed by Mr. Mordey would be quite inapplicable to a ring-wound armature in which there is only one surface conductor to each section, or to a drum with one turn per section. This will be self-evident on inspection of the diagrams, and it is really on such machines that such a device, if applicable, would be most valuable; and here I think Mr. Sayers's has a decided advantage. It is perfectly well known to those of us who have worked with slotted-core machines that the serious difficulties do not begin until the armature is about 20 inches or thereby in diameter, and that the sparking difficulties on the larger machines are much more serious than on smaller ones.

Mr. Crompton. Mr. R. E Crompton: I am afraid that the designers and constructors of dynamos have not been able to learn very much from discussions in this Institution, as so much nowadays depends on the nature of the materials used and on petty details which are never made public in papers such as the one under discussion. We are, however, undoubtedly indebted to Mr. Sayers for bringing before us the method of preventing sparking by adding special coils to the armature winding. In our own firm a gentleman of the name of Brown has been working on the same lines as Mr. Mordey to such an extent that when I first heard Mr. Mordey's paper read I felt that Mr. Mordey and Mr. Brown had gone over identically the same ground. I must confess that I have not been so encouraging to Mr. Brown as I

perhaps ought to have been, as I feel that it is difficult to realise in practice to the full the advantages that Mr. Mordey claims for his improved system of winding. Although it is quite possible that these results may be obtained in small machines, when we come to applying it to larger machines the economies that we can obtain are not so great as Mr. Mordey expects. Further than this, we, the builders of large dynamos, are obliged to be exceedingly careful in introducing radical changes in armature winding, as great money losses are involved in these changes unless they are carried out very gradually and with very great care. Mr. Sayers, in his remarks this evening, has shown this very clearly; this evening the very moderate and careful manner in which he speaks is somewhat in contrast with the much more confident manner that he described his invention when he first brought it before this Institution. I think this will also be the case with Mr. Mordey. I do not say this at all to minimise what Mr. Mordey has done; I, for one, am pleased to welcome his paper as a valuable contribution to our stock of knowledge. Referring to his proposal to use a stationary armature with a revolving field, which involves the use of revolving brushes: This suggestion is an enticing one; it has been before us for many years past; but I think that it will be found that the disadvantages exceed the advantages, principally because of the impossibility of making any kind of adjustment to the brushes when they are in motion. Mr. Crompton.

Mr. C. C. Hawkins: There is one point in connection with two of the diagrams in Mr. Mordey's paper which is not very clear. We are told in the paper that the three turns of Fig. 7 form the *leading* portion of the coil, and we are also told that the effect of this arrangement is shown in Fig. 8. Yet in Fig. 8, according to the direction of the arrow, the leading portion of each coil is the single turn; so that there seems to be a misprint in the direction of the arrow. As regards the general arrangement of winding, everything which I should have said has been much better said by Mr. Sayers. It appears to me that the reversing action is obtained entirely from the leading turns of the coil, and the only difference from the ordinary dynamo Mr. Hawkins.

Mr. Hawkins. is that attached to these is a trailing portion in which an E.M.F. is being developed which is in the old direction; hence, if the machine of which Mr. Mordey speaks is so sparkless, it should be very much more sparkless if the coils were closed up and reduced to the ordinary winding. In fact, there appears to be some feature in the machines rendering them sparkless which is not given on the diagrams of the winding; and if this feature were discovered and investigated, even though the output of the dynamo is only 15 kilowatts, the result would be an important contribution to the Institution. In Mr. Mordey's arrangement there is no automatic strengthening of the reversing force as the load increases, which was the particular charm of Mr. Sayers's device. So far as one can gather from the diagrams, the brushes must be shifted forwards at full load in order to obtain further reversing action. Further, as has been already pointed out, the Gramme ring with three, or even four, turns, as shown in Fig. 7, is quite inadmissible for any large current, such as, say, 400 or 500 amperes.

I should like to emphasise the remarks of Mr. Mavor to the effect that the bending of armature shafts in small air space dynamos must be seen in order to be realised. Having had some personal experience of it, I can say that its amount is very astonishing, as even the steel magnet castings spring.

It is surprising what an affection Mr. Mordey still has for the ring armature; but if this is attacked, I think that, supposing the difficulty of non-sparking has been overcome, the disadvantages of the Gramme method of winding are not so great with toothed armatures. The necessary gun-metal arms do not interfere with the symmetry of the winding, and so far there is no disadvantage. The only remaining disadvantage of the ring is the presence of eddy-currents in the gun-metal arms, which slightly alter the magnetic circumstances of the surrounding coils which are close against these arms.

Perhaps the reason for the success of the 15-kilowatt dynamo is to be found in Fig. 9. There can be no doubt of the arrangement annulling the direct effect of the armature reaction; that it gets rid of the back ampere-turns has been known ever since

chord winding was invented, and is shown very forcibly in the two curves A and B of Fig. 9. The absence of direct armature reaction is of some assistance in obtaining sparklessness, and this suggestion is borne out by the second machine alluded to in the paper. Here the armature reaction was very powerful, and originally there was no field in which the brushes could be worked sparklessly. After the direct armature reaction had been annulled the field was stronger, and the effect of the cross turns on this field was less than it was before. I can only confess my inability to see how the windings shown in the paper can in any other way assist in stopping the sparking of toothed armatures. Mr. Hawkins.

Professor JAMIESON: I am very sorry that I had no opportunity of reading Mr. Mordey's paper before coming to the meeting this evening. I have, however, been very much pleased with the lively and interesting discussion, and the various views that have been expressed to-night. Prof. Jamieson.

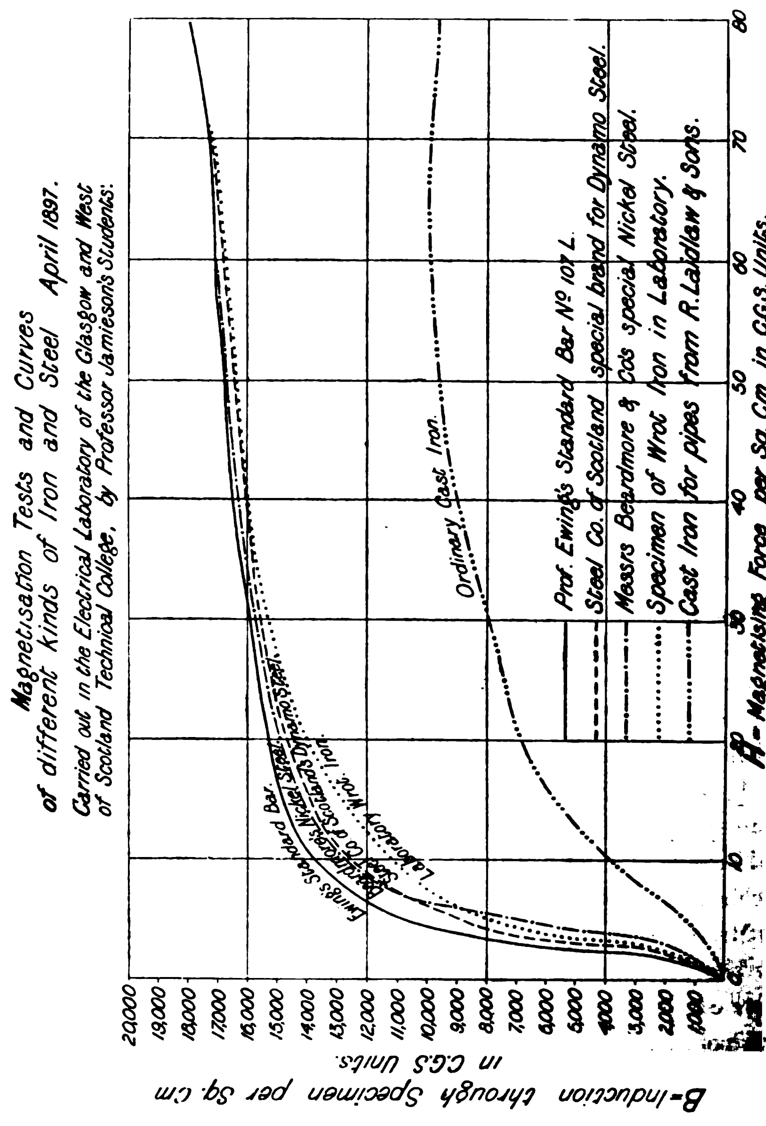
In the old days of 1873–74, when Professor Ayrton and I used to attend the meetings of "The Society of Telegraph Engineers," we could never muster such a vigorous discussion as you have been able to achieve this evening. It is now more than 10 years since I have been present at a meeting of this Society, and I have much pleasure in renewing my acquaintance with the Institution of Electrical Engineers. It is, however, too late in the evening for me to enter upon any details, and I shall avail myself of the privilege of making my remarks in writing.

[*Communicated.*]—I have had great pleasure in reading Mr. Mordey's paper, and thoroughly agree with most of his arguments and believe that he has arrived at a more practical solution of the problem of small gaps and light field magnets than that obtained by Mr. Sayers.

The discussion on this paper was so full and complete that there can be no good in reiterating the various opinions expressed. One point, however, was not touched upon, viz., the advantages to be derived from the use of cast steel of high magnetic qualities. It so happened that a very short time ago Mr. Beardmore sent me a specimen of his cast nickel steel which he uses for Harvey-ised armour plates, and I was astonished to find that its

Prof. Jamieson.

permeability very closely approached that of Professor Ewing's standard bar. I have therefore much pleasure in enclosing the set of curves showing its position with regard to other well-known



Magnetisation Tests and Curves of Nickel Steel, compared with other kinds of Iron and Steel.

brands. It will readily be understood that if this material can be produced with a definite percentage of nickel, and cast in any

desired form, dynamo makers have at their disposal a cheaper means of producing field magnets than has hitherto been the case, because no shaping or planing is necessary. Moreover, for dynamos to be used on board men-of-war, where they are liable to the effects of shot and shell, the outer surface can be so formed and Harveyised as to render it impervious to these destructive agencies. Prof. Jamieson.

The other day I had an opportunity of testing a dynamo and a set of motors made by a French firm for a large factory. The design was of the old Schückert type illustrated in the early editions of Professor S. P. Thompson's "Dynamo-electric "Machinery." I found that the temperature of the armature rose to 190° Fahr., thus showing that some of our friends across the Channel are still lagging behind, and would do well to study Mr. Mordey's paper and the discussion thereon.

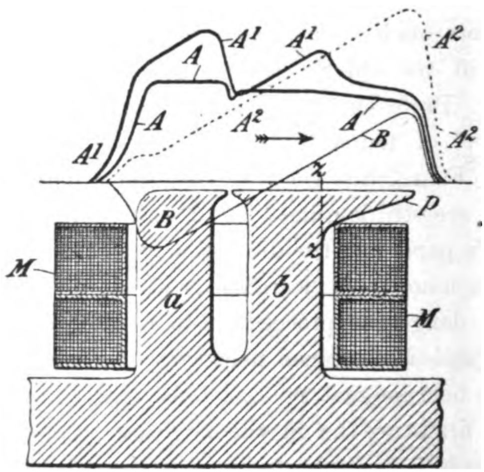
A certain amount of clearance is absolutely necessary, and it would be a dangerous proceeding to minimise the same to the merely mechanical clearance necessary to allow a body to rotate between the field-magnet poles. This arises from the fact that, should the armature be in the slightest degree eccentric, an abnormal stress is brought to bear on the shaft and one of the poles, tending to bend the former and cause contacts.

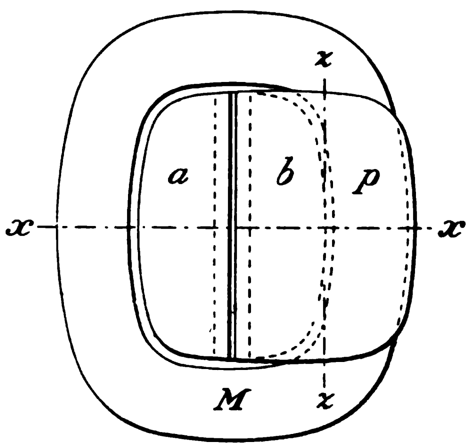
In section 35 of his paper, Mr. Mordey merely mentions as an experimental fact that "it is quite possible to shift the collecting "line . . . . previously was the best one." The explanation of this phenomenon lies in the mutual relation between the armature reaction and the lead. If we start with a large lead, and then shift the brushes back so as to collect the current further back, we shift the element under commutation to a weaker or a reversed part of the field. We also, however, reduce the number of armature turns acting in opposition to the field magnets, and so increase the whole field.

This increase may compensate for the first effect, so that collection is as good at the second position as at the first. The dynamo will, however, give different voltages at the same speed for different positions of the brushes.

Mr. R. W. WEEKES: I would ask Mr. Mordey to give us, in Mr. Weekes.

Mr. Weekes. reply, the details of the machine he has tested, so that it can be compared with other types. I think that with toothed-core armatures the size of dynamo at which chord winding will cease to be advantageous will soon be reached. A new way of reducing the effect of armature reaction was recently described to me by Mr. Robert Lundell, of New York. It is shown in the accompanying diagrams, and is particularly applicable to multipolar dynamos. As





will be seen, the cores of the field-magnet limbs are divided into two halves, "*a*" and "*b*," and the pole-piece on "*b*" is made roughly twice the area of that on "*a*." In this way the limb

"*b*" is saturated at no load, and hence the effect of the total cross turns is checked, and the distortion of the air-gap induction is confined to the individual poles. When the dynamo is loaded the armature drop calls for increased armature induction to keep up the potential difference. Owing to the limb "*b*" being saturated, the effect of the series turns is only felt in the limb "*a*," and the induction under the "*a*" pole-piece is increased in consequence. This is just what is wanted to counteract the reaction of the cross turns, and by suitable proportions a constant lead for the brushes can be obtained. The curves over the poles denote the following quantities:—A A is the curve of air-gap induction at no load; B B shows what the cross induction would be without the Lundell arrangement; and A^2 A^2 is the resultant of these first two curves. This shows an unstable condition as regards sparking. The final curve, A^1 A^1, is the actual distribution of induction at full load with the new type of field. This arrangement allows either of smaller air gap being used, or, conversely, of larger outputs with a given air gap, when the output is determined by the sparking limit. Mr. Weekes.

Mr. C. H. GADSBY: I agree in principle with Mr. Mordey's claims for the advantages of this winding, but it involves cross-connections at the ends which spoil the simplicity of the ordinary Gramme winding. I should like to suggest a method of securing Mr. Gadsby.

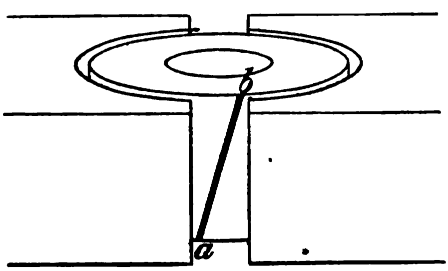


FIG. 1.

the same results whilst avoiding the cross-connections. Instead of winding the coils in two parts, it seems to me that a better method would be to arrange the conductors in a slanting direction on the core, as indicated by the line *a b*. This would not entail

Mr. Gadsby. any additional expense in manufacture with toothed cores, as, of course, the stampings would be made in the usual manner, but in building up would be arranged on the shaft to give the slanting groove. This leads me to another suggestion, which would, I think, have the same effect. Instead of putting a slanting conductor on the armatures, we might keep it parallel to the axis of revolution and adopt slanting pole-pieces, thus—

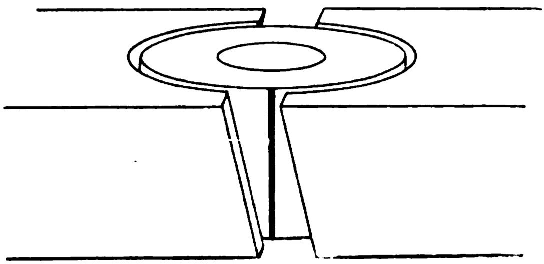


FIG 2.

Mr. Evershed. Mr. EVERSHED: The method of slanting the pole-pieces in order to obviate the necessity for a diagonal winding along the armature was, I believe, first suggested by Mr. H. B. Atkinson, during the discussion on the papers read by Mr. Swinburne and Mr. Esson in 1890.

Lord Kelvin. Lord KELVIN [*communicated*]: I am much interested with Mr. Mordey's experimental proof (section 47 of his paper) of the theorem—so important in dynamo construction—that partial burying in iron of the armature conductor in a dynamo or alternator, greatly diminishes the "drag." From section 43 it seems that this theorem is not universally known among capable engineers. It is possible, therefore, that the theoretical proof, indicated by lettering and colours, added to a copy of Mr. Mordey's Fig. 13, which I enclose, may be new to some.

Red and blue indicate, according to Sir George Airy's convention, "true northern" and "true southern" ideal polarities, or ideal magnetic matters. The blue on the inductor poles indicates their polarities. The red and blue marks on the iron of the armature denote the polarities due to current towards the eye and from the eye, according to the lettering written on the

diagram. The dotted arrow indicates the direction of motion of the inductor poles to induce this current; and the plain arrow Lord Kelvin.

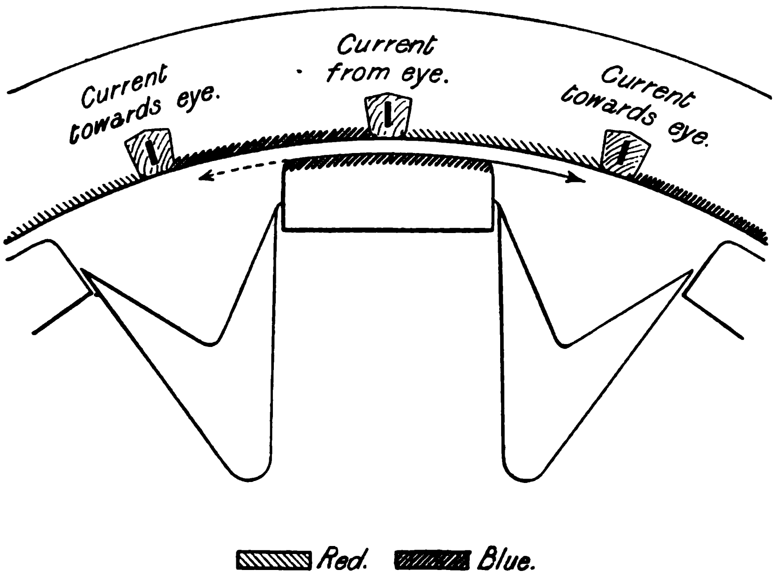


FIG. 13.

indicates the direction of the resultant force between these poles and the polarities of the armature iron.

With reference to the details of the experiment, I would remark that to get a correct measurement of the force the weight should be applied uniformly over the whole length of the flexible conductor. This, in virtue of the flexibility of the conductor, would give a considerably greater deflection than that produced by an equal force applied at the middle. An easy modification of the experiment to get over this trouble is to make the straight bar stiff, and to support its two ends symmetrically by a springy mounting, so that the displacement by which the force is measured may be a motion of the bar parallel to itself.

Mr. GISBERT KAPP [*communicated*]: I wish to congratulate the author for having succeeded in imparting to so well-known a subject as the ordinary continuous-current dynamo, not only a new and highly scientific interest, but also great practical utility. It Mr. Kapp.

Mr. Kapp. is certainly remarkable that until Mr. Mordey has pointed it out the real merit of chord winding has been overlooked, and I may say not understood. for so many years. There is a fundamental difference between the Sayers and Mordey winding; not only is the action of the auxiliary coil in Sayers's arrangement intermittent, but the E.M.F. to produce reversal is injected into the middle of the section, and not into one end of it, as with Mr. Mordey's arrangement. Or, to put it in another way, Sayers's has auxiliary coils which form shunts to the main coils; Mordey's reversing coils are part of the main coils, and in series with them.

The effect of chord winding is to decrease the width of the neutral gap. With the ordinary winding a large neutral gap is necessary to reduce the cross turns and avoid sparking, but it has the disadvantage of producing back turns. To eliminate back turns we must widen the pole-pieces until they meet, and thus reduce the neutral gap to zero. A machine of this kind would, however, spark badly. Now by adopting Mordey's system we secure the advantage of negligible back turns without having to widen the poles; that is to say, we can make a machine having no back turns and get only the usual amount of cross turns. That the end connections become shorter, and, therefore, more easy to stow away, is an incidental advantage. This applies, of course, only to two-pole drums, or to multipolar drums with lap winding. In multipolar drums with wove winding the end connections become shorter on one side and longer on the other.

I think the author has somewhat overrated the reduction in size which becomes possible by his method of winding. The absence of back turns, and the reduction in exciting power resulting therefrom, will certainly diminish leakage, and thus the magnets may be made lighter. The shorter end connections are also important, and the possibility of reducing the polar arc permits of loading the armature to a greater extent. It must, however, be remembered that to this reduction there is, especially with toothed armatures, a practical limit, which is soon reached. The average reduction in the tooth is with ordinary machines from 17,000 to 20,000—in some American railway generators even 22,000. Any reduction in the polar arc causes a proportional

increase of this induction; and since through the cross turns in the armature the induction in the teeth is reduced at the entrance edge, and increased at the exit edge of the pole, we get very soon values of the induction in those parts which are physically impossible. The iron of the teeth under the exit edge cannot carry all the lines. Some must pass through the air between the teeth; and thus the magnetic resistance increases with the load, compelling us to either put on more excitation or submit to a drop in volts. The greater heating of the teeth, and of the conductors, which are no longer completely shielded by the teeth, may also become troublesome. That in the hands of a skilful designer the Mordey winding will tend to increase the output per unit of weight and labour, I freely admit; but I contend that this increase is not so great as the author states. Mr. Kapp.

As regards the much-discussed question whether there is drag on the conductors in a slotted armature, I must confess that neither Mr. Swinburne's explanation by analogy nor the author's experiment have convinced me. The explanation I consider at fault, because it is based upon the conception that each line of force is a physical entity like one bar of a railing. There is no such thing as a single line of force which traverses the iron of the tooth closely and jumps quickly across the space between two teeth. The experiment would be convincing if Mr. Mordey had measured the phase difference between impressed E.M.F. and current and had found it small. Since he does not tell us how great the inductionless resistance was, we can form no opinion of the lag. It is evident that if the lag is considerable a bar would, even with a smooth core, show very little deflection, because the mechanical impulses would be alternating and nearly equal in both directions. Perhaps Mr. Mordey will, in his reply, state what was the lag between impressed E.M.F. and current. If this was small, then we might consider the experiment to have definitively shown that buried conductors experience no drag.

Mr. JAMES SWINBURNE [*communicated*]: Without wasting time on congratulations, however well deserved, I will go to the point at once. In the early days of dynamos, before any of us Mr. Swinburne.

Mr. Swinburne. knew very much, I designed a winding for Gramme and drum machines, which, as far as the winding itself goes, resembles Mr. Mordey's very closely. The Gramme had its coils in pairs, like Mr. Mordey's, but, instead of being separated by approximately the distance of the uncovered part of the armature, they were separated by about the arc of the pole-piece. This gives the same result as Mr. Mordey's arrangement. The drum form of this winding is better known as "chord winding." As a matter of fact, however, my winding was designed solely to overcome the "back induction" of the armature winding, and this it did by arranging conductors in the uncovered part of the armature carrying opposite currents. But this was all. I never contemplated applying this winding to embedded armatures, and was not dealing specially with the reduction of sparking, except as far as it was involved in the other questions. I certainly never thought of this winding as a means of reducing sparking. I thought of it more as a means of reducing back induction without, I hoped, increasing sparking. My form of Gramme winding was never used to my knowledge; the chord winding was tried on drum machines, where it had the additional advantage of lessening the difficulties of the end connections. It did not prove successful in my hands for reducing sparking in early days—1887—as it gave rise to such bad sparking at light loads that I had to abandon it. This was in smooth-cored machines, of course, and single-limbed magnets with massive pole-pieces were used. I never used the chord winding again till I had a chance of putting it on multipolar machines with symmetrical fields. Here it was used to simplify the end connections, and to lessen back induction as far as I thought safe without running risk of sparking at light loads.

It is difficult to look back ten years and see exactly what one had in his mind then; but I am quite sure I did not introduce chord winding to stop sparking, but to reduce back induction and (incidentally) to simplify end connections. I knew the angle of movement of the brushes with increasing load was small, but I always foresaw difficulties as regards sparking; and I never saw any special applicability to embedded armatures. This is especially curious, because in 1886 and 1887 I was studying

dynamos a great deal; and I not only designed the chord winding, but also investigated the questions of cross and back induction, was first to go into the matter quantitatively, developing Hopkinson's theory in this direction, and the theory of armature reaction more fully discussed in the paper I had the honour to read before this Institution in 1890 was really published by me in 1887, and is now the generally accepted theory. Also, at this time, though hole-wound armatures had been described by Wenström, I was the first to point out that they gave small air space, and therefore small field magnets with little excitation. It is difficult, of course, to realise that in 1886 people did not appreciate a point like that at all. Yet, having the chord winding, the theory of armature reaction, and the embedded armature with small air gap in my mind, I quite missed all idea of using the chord winding in a small air space machine with embedded armature conductors. I have descended into purely personal history, because it is clear that I am mistakenly supposed to have anticipated Mr. Mordey in some respects. I am sorry to say I have not done so; and can only add my hearty congratulations to a man who has hit where I missed—whether by much or little does not matter. Mr. Swinburne

As regards the utility and practical value of Mr. Mordey's invention, I feel a little sceptical as to large sizes. If the air gap is small, the cross induction is great, and reversal under one horn is the result in a machine of any size. This means a large air gap, which seems to me still unavoidable unless you have some sort of reversing pole-pieces, or, better still, some such device as Mr. Sayers's. As regards the advantage of small air gaps otherwise, I am, of course, entirely with Mr. Mordey. They have the advantages he enumerates. The experiment to show there is no appreciable mechanical drag on the conductors seems hardly necessary at this date; but many seem still to think there must be force on the conductor. They only have to remember that, as the force is proportional to the product of the field and current, it is zero when the field is zero, as in an armature hole when it is "shaded" by the iron. It may be asked where the pull comes; the answer is, on the iron. When there is an armature current which cross-magnetises the poles, the fringe is strengthened at

Mr. Swinburne. one corner or horn, and weakened at the other. This induction fringe enters the iron obliquely, and when one is strengthened and the other weakened the torque is found to be devoted to overcoming the difference of pull at the two fringes. The fringe seems a small thing to give all this pull. The pull at the fringe also varies as the square of induction; but, as we are concerned with the difference of pulls on the fringes, the torque follows the straight-line law as regards the armature current, and the field comes in as a factor. The whole pull thus comes on one or two of the teeth that are leaving the field. In a smooth-cored armature, on the other hand, all the pull is on the wire, as the "lines of induction" enter the iron radially all over, and never obliquely.

In addition to there being no mechanical force on the embedded conductors, there are no Foucault currents due to one part of the conductor passing through a stronger field than the other, so that solid bars or rods can be used even in alternators, as far as these Foucault currents are concerned. The skin effect, or a modification of it akin to the Thomson effect, explained by Lord Kelvin at the B.A. meeting of 1890, will, however, still take place, and will depend on the shape of the holes or notches.

Returning to the question of Mr. Mordey's method of dealing with sparking, Mr. Mordey tells us it improves small or medium-sized hole-wound machines. This I can quite believe, and congratulate the inventor on the result. But in large machines I do not see that it will help very much, unless they are multipolar.

Resistance in the commutator connections, or between various layers of the brushes, is an old expedient. I tried it 12 years ago, and believe it was several years old then. It can only be necessary when the brushes have to be broad enough to collect very large currents, the breadth allowing the current in the short-circuited section to increase largely at first, instead of diminishing. To overcome this I have proposed to use several brushes in different angular positions, the conductors from them being kept separate for some distance; perhaps going in parallel to the electro-deposition vats, for example.

There is a great deal more one would like to discuss in a refreshing paper like Mr. Mordey's, but a "communication" is rather a dry way of discussing at any time. Mr. Swinburn

Mr. ARCHIBALD H. FINLAY [*communicated*]: It would appear at first glance that the position of sparkless collection, in the devices brought forward by Mr. Mordey, would alter with the load. Towards the end of 1893 I devised a plan to get over this difficulty, which Mr. Mordey has proved to be not very real, and that was to place the reversing turn or turns of the section behind the main portion, and, of course, wind it in the opposite direction, so that the reversing turn would come under the strong horn of the pole and its E.M.F. would increase with the load. The E.M.F. of this reversing turn opposes the E.M.F. of the rest of the section for most of the revolution, and so the proportion of waste copper on the armature would be very great *unless* there were a large number of turns in the main portion of the section, *or unless* the reversing turn were wound on a small portion of the armature, *or*, *better* (from a theoretical point of view), the reversing turns could be wound on a small auxiliary armature, where they would not oppose the E.M.F. of the main part of the section except at the moment required. The magnets for this auxiliary armature could be pieces projecting from the strong horns of the main magnets, and they could have a series winding on them, and could be arranged so that the auxiliary armature would assist the main armature for nearly all the Mr. Finlay.

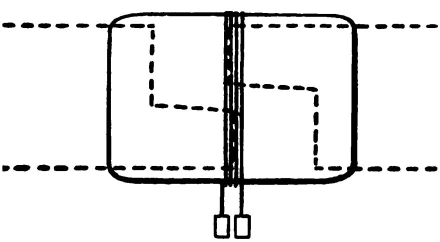


FIG. 1.

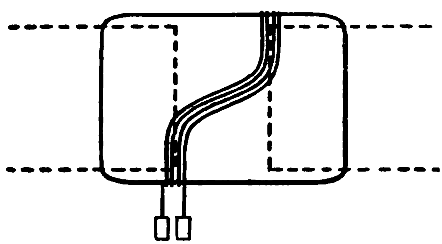


FIG. 2.

revolution. It is, of course, a question of cost whether such a device would be useful in practice. Returning to the device brought forward by Mr. Mordey: Similar results could be

Mr. Finlay. obtained by arranging one part of the pole to lead in front of the remainder, or by having the armature wound diagonally (Figs. 1 and 2). (The magnets are shown dotted.)

Mr. Brown. Mr. S. G. BROWN [*communicated*]: About nine months ago I began independently experimenting at Messrs. Crompton & Co.'s works with small slotted drum machines, winding the armatures with this new winding, and, as it has been suggested to me that the results would be of interest if published, I take this opportunity of sending them as a contribution to the discussion of Mr. Mordey's paper. One of the machines was an ordinary diameter-wound, 4·5-kilowatt slotted drum, 44 sections of three complete turns of six conductors to each section. The field magnets were shunt-wound, and the clearance space was 0·1 inch a side. On a run down as a dynamo at 1,140 revolutions it gave the curve A in Fig. 1. There was a 20-volt drop at

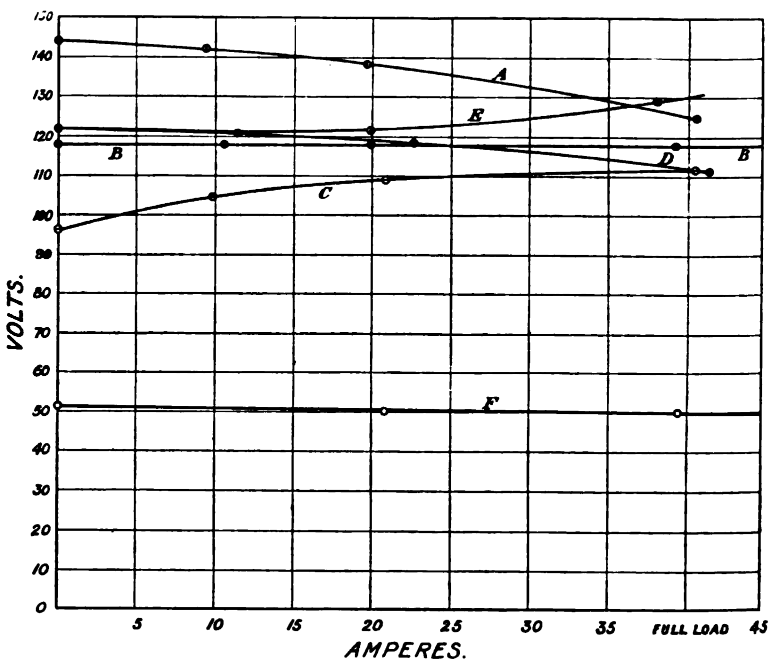


FIG. 1.

full load, and, as the machine sparked soon after, and lost its field if the brushes were moved forward, the maximum load was

practically at full load. The armature was then rewound with wire of the same cross section, but in this case there were only 22 sections of six complete turns of 12 conductors to each section, two conductors being placed forward, a distance little short of the distance between the polar horns. Running as a dynamo at 1,150 revolutions with brushes covering three-fourths of the width of a commutator sector, it gave the curve D. This shows a 10-volt drop at full load, and a moderate overload could be got. Increasing the brush to just under seven-eighths of the width of a sector, and running at 1,200 revolutions, the machine compounded exactly, and, moreover, would stand without any drop of volts whatever, and without sparking, an 80 per cent. overload. The curve of this run is lettered B in the figure. The brushes were broadened to the width of one whole sector, and running at 1,200 revolutions it gave curve C, over-compounding by 14 volts at full load. Separately exciting the field, we obtained curve E—an over-compound of 8 volts. With the width of brush and the same speed as in the two last cases, but weakening the field so that the machine gave out less than 50 per cent. its normal voltage, it nearly compounded, the curve F showing the result, and a 25 per cent. overload could be obtained. Running as a motor, in a reverse direction to the dynamo, having brushes just under seven-eighths the width of a commutator sector, it ran with constant speed, with constant volts at all loads. A great number of other tests were made, some on other machines, all pointing to the same general results. Mr. Brown.

In all the tests mentioned gauze brushes were used. I was at first put to some trouble to suitably explain the action of the new winding, but I think I have found a partial solution; I give it here. In Fig. 2 consider the plane *n*, *o* P. If the whole section, *m*, *n*, *o* P, was an ordinary diameter winding—that is, if the forward conductor, *m*, was closed up to *n*, and the section was undergoing commutation—the plane *n*, *o* P would be the plane of maximum magnetic potential, or maximum H of the armature, and points *n* and *o* P would be points of maximum magnetic potential or maximum H; nd therefore, if the section is to be commutated sparklessly with

Mr. Brown. ordinary brushes, there must be a small positive field at *n* or *o* P. This can be obtained if the ampere-turns on the armature never exceed the ampere-turns on the field magnets for the air gap.

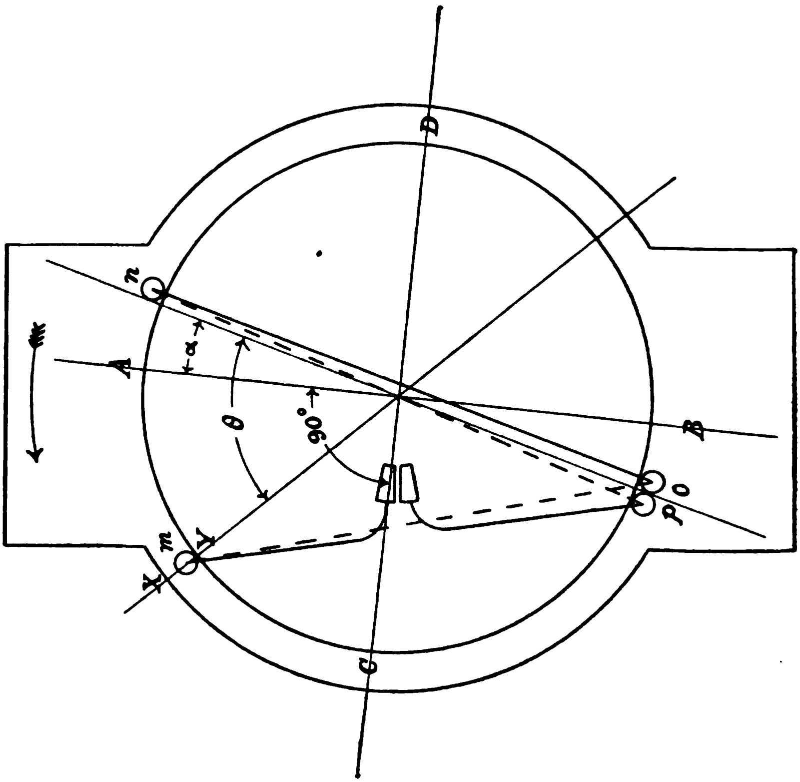


Fig. 2.

In the new winding, if *m* is the forward or commutating conductor, and the angle that it makes with the plane of the main part of the section is θ, the plane of maximum H will move forward through an angle a such that

$$a = \theta \frac{\text{forward conductors}}{\text{whole conductors}}.$$

Draw a line, C D, through the centre of the armature at right angles to A B: then C D will be the plane of minimum magnetic potential, or zero H, and points C, D, are the points of mean field, or zero H.

Therefore, if we desire to have a small positive field across X Y for commutating the section, the ampere-turns on armature Mr. Brown.

$$< \left\{ \frac{90 \text{ (ampere-turns on field for air gap)}}{90 - \theta\left(1 - \dfrac{\text{forward conductors}}{\text{whole conductors}}\right)} \right\};$$

θ being the number of degrees in the angle the forward conductor makes with the plane of the main part of section, or the plane that it would occupy if the section was a diameter winding.

At present it is not quite clear how the broadening of the brush affects the performance of the machine, except that it would shift the plane of maximum H further back towards *n*, *o* P, and increasing the time of short-circuit, allowing a weaker field to accomplish the commutation.

Mr. LLEWELYN B. ATKINSON [*communicated*]: Mr. Mordey's paper deals really with a variety of points illustrated by interesting experiments, but the writer proposes to confine himself to the question of small air space dynamos, and to consider how far Mr. Mordey's paper advances our knowledge of the subject. Mr. Atkinson

To do this it will be convenient to consider the difficulties arising with small air space dynamos. These are as follows:— If the brushes are, or can be, kept on the neutral line, the cross induction producing a weakening of the field under the leading pole-horn, and a strengthening under the trailing horn, does not produce any decrease or alteration of volts, but it produces a localisation of the total induction, occasioning loss due to excessive hysteresis. This may be modified by cutting the poles right through, or may be balanced by series-wound coils in the pole-pieces (Thompson and Ryan's method). Mr. Mordey's winding does not touch this difficulty. The next difficulty is that a forward lead is not allowable, owing to the demagnetising effect of the armature, as shown in the paper; and to enable the brushes to be kept on the neutral line, or to have a backward lead, some method of reversing the current in the coil under the brush must be resorted to. Various methods have been adopted. Swinburne and Kapp have described methods with subsidiary armatures carrying reversing coils, acted on by independent series-wound magnets; the present writer has shown (Patent No. 4352, 1895)

Mr. Atkinson how such series-wound magnets may be dispensed with, and the armature ampere-turns utilised to produce the necessary field. Sayers has shown how, by using reversing coils working under the the trailing pole-horn, a reversing E.M.F. is always available; Swinburne, Housman, and Ravenshaw have described methods of applying series-wound magnets to the armature coil itself; and now Mr. Mordey shows how, by placing part only of the armature winding so far forward, a reversing E.M.F. is available. The method applied to the drum armature is Swinburne's chord winding, and it has therefore been objected that Mr. Mordey gives us nothing new; but he has replied—and the writer thinks rightly—that the advantage of the chord winding has not been recognised, or it would have been used. The cause of this was probably the fact that Swinburne himself stated that it was difficult to use, as it caused sparking at small loads. Now the question is, Is Mr. Mordey's solution a real solution for small air space dynamos? The writer thinks the answer is No. The E.M.F. required to reverse the current is proportional to the current; the field under the leading pole-tip is a constant minus a quantity proportional to the current, and this with small air space dynamos would become zero. Hence there must be shifting of the brushes, and a load reached at which even this would fail to give the required reversing E.M.F. What Mr. Mordey has proved is this—that with a moderate-sized dynamo, apparently *not* a small air space machine, the load may be carried further than with an ordinary winding, because the leading pole-tip is only weakened by the cross induction, and not by the cross induction plus the back turns; and thus far he has usefully contributed to our knowledge. So far the writer has dealt with the question of sparkless working where the reversal is a true electro-dynamic reversal—that is, where the electro-kinetic energy due to the current in the coil to be reversed is first converted into energy of motion of the armature, and then into electro-kinetic energy in the coil, in the opposite direction. But Mr. Mordey describes a form of brush having layers, with resistance from layer to layer, which obviate sparking. This form of brush was patented by the writer in 1886 (Patent No. 8003, 1886), when sparking

difficulties were much more rife. Brushes of this sort applied Mr. Atkinson to some old Hochhausen machines, which had few coils in the armature and weak fields, enabled them to run sparklessly. The writer has, moreover, for years used and specified brushes made of fine copper wire *oxidised* so that the only contact is at the top of the brush, and the action of the very finely laminated brushes is similar. Carbon brushes act in an almost identical manner. The *rationale* of this method of current-reversal is as follows :—At each brush, between the circuit of the coils on half the collector carrying a steady current and the external circuit, is the resistance of the brush from point to heel. This resistance causes a fall of pressure between point and heel proportional to the current. This difference of pressure is between the two commutator sections at the ends of the coil to be reversed, and forms, therefore, the reversing E.M.F. required. The current in the coil dies down, expending its energy in the brush, and the energy is supplied to restart it by all the other coils in the armature—that is, from the engine. The heating of such brushes confirms the view that they are wasteful, and by making the resistance from front to back of the brush of incandescent lamps, as the writer has done, they may be rendered incandescent. All these methods, therefore, are delusive, and because they prevent sparking lead to the belief that the machine runs well, when it is running inefficiently. So that here again a warning must be given that Mr. Mordey's suggestion does not make for efficiency, and in large machines the heating of the brushes becomes prohibitive.

In the course of the discussion on Mr. Mordey's paper, several speakers challenged the desirability of aiming at small air spaces, owing to the strains thrown on shafts and bearings in such machines. This happens more particularly in two-pole machines; in multiple machines to a much smaller extent. The condition necessary for a side strain tending to move the armature, is that the sum of the electro-kinetic energy of all the field-magnet circuits should be augmented. The electro-kinetic energy of each field-coil is measured by the total induction through the coil multiplied by the ampere-turns on the coil. These latter remain constant. Hence the magnetic circuits should be arranged so

Mr. Atkinson that by any movement, any increase in induction through one coil, is balanced by a decrease in some other coil or coils. This is the case with Manchester-type fields or with iron-clad fields, if the fields are cut right through. In a case tried by the writer where with about 1-16th inch clearance the shaft of a small 5-inch diameter armature, 1 inch diameter, always bent so that it touched the poles, cutting through reduced the side field practically to *nil*, the effect being inappreciable.*

So far as small air space dynamos are concerned, Mr. Mordey's paper will not, in the view of the writer, advance our knowledge far; but for moderate air space machines of medium dimensions there is every possibility that the attention called by Mr. Mordey to chord winding, or its equivalent, will have useful results.

Mr. Mordey. Mr. W. M. Mordey, in reply, said: I pass over the complimentary remarks made by several speakers. I am very much obliged for them.

Professor Thompson's reference to some of our joint studies of this subject is incomplete in one particular. Before the Ryan proposals were announced, we had gone carefully into a construction which has, I think, never been tried. The stationary conductors which surrounded the armature were to be of iron fixed to, but insulated from, the pole-faces of the magnet with the idea of avoiding the increased reluctance, these iron conductors carrying both the current and the magnetism. The working drawings made it appear that the advantages of this construction would be too dearly purchased, and we therefore did not complete our patent.

Mr. W. B. Sayers's interesting contribution to this discussion will well repay study, especially the particulars he gives as to his own practice. His unwillingness to accept my facts, so evident in the first portion of his remarks, need not now be referred to in detail, because in his second and later contribution he practically does accept them. I may, however, be allowed to express my admiration for one who refuses to believe in facts until he has invented a theory of his own to explain them.

* See *The Electrician*, No. 995 (June 11, 1897), p. 215.

But Mr. Sayers is wanting in faith in the efficacy of my proposals, so far as large machines are concerned. As on this point I have no facts to put forward, I will not attempt to discuss it; I will only say that, so far as I can see (and after consideration of all that has been advanced in this discussion), what is sauce for the gosling is sauce for the goose. I may add that whether the proposals are useful or not on very large two-pole machines is not a matter of great importance, because such machines are likely to become obsolete on the score of cost. For all large work, especially slow-speed work, the cheaper constructions are undoubtedly multipolar; and as a large multipolar dynamo, properly considered, is merely a number of small dynamoes disposed about one shaft, I foresee no difficulty with them which cannot be met by methods proved to be efficacious by trial on small machines. Mr. Mordey.

An objection put forward by Mr. Sayers and others is that the drum dynamo referred to in the paper as greatly improved by the alteration of its winding must have been, at the start, a very bad machine, and, therefore, is not a fair example. But the whole object of my paper was to show how to make bad machines into good ones. The dynamo referred to was specially used to show how an inexpensive machine that would have been rejected on account of sparking (although otherwise a good and well-made machine) was not only made to run sparklessly, but was at the same time still further cheapened and was also made more efficient. It does not follow that no improvement is to be obtained in dynamoes which work well already; in such cases the improvement must be applied to get equally good results at less cost, or, in other words, to increase the satisfactory working range of machines of a given cost.

I may appropriately explain here how the windings described in this paper originated, so far as I am concerned. When Mr. Sayers read the papers describing his very ingenious devices, I was behind no one in my admiration of his work, but a study of the subject always left me with the feeling that only a very small portion of his auxiliary winding was really essential. For example, if on an armature he had, let us say, 100 conductors, he

Mr. Mordey. placed, let us say, 100 auxiliary conductors, or "commutator "coils." Now of these 100 extra conductors he only used, at any one time, one or two at each brush—perhaps four altogether—but in order to have four always available at the right place he had to provide 100, and allow 96 of them to be idle. It seemed to me some simpler way might be found of obtaining at least a partial solution of the problem—some way of arranging the ordinary winding so as to get the effect of the four useful turns without the necessity for adding the 96 useless turns. If the winding shown in Fig. 5 and other figures be studied, it will be found that for all practical purposes it is this desired simpler way. The "element" shown in thick lines in Fig. 5 is substantially identical with the Sayers "element," only, instead of being composed of a useful turn in series with an ordinarily useless or idle extra turn, it is composed of two of the useful turns, one of which for the moment is reversed and is acting as a "commutator coil."

Admirable as is the conception of Mr. Sayers's arrangement, it must be admitted that his extra winding occupies a considerable space which might otherwise be occupied by useful conductors. This is well shown in his Fig. 4. One-third of the slots (that is, one-third of the whole armature) is so occupied. It says much for the Sayers winding that, in spite of this, it will give results excelling ordinary machines.

Mr. Esson was very hard on me. He said there was very little in the paper. I quite agree with him. I only aimed at putting another brick on a wall in process of erection. He intimated especially that there was no reference to what he had written in 1886. The literature of the dynamo is very extensive, and I must confess I am not acquainted with all of it; but I am acquainted with many of the excellent things which Mr. Esson has written, and I think I have studied them all at one time or another; but, unlike the papers of the Hopkinsons, of Swinburne, and of Sayers, they had not helped me in the present matter. As to matters of spelling, it really does not matter whether we agree or not.

But the real interest of Mr. Esson's remarks lies in his long-

continued efforts to make good toothed armatures. He never succeeded, and now, with as good a grace as he can, he should stand aside and let other people try. Mr. Mordey.

[Mr. Esson: I did succeed.]

Mr. Esson says that slotted armature machines were given up on the score of cost. They are, however, much cheaper than smooth-core machines, if properly made. I think, if Mr. Esson were to take some of his old machines and convert them as I have suggested, he would be so surprised at the result that he would not be able to recognise them as his own.

He refers me to Mr. Swinburne as having practically said everything and forestalled everything I have said in this paper. I need not say that I am greatly indebted to Mr. Swinburne's paper, and have duly acknowledged my indebtedness. But Mr. Swinburne has been a great deal kinder to me than Mr. Esson. He has written me a very kind and generous letter about this poor paper of mine—a letter just as generous as Mr. Esson's remarks are the reverse. Most of Mr. Esson's remarks are quite irrelevant to the subject under discussion; but I may be allowed to allude to some of them, to prevent misunderstanding. He refers to the fact that I once said the hysteresis loss in transformers got less at full load. I did say so, and many people wrote and spoke about it, and said that I could not be right. A very complete investigation was made at King's College, and was quietly published by Mr. Wilson in the *Electrician* some months afterwards. It was so extremely scientific that very few people read it, and still fewer understood it; but the result arrived at by that investigation was that I was right. The effect was a small one, and of no practical importance, and I am a little at a loss to know why it should now be referred to.

Then Mr. Esson says that I objected in 1886 to small air gaps. I did; I objected to them as it was proposed to make them at that time, and Mr. Swinburne now, in effect, says I was quite right. I objected to them because I knew their effect in the slotted armature machines which Mr. Esson was making, and I object to them still if they are made as they were then made. But I have been misunderstood if I have given the impression

Mr. Mordey. that I recommend very small air gaps, even now. I have only tried to show how to work without very large air gaps.

Mr. Esson is curiously incorrect in his reference to the position I took up ten years ago on the theories of Profs. Ayrton and Perry regarding electric motors. I only refer to this matter to prevent misunderstanding of the drift of my paper.

If my critic, instead of relying on a treacherous memory, had looked up the paper of Ayrton and Perry,* and my paper,† he would have found that the Professors advocated weak fields and relatively strong armatures,—that I showed the better plan was to use a strong field and an armature magnetically weak, so as to react on and disturb the field as little as possible. This principle has, I am glad to think, been generally adopted by electrical engineers. My present proposals follow up and are based on the same principle, providing means for still further weakening the magnetic effect of the armature—that is, reducing the armature reaction—and drawing attention to what appears to to be the best way of obtaining a strong field in the gap.

I am obliged to Mr. Raworth for so kindly referring to the actual experience of the Brush Company.

In reply to Mr. Scott, I may say that the $C^2 R$ loss is not perceptibly increased in ring armatures, while in drums it is reduced. I quite agree with his objection to soldered joints. As a matter of fact the machines actually made had not any soldered joints at all. It is a good thing to get rid of the soldering iron, which is the curse of all winding shops.

Mr. Mavor's remarks were very interesting and instructive. I wish he had given us some particulars of the sizes of the shafts, and the sizes of the armatures and the gaps which were used, particularly with reference to the question of bearing troubles referred to by himself and others. The difficulty of keeping the shafts stiff enough or true enough is one I have not experienced.

Mr. Crompton is very naturally a little indisposed to be converted to any new construction; but I hope that, if I cannot convert him, Mr. Brown will be more successful. Mr.

* *Journal*, vol. xii., 1883.

† *Philosophical Magazine*, Jan., 1886.

Crompton thinks we should not revert to internal magnets, Mr. Mordey.
because we cannot adjust the brushes. I am not specially advocating internal revolving magnets. I have only alluded to them because it seems to me that the advances made since they were originally proposed are sufficient to remove some of the objections which then stood in the way of their adoption.

For example, we know perfectly well that in many cases we do run now where we cannot adjust the brushes. There are thousands of tram-cars running to-day where no attempt is made to adjust the brushes. If we do not now adjust when the field is fixed, there is no particular reason for retaining fixed fields for the sake of this adjustment.

Mr. Hawkins concludes his interesting remarks by confessing that he cannot understand how the proposals made in my paper can improve the collection, but I am glad to think that does not exactly disprove my facts. Mr. Hawkins says chord winding has always been known. I should like to ask, Who has known or realised its effects? Where is the maker of it, in spite of the commercial competition, and all the scientific study and all the reams of mathematics which have been written on it? Who is to-day making slotted chord-wound armatures? The proof that the advantages of these things have not been known and realised, is that they have not been acted on by Mr. Hawkins and others.

Mr. Gadsby has referred to diagonal windings and diagonal poles. I remember Mr. Sayers showing me a machine at Birmingham some years ago constructed in that way; it was tried and abandoned. I think to some extent such an arrangement does give some slight useful result, but not very much—partly because the poles shield the magnetising space, and for other reasons into which I need not go now. Machines made in this way have been supplied by the Brush Company.

I am very pleased to see the contributions to the discussions sent in to the *Journal*, as I have always felt that the papers read before this Institution should be discussed in the *Journal* rather than in the various technical periodicals. I cannot, however, refer to these communications in detail (nor, indeed, to a great many points raised in other portions of the discussion), unless I

Mr Morley. write another paper; but I am sure they will add to the value of the discussion as a whole. I have to thank Lord Kelvin for sending his explanation of the absence of drag on buried conductors. The effect of the flexibility of the conductor was a point that received attention, but the possible error due to this cause appeared to be relatively unimportant.

Mr. S. G. Brown's remarks are especially welcome to me, as they so entirely confirm the experimental results I have obtained.

Mr. Atkinson's remarks deserve fuller consideration than I can now give them.

Mr. Swinburne's communication is apt and clear, so far as the special subject of my paper goes, and I thank him for it. I am glad to find that he is in general agreement with me. But Mr. Swinburne cannot write on this subject without throwing some new light on it, or raising some interesting practical question; and he now puts forward a hypothesis that is at any rate new to me. His statement that the pull is all at the fringes, and that "the "torque is devoted to overcoming the difference of pull at the two "fringes," requires some consideration. I do not say that I decline to accept it, but I may explain why I am not at present convinced. Cases are conceivable where there is no fringe. For instance, in the Ryan method, armature reaction is neutralised by placing all round the armature a belt of fixed conductors carrying current equal in amount and opposite in direction to the current in the armature conductors. In such a case there can be no difference of fringe, but obviously the torque must be just the same as in an ordinary case. Then, again, in the case of a "unipolar" dynamo, where the poles are continuous, there may be an equally distributed distortion, but there can be no fringe. If the view is correct that absence of fringe means absence of torque, then such machines should be incapable of working; but we know they do work.

In reply to Mr. Kapp's remarks on the subject of output, I can only wait for him to assure himself by actual trial that the load limit of a dynamo can actually be considerably raised in all cases where the present limit is not rise of temperature. As to

the experiment on drag, I believe the lag must have been very small, because the external circuit was practically non-inductive, and the self-induction of the coil itself also low, as shown by the very small inductive drop in the alternator. Let me say how glad I am to see Mr. Kapp once more taking part in the proceedings of this Institution. Mr. Mordey.

To those critics who object to certain proposals in my paper that they cannot give good results because they fail to satisfy the conditions of sparkless collection demanded by theory, I would, in conclusion, suggest the following considerations:—

The fundamental data of all electro-magnetic problems consist, it is true, of a few quantities related to each other individually by simple enough laws, about which there is little doubt; but, nevertheless, as soon as we have to deal with an actual problem, such as the commutation of a dynamo, we find that the interaction of the various quantities is so complex that we must content ourselves with approximate theoretical solutions. We must be careful to guard ourselves against the fact that quantities and effects which in one particular case are negligible, in another and apparently similar case may become of such great importance as to vitiate our results and necessitate a modification of our views.

An example will illustrate my meaning. The dynamo referred to in my paper, and which yielded the results shown in Fig. 9, would not work with carbon brushes, even at small load. The collection was execrable. The results obtained were with copper brushes. Other cases where the exact reverse occurs are common in my experience, and doubtless in that of others. Every practical constructor knows that very slight changes sometimes produce effects quite out of proportion to the apparent insignificance of the changes, and he has sadly to acknowledge, in such cases, that theory has utterly failed him. If he is a wise man, he will not blame the theory, but he will determine in future to theorise after the event.

The PRESIDENT: Gentlemen,—I am sure you will all agree with me that it is a matter of regret that this paper was not read earlier in the season, so that we might have devoted at least two The President.

The President. evenings to the discussion. But I can assure you it is not the fault of the Council, as we did our best to obtain this paper from Mr. Mordey at a very much earlier period. However, it is better late than never; and, as the hour is also very late, I shall do no more than ask you to accord with acclamation a vote of thanks to Mr. Mordey for his paper.

The resolution was carried unanimously.

The PRESIDENT: I have to announce that the scrutineers report the following candidates to have been duly elected:—

Associates:

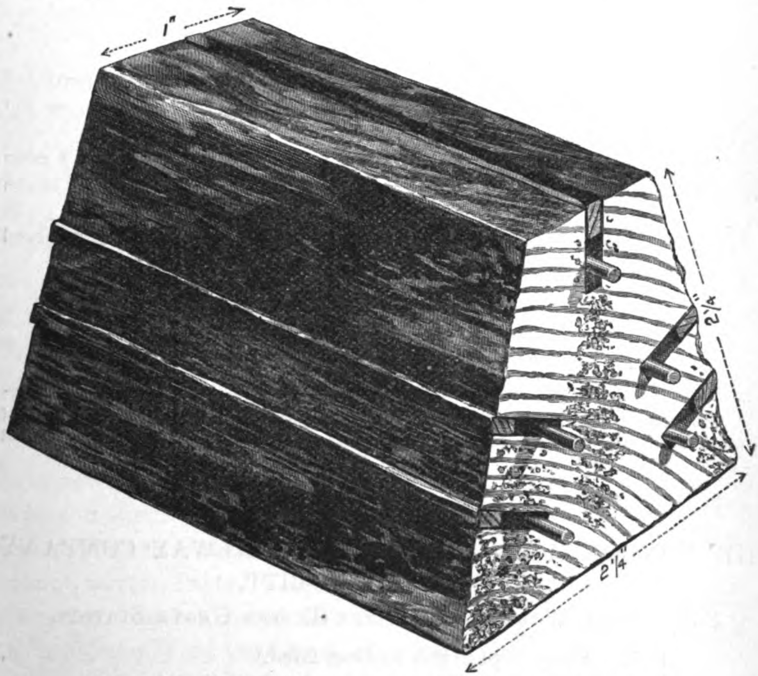
G. Chandra Bose.	Lionel Edward Harvey.
Robert Campbell.	John Douglas Knight.
John William Fielding.	Emile George Lind.
James L. Golding.	William Ramsay.

William Routledge.

COMMUNICATION.

By W. H. Preece, C.B., F.R.S., Past-President.*

In September, 1837, Mr. Cooke laid down beneath the permanent way of the London and Birmingham Railway (now the London and North Western) the first line of practical telegraph ever constructed. It consisted of five wires, and was fitted with the five-needle instruments patented by Cooke and Wheatstone on June 10th, 1837.



(Drawn to scale from the specimen in Mr. Preece's possession.)

The invoices of the work done on that occasion have recently been found in the office of Sir Douglas and Mr. Francis Fox. They were certified by Messrs. Fox's father (Sir Charles Fox), then

* In a letter to his mother, dated "Midnight, 25th July, 1837," Cooke writes: "I have just given orders for 5,000 feet of wood to be sawn in a particular manner "with grooves for the wires, which I am going to have boiled in coal tar previous "to laying down. Our wire is all ready." (See "Extracts from the Private Letters "of the late Sir William Fothergill Cooke, 1836–39, relating to the Invention and "Development of the Electric Telegraph." E. & F. N. Spon.)—Ed.

Mr. Robert Stephenson's resident engineer on the railway. They are published herewith, and are now of great historical interest. A portion of this "fossil" telegraph was dug up a few years ago, and it was used on Tuesday, June 22nd, 1897, to form a portion of the circuit through which the Queen signalled her instructions to forward her message to all the Colonies. The telegraph in England is thus coterminous with the Queen's long reign.

Victoria Mansions,
28, Victoria Street,
Westminster, S.W.,
January 25th, 1897.

W. H. Preece, Esq., C.B., F.R.S.

Dear Mr. Preece,—You kindly gave me some little time ago a portion of the first telegraph ever laid, viz., between Euston and Camden Town, on the London and North Western Railway, in 1837.

The other day, in going through some old papers, Sir Douglas and I came across the original account rendered by Messrs. Cubitt for the execution of the work.

I enclose a copy of the account, in which you will observe a small freehand sketch of the cross section of the timber.*

Believe me,
Dear Mr. Preece,
Yours very truly,
FRANCIS FOX.

* The illustration attached to Mr. Preece's communication being practically a facsimile of the actual telegraph, it has not been considered necessary to reproduce the sketch here referred to.—Ed.

[*Copy.*]

THE LONDON AND BIRMINGHAM RAILWAY COMPANY
To WILLIAM CUBITT,

For Sundry Works done at the Euston Grove Station.

1837—From Sept. 16th to Dec. 23rd.	£	s.	d.
To erecting Enclosure in Coach House for Mr. Cook's Apparatus, winding up Wire, and attending upon Mr. Cook, and Boy watching apparatus:—			
Carpenter, 16 hours; Labourers, 46 hours	1	6	10
Boy, 12 weeks and 1 day attending	7	6	0
1 : 10 cube Fir; 18 : 0 supl. ¾ rough Deal	0	12	5
13 : 0 supl. ¾ ledgd. Door; 2 cwt. 10d. Nails	0	12	6
	£9	17	9

(Signed) C. FOX.

[*Copy.*]

LONDON AND BIRMINGHAM RAILWAY COMPANY,
To WILLIAM AND LEWIS CUBITT,

FOR SUNDRY WORKS DONE AT THE EUSTON GROVE STATION, &c., IN ATTENDING UPON MR. COOK, AND ASSISTING TO FIX WIRES AND WOODWORK, &c., FOR SIGNALS ON THE ELECTRICAL APPARATUS, &c.

	£	s.	d.
Carpenters, 50¼ days; Labourers, 143¾ days	43	16	6
Excavators, 82 days; Boys, 83 days 6 hours	28	17	2
Watchmen, 12 days 3 hours	2	6	1
235 Stocks; 450 Plain Tiles	1	15	9
570 Drain Tiles; 10 bushels Sand	6	18	7
18 : 9 feet cube Fir	2	19	4
6 : 0 supl. ½ Deal; 80 : 8 supl. ¾ Deal	0	18	3
15 : 2 supl. inch Deal; 10 : 0 supl. 1¼ Deal	0	9	10
4 : 1 „ 1½ Deal	0	2	0
10 : 0 run ¾ Batten; 2 : 6 run Fir, 2 × 2	0	2	8½
20 : 0 run Deal, 2 × 1½; 14 : 0 run Fir, 3 × 1½ ..	0	3	5
12 : 0 run „ 2 × 1½; 9 : 0 „ 5 × 1½ ..	0	4	3
8 : 0 „ Fir, 3½ × 2½	0	1	8
Deal Box, with Lid, &c.	1	7	6
½ cwt. 3d. Nails, 48 cwt. 4d., 5 cwt. 6d., 2 cwt. 10d., 2¼ cwt. 20d., ⅛ cwt. 2s. Nails, Nails 9d.; 4½ doz. 1½ Screws; 6 Brooms	1	10	5
Horse and Cart fetching sundry materials	0	7	0
Brassworker's time assisting with Copper Wire, paid ..	4	9	11
Solder, Charcoal, Paper, Pliers, and Sundries	2	4	5
Charcoal and Basket, 2s. 6d.; New Pail for Tar, 2s. 6d.	0	5	0
1 6-inch Brass Ward Dead Lock, with 2 Keys	0	7	6
1 pair 18″ Cross Garnets	0	3	0
2 brass-bushed Padlocks, with Hasps, Staples, and 2 Keys	0	6	0
3 Clearing Hooks for Conducting Wires	0	3	9
FT. IN. 5,524 : 0 run Fir in Scantling, sawn triangular, and grooved for copper wires, including all Labour; to preparing and tarring same over, and Tar to ditto..	69	1	0
27,620 Deal Tongues to ditto ditto ditto	18	8	3
Tar for Bedding the Fir in at the Extension, &c. ..	1	15	0
	£189	14	4½

ABSTRACTS.

G. LE BON—ON THE ELECTRIC PROPERTIES OF RADIATIONS EMITTED BY BODIES UNDER THE INFLUENCE OF LIGHT.

(*Comptes Rendus, Vol.* 124, *No.* 17, *April*, 1897, *p.* 592.)

The author in the first place refers to the objections raised against his previous experiments; and mainly against the optical transparence possessed by ebonite.

All doubts on the point were removed by employing an ebonite disc 2 to 3 mm. thick instead of 5- to 7-10ths of a millimetre. On one side of the disc was fixed a metallic star, and on the other side a photographic film, half of which had been fogged.

If this be exposed to the sun for one hour, an image will be seen on the fogged portion, but not on the other.

The ebonite may be replaced by any opaque body, such as a sheet of metal, and the sensitive plate may be replaced by a phosphorescent sheet of sulphide of zinc exposed for one second to light.

The metallic star may also be replaced by such optically transparent bodies as mica or quartz, or some samples of tracing paper, to prove that the experiment is not affected by mere opacity.

In these experiments it is essential that the exposures should be as accurate as possible.

The author next deals with the action, on the electroscope, of radiations emitted by bodies exposed to light.

This action has been produced by several methods, which have yielded analogous results.

The first set of experiments was carried out with samples 1 cm. square by 1 mm. thick. In each case the instrument was charged to the same potential, and the time taken for the leaves to drop 10° was taken as unity.

It is found from these observations that all bodies acted upon by light produce a loss of either negative or positive electricity.

The loss of charge is much more rapid if the charge of the electroscope is negative; but with a large number of bodies the effect is independent of the sense of the charge. The rate of discharge varies widely with different bodies, as shown by a number of experimental results, and when the sense of the charge is immaterial. With pure and amalgamated zinc and aluminium, the discharge is considerably more rapid if the electroscope has received a negative charge. These radiations, which are excited by the action of light, appear to exist on the surface of bodies. They retain for a certain length of time the property of discharging the electroscope in the dark (1° to 2° approximately per hour). This property is greatly influenced by the surface of the substances, especially in the case of metals. The cleanness of the surface is of importance, but not the degree of polish.

The effect due to cleaning disappears very quickly with time. Such effects as the momentary action of heat or of an electric current, the momentary immersion

into a bath of alcohol followed by simple evaporation, considerably slow the phenomenon; and this is also the case when a sheet of colourless glass is placed over the metal which is exposed to light.

Most monochromatic lights act in the same manner as ordinary lights.

It is concluded from the above investigations that all bodies possess, at least as far as their action on the electroscope is concerned, properties of the same order as those strongly manifested by uranium, and as demonstrated by M. Becquerel. The properties of uranium would then form a particular case of a general law.

M. PERRIGOT—ON BLACK LIGHT.

(*Comptes Rendus, Vol.* 124, *No.* 16, *April*, 1897, *p.* 857.)

The author criticises M. Le Bon's communication (*C. R.*, 5th April, 1897) in which was described some of the properties possessed by black light. In these experiments a fogged sensitive plate is covered with a thin sheet of ebonite, about ½ mm. thick, on which metallic figures are stuck. When the plate has been exposed underneath the ebonite for three hours to diffused light, the images of the letters are observed in black on a grey ground.

M. Perrigot has repeated these experiments, and has obtained the same results as M. Le Bon, but considers his explanation erroneous, as it can be proved that ebonite ½ mm. thick is not completely opaque to white light. If the experiments be repeated with the same kind of photographic plate which has not received any initial exposure, then the images are reversed; the metallic letters appearing light against a dark ground,—the two cases therefore contradicting one another.

It is, moreover, possible to observe the light of an arc lamp through the sheet of ebonite, and this light would affect a sensitive plate.

The author considers that the facts published by M. Le Bon are due simply to the transparence of ebonite to white light, and are explained by the well-known phenomenon of the reversal of photographic images, the law of which was published by MM. A. and L. Lumière (Soc. Française de Photographie, July, 1888), and of which the author gives a description.

A. BLONDEL—ON PHOTOMETRIC UNITS.

(*Journal de Physique, Vol.* 6, *April*, 1897, *p.* 187.)

It was proposed by the Committee of the International Congress of Geneva that M. A. Blondel should present a report, with the object of establishing a complete system of defining the dimensions and units of photometry, and to make these as accurate as other branches of physics.

The author deals with the subject under the following headings:—

1. *Photometric Dimensions.*—In order to avoid a confusion of terms, it is suggested to apply to light analagous definitions to those employed in the study of magnetism, and to introduce three fundamental quantities—the luminous flux, which is analogous to the magnetic flux; the intensity of a constant source, which is analagous to the intensity of a magnetic pole; and illumination, which plays the same part as magnetic induction. A table is given embodying the name of the physical quantity, the definition, and the symbol and equation of the definition.

2. *Standards.*—As photometric quantities are of a physiological order, one is unable to give them a physical value, and it becomes necessary to adopt empirical standards. The standards employed at present form two groups—flame standards and incandescent standards. The advantages and disadvantages of the different types in use are discussed. With flame standards generally, it is necessary to avoid the use of a screen before the flame, and to take into account, not only the variation in the composition and pressure of the combustible, but also of the supporter of combustion. The two best flame standards are the Vernon-Harcourt pentane-gas lamp, and the Hefner amyl-acetate lamp. The latter is the more reliable as an instrument of precision, but its use involves many precautions.

Incandescent standards, although theoretically perfect, are very difficult to manipulate.

The most rational system in use is that due to M. Violle, which consists in the use of platinum at the well-defined temperature of solidification. As international standards the author proposes the use of a primary platinum standard, kept in a standard laboratory, and the Hefner lamp to be used as a secondary standard, and = 1·02 or 1·06 "bougie-decimalé" ($\frac{1}{20}$ violle).

3. *Units.*—The want of a concrete standard makes it necessary to adopt as fundamental unit, not the unit of flux, but the unit of luminous intensity, which is both physical and physiological; and it is also necessary to add the units of length and time—the metre and the hour.

The units proposed by the author, with their definitions, are stated in a table.

4. *The interpretation of photometric units in heterochrome photometry* is next considered, but, owing to the newness and complexity of the subject, the author merely refers to the work done by MM. Charpentier, Mace de Lépinay and Nicari, Crova, and others.

This report was submitted for the examination of a commission presided over by Von Hefner Altenek, and was accepted, subject to a few modifications.

The following are the decisions adopted:—

1. The international photometric values will be based on the luminous intensity of a regular source, and are given in the following table:—

Dimensions.	Name of Unit.	Equation of Definition.
Luminous intensity	Bougie-decimalé	I
Luminous flux... ..	Lumen	$\phi = I\,M$
Illumination	Lux = $\dfrac{\text{Lumen}}{\text{Square metre}}$	$E = \dfrac{\phi}{S}$
Brilliancy...	Bougie-decimalé per square centimetre	$e = \dfrac{I}{S}$
Light...	Lumen-hour	$Q = \phi\,T$

2. The unit of luminous intensity is the decimal candle.

3. The bougie-decimalé may be provisionally represented for industrial requirements by the horizontal luminous intensity of the Hefner lamp, on the condition of making the necessary corrections.

M. H. DESLANDRES—A NEW PROPERTY OF THE CATHODE RAYS, WHICH REVEALS THEIR COMPLEX NATURE.

(*Comptes Rendus, Vol.* 124, *No.* 18, *p.* 945.)

In a previous communication (*C. R.*, p. 678, 29th March, 1897), the author published the results of experiments carried out at the Paris Observatory on the mutual action of cathode rays, and bodies placed in the interior of tubes containing rarefied gases.

It was shown that all these bodies, whether conductors or insulators, influence the direction of the cathode rays, if they are anodes and merely insulated. When they are cathodes or connected to earth, the action is generally stronger and accompanied by repulsion. In the first researches the author observed the following general fact, which he subsequently investigated by special experiments:—

When a cathode ray is deflected by a neighbouring body, it generally divides itself at the same time into several distinct beams, which are unequally deflected. The secondary rays which were combined in the primary ray are thus separated.

This property of the cathode rays had not hitherto been published. The apparatus employed involves the use of a tube containing two electrodes. One of these consists of a screen pierced with a fine slit; on one side is placed the main cathode, and on the other side of the screen is seen a shadow projected by the screen on the glass, and in the centre the brilliant line of the slit.

This curious division of the rays is not merely characteristic to this tube fitted with two cathodes; it takes place also with the disc which constitutes the cathode of ordinary tubes, in the portion of the tube where this disc is connected to the glass by a wire normal to the disc.

The same phenomenon is also observed, under the conditions where the interference surfaces of Jauman and the "summation gebilde" of Wiedemann and Schmidt are found. In the tube employed by the author, the image of the slit in the screen is represented by a line of light at the end of the tube, and if the cathode beam be divided up a number of less intense and unequally divided lines are observed. If different coils be employed with the same tube, the images of the dispersed lines present differences, but at the same time have points in common.

The greatest deviation of the lines appears at very low pressures.

The author suggests that this division by the action of a neighbouring body may be analogous to the action of the magnet in the experiments of Lénard and Birkeland. The want of suitable magnets has prevented the author from ascertaining whether the division of the beam, were the same in the two cases. These experiments lead to the following questions:—Have the unequally divided rays the same rate of propagation? Are they emitted successively or simultaneously? How does their deviation vary with increasing distances of the cathode? The difficulty of making observations in a vacuum does not allow of an immediate answer. These researches have an interest, not only to physicists, but also to astronomers.

The conditions necessary for the production of cathode rays exist in celestial bodies, and particularly in the solar atmosphere.

The author suggests that the theory of the division of the cathode rays might explain the division of the tail of a comet.

N. EGOROFF and N. GÉORGIEWSKY—ON THE PARTIAL POLARISATION OF RADIATIONS EMITTED BY SOME LUMINOUS SOURCES UNDER THE INFLUENCE OF A MAGNETIC FIELD.

(*Comptes Rendus, Vol.* 124, *No.* 18, *p.* 249.)

In their previous communication of April 5th, the authors described the method of observing the partial polarisation of emission of flames and of sparks under the action of a moderately strong magnetic field.

A continuation of these researches have led to the following results:—

1. The Babinet compensator, and also the Savart analyser, exhibit the partial rectilinear polarisation of the equatorial radiations, and the elliptic polarisation of opposite sign of the radiations inclined to the equator of the field.

2. The relative quantity of equatorial radiations emitted by the sodium flame and polarised in a rectilinear direction, varies with the intensity of the magnetic field, by following a particular course.

In these experiments one or two strips of glass were used to compensate for the rectilinear polarisation of equatorial radiations by refraction.

3. Under the influence of a magnetic field of given intensity, the quantity of light polarised in a rectilinear direction and emitted equatorially by the sodium flame varies with the temperature of the flame.

4. The authors stated in their previous communication that the partial polarisation is observed as clearly in induction sparks between electrodes of magnesium; whereas with electrodes of copper, zinc, carbon, &c., no signs of polarisation are observed with the Savart analyser.

In further investigating the influence of a magnetic field on the sparks between different metals, the spectrum of these metals was observed with a field intensity of 7,500. For this purpose a direct-vision spectroscope was used, the spectrum being observed through the Savart analyser. After refraction in the prisms, the black Savart lines are observed on the spectrum bands.

5. The hydrogen and helium lines in Geissler tubes have so far not yielded definite results.

The authors conclude their communication with a remark concerning the Drummond flame coloured with amianthus soaked in common salt. On several occasions a small displacement of the fragment of amianthus in the flame was observed on closing the circuit of the electro-magnet; and this displacement produces such great and sudden variation of temperature of sodium that the authors were able to observe, not only the exceptional enlargement of the bands D_1 and D_2, but even their reversal.

After breaking the circuit of the electro-magnet, the amianthus instantly resumes its original position in the flame, and the bands their original appearance.

J. R. MOURELO—RESEARCHES ON SULPHIDE OF STRONTIUM, AND A METHOD FOR OBTAINING IT IN A VERY PHOSPHORESCENT CONDITION.

(*Comptes Rendus*, *Vol.* 124, *No.* 19, *May*, 1897, *p.* 1024.)

Mr. Verneuil some years ago (*C. R.*, vol. cii., p. 600, and vol. civ., p. 501) showed the influence which different substances had on the intensity of phosphorescence of sulphide of calcium.

The author has closely studied the conditions of phosphorescence of sulphide of strontium, by employing the original conditions for obtaining this substance.

The method consists mainly in the reduction of the artificial sulphate of strontium by means of carbon.

The author has, however, modified the method previously employed, and has had recourse to a process which yields sulphide of strontium possessing very strong phosphorescent properties of a greenish blue colour. It is prepared by taking 285 grammes of impure commercial carbonate of strontium, 62 grammes of fluor spar, 4 grammes of crystallised carbonate of soda, 2·5 grammes of chloride of sodium, and 0·4 gramme of subnitrate of bismuth; the mixture is thoroughly powdered, compressed in an earthenware crucible, and covered with a layer of powdered starch not exceeding 2 cm. in thickness. The crucible is placed in an oven and heated to a bright red for five hours, and then allowed to cool slowly for 10 or 12 hours, after which time an almost white agglomerate can be removed from the crucible, possessing very strong phosphorescent properties, which the least light will provoke. The author has confirmed the observation made by M. Verneuil that most of the sulphides of strontium which he has prepared lose their phosphorescent power when they are reduced to powder; but in certain cases these powders when mixed with starch and heated to a bright red for five hours may regain their phosphorescent properties. The author hopes to publish shortly the results of numerous experiments which he has carried out on phosphorescent metallic sulphides.

H. BECQUEREL—THE EXPLANATION OF SOME OF M. G. LE BON'S EXPERIMENTS.

(*Comptes Rendus*, *Vol.* 124, *No.* 19, *May*, 1897, *p.* 984.)

M. Perrigot (*C. R.*, vol. cxxiv., p. 857, April, 1897) criticises the conclusions which M. Le Bon deduces from certain experiments.

The fact that the sheets of ebonite used in these experiments are transparent to active radiations is undeniable; but the author shows that the phenomena which are observed are not due to what is known as white light, but to the red and infra-red radiations of the spectrum, to which ebonite is very transparent.

The experiment of M. Le Bon to which the author alludes, consists essentially in exposing to light a surface coated with phosphorescent sulphide of zinc, and then covering it with a sheet of ebonite on which different metallic objects are placed, and then exposing to the light for several seconds. On examining the phosphorescent surface in the dark, it was found to be no longer

phosphorescent, except where the metallic figures had been placed. M. Le Bon's conclusion from this experiment, was that the metal emitted rays which excited phosphorescence.

This explanation is, however, erroneous.

The red and infra-red rays of the sun pass through ebonite, and, as has long been known, extinguish phosphorescence over the entire luminous surface, except at such places where the metallic figures act as screens, which explains the above observations. The author repeated these experiments with a sheet of red glass, with the same results, but more intense than with ebonite. These results can be repeated with any other substance stopping blue, violet, and ultra-violet rays, and allowing the infra-red rays to pass. M. Le Bon's results are further explained by experiments made by the author's father in 1840, in which it was found that a photographic plate which was insensitive to the yellow and red rays, became sensitive to these rays, and even to the infra-red waves, when slightly fogged. The red and infra-red rays continue the action commenced by the white, blue, or ultra-violet light. M. Le Bon found it essential to the success of his experiments that the plate should be lightly fogged.

The above somewhat complex phenomena, which M. Le Bon attributed to a "black light" of unknown nature, are really due to the effects produced by the red or infra-red waves, of which the chief properties have been known for more than 50 years.

J. R. MOURELO—THE PHOSPHORESCENCE OF SULPHIDE OF STRONTIUM.

(Comptes Rendus, Vol. 124, *No.* 22, *May,* 1897, *p.* 1237.)

It has been shown that the pure sulphides of barium, calcium, and strontium are not phosphorescent, even after having been exposed to direct solar light for long periods; neither do the polysulphides possess this property.

The monosulphides are only capable of phosphorescing when they agree with the formula M. S, M being equal to

$$Ba - Ca - Sr;$$

on the condition, however, that they should contain alkaline compounds (carbonate and chloride of sodium) in a very small quantity, and subnitrate of bismuth, as has been shown by M. Verneuil for sulphide of carbon, and which the author has himself verified in the case of sulphide of strontium.

The conditions of phosphorescence in this last body offer a number of characteristics which the author has observed in the course of his experiments.

By oxidising sulphide of strontium at a high temperature, prepared by the reduction of the sulphate and also by other methods, the author was able to gradually diminish the amount of phosphorescence, and to eventually lose it, and even to convert the sulphides into sulphates of strontium.

In many cases the substances regained their phosphorescence by the addition of carbon, and by submitting the mixture to a high temperature; but under these circumstances they acquire a grey or brown tinge. If one uses sulphide of strontium prepared according to the method employed by M. Verneuil for sulphide

of calcium, or the modification proposed by the author, then different results are obtained than by the ordinary methods.

The author states in conclusion that a system of oxidation is necessary, as well as a particular structure of the substance, in order that sulphide of strontium may possess the property of phosphorescence, and that the presence of certain substances have a direct and positive effect on this phenomenon.

M. PERRIGOT—ON THE TRANSPARENCE OF EBONITE.

(*Comptes Rendus, Vol.* 124, *No.* 20, *May*, 1897, *p*. 1087.)

In his communication of April 20th, the author stated that ebonite sheets are transparent, and that the phenomena attributed to black light are explained by the fact of the well-known photographic reversal.

Further experiments were made with the utmost care on sheets of ebonite 0·5 mm. thick, and which had been carefully polished. The ebonite appears to behave in the same manner as a colour screen. If a thin ebonite sheet be placed before an intense source of white light, the eye perceives a weak light in which the orange-red radiations predominate. With orthocromatic plates sensitive to red and yellow, the same, but more marked, results were obtained. With sheets 2 mm. thick photographic impressions can be obtained, although they do not appear transparent to the eye. The conclusions arrived at by the author agree with the experiments recently described by M. H. Becquerel.

C. MALTÉZOS—ON THE CATHODE RAYS AND SOME PHENOMENA IN VACUUM TUBES.

(*Comptes Rendus, Vol.* 124, *No.* 2, *May*, 1897, *p*. 1084.)

The tubes employed in these experiments were pear-shaped, the electrodes ending in two aluminium discs placed at each end of the tube with their surfaces at right angles to one another. At the anti-cathode is produced a luminous ring with a central spot.

When the disc on the narrow portion of the tube is made cathode, the following are some of the pnenomena which are observed :—

1. If the tube near the cathode disc be touched with the finger, an attraction of the cathode rays is observed; the glass near the finger becomes more phosphorescent, and the whole cathode beam is deflected towards the hand. It is simultaneously observed that on the anti-cathode ring the phosphorescence becomes stronger near the finger, and that the central spot undergoes a change from a circle to an ellipse, as though submitted to a pressure of which the small axis points towards the finger. The phenomenon also takes place if the tube be touched with one of the armatures of a Leyden jar the other armature of which is connected to earth ; and it appears that every conductor touching the outside of the tube is electrified by influence, and that the negatively charged body is within the tube.

2. If the conducting wire connected between the cathode and the coil is placed

near the tube, or in contact with it, the beam is also attracted towards the wire; and if a series of sparks passes between the wire and the tube, the cathode beam is alternatively attracted and returns to its original position. The electrical charge in the wire is also negative; consequently, if the phenomenon were due to electrostatic influence, there would be repulsion. The phenomenon is due to the electro-magnetic action between the current which starts from the cathode along the wire and the current of the cathode system.

3. The first phenomenon shows that in the cathode phenomenon there is influence due to electrified matter; the second shows also that this matter travels along the cathode rays, starting from the cathode. The author further discusses the above phenomena at some length.

4. If the anti-cathode system be examined, it is seen to consist of a not very brilliant central spot, and of alternately dark and bright rings; and the same thing is observed on the phosphorescent Röntgen screen.

In order to better examine this phenomenon, the action in an exhausted spherical tube was investigated, in which the electrodes consist of aluminium wires, one being double the length of the other.

When the long wire is used as cathode, there does not appear on the side of the tube an entire phosphorescent zone, of the height equal to the length of the electrode, but bands parallel to the wire, alternately bright and dark. With one of the tubes the author counted more than 20 bright bands.

5. If the strength of the current increases, the diameter of the luminous rings decreases. This phenomenon may be explained by the visibility of new rings with an increase of current (Birkeland's phenomena), or by the increase in the velocity of the cathode rays, and consequently by the electro-dynamic attraction of these rays.

G. LE BON—ON THE PROPERTIES OF CERTAIN RADIATIONS OF THE SPECTRUM.

(*Comptes Rendus, Vol.* 124, *No.* 21, *May*, 1897, *p.* 1148.)

Mr. Becquerel has explained some of M. Le Bon's experiments, by the transparence of ebonite to the less refrangible portion of the spectrum.

The following observations show that the above interpretation does not account for all the phenomena observed:—

1. If the action is due to the red rays passing through the ebonite, then a sheet of green glass placed over the ebonite should prevent the formation of an image. The image is produced a little slower with the sulphide of zinc, but not at all with the fogged photographic plate. About the same results are obtained with any monochromatic light.

2. If the metallic figure placed over the ebonite acted as a simple screen, any bodies placed under the figure should not alter the impression. Certain bodies—notably mica—placed under the metal during exposure, produce an impression, providing the exposure has been sufficiently long.

3. The presence of active radiations under the metal can also be proved by lengthening the exposure. With suitable plates the image gradually diminishes

in density, and finally disappears. It is therefore evident that some effect due to the metal must have acted on the plate.

4. The ebonite may be replaced by any opaque body, such as a sheet of black paper or a sheet of metal. In order to obtain an image, it is only necessary that the superimposed substances should present different opacities to these radiations.

Further, if these experiments be made with ebonite or with metal, and long exposures be given with one half exposed to light and the other half in the dark, then the action of the radiations extends under the portion which is not illuminated, and the author maintains that this action is due to what he has termed "black "light." He hopes to show soon that this form of energy possesses several properties common to electricity and to light which makes it difficult to class it. It appears to occupy an intermediate position and is no doubt characterised by a great wave-length. With regard to the thickness of ebonite employed, the author states that with pure ebonite he has obtained good results up to ½ cm. thick, which would be quite opaque to ordinary light. An object enclosed in an ebonite box of this thickness would give an image after a few seconds' exposure to light on a phosphorescent screen enclosed in the box.

Even if a body of this thickness allows the red waves to pass, it does not affect the results obtained by the author, since these can be produced through substances which are perfectly opaque to red light.

M. GOUY—ON THE REFLECTION OF LIGHT BY A LONG AND NARROW SURFACE.

(*Comptes Rendus, Vol.* 124, *No.* 21, *p.* 1140.)

MM. Nichols and Rubens (*Phys. Review*, 1897) found when operating with heat waves of great length (24 η.) that a long and narrow strip of silver reflects these rays, and polarises them in a direction perpendicularly to that of the length of the strip. The author in 1886 had published an analogous fact in connection with light waves. This phenomenon was essentially similar to that observed by the above workers, but with them the width of the reflecting surface must be extremely small, owing to the smallness of λ; consequently, the experiment necessitates the use of strips of steel of great perfection.

C. MALTÈZOS—ON A PHOSPHORESCENT ANTI-ANODE SYSTEM, AND THE ANODE RAYS.

(*Comptes Rendus, Vol.* 124, *No.* 21, *May*, 1897, *p.* 1174.)

This forms a continuation of the author's researches (*C. R.*, vol. cxxiv., p. 1084). It was found that in an exhausted pear-shaped tube, when the electrode in the narrow part is made cathode, there appears before the anode a diffused bluish light of confined area.

1. When the strength of the current is not very great, there appears at the anti-cathode a similar spot to that at the cathode, but less brilliant.

This spot consists of a dark circle and of a brilliant ring. The effect becomes

more marked if the tube be touched with the hand. With a strong current this anti-cathode system is not formed, and instead of the dark central spot there exists a phosphorescent spot, as in other parts of the tube; the first bright ring of the anti-cathode system being replaced by a dark ring. This is explained by the fatigue of the glass; but it is shown at the same time that the anti-anode system is a phenomenon of the same order as the anti-cathode system, with regard to the results on the glass. If the enlarged portion of the tube be touched, the anode light is repelled, but there is at the same time a concentration of phosphorescence on the luminous ring of the anti-anode system in the opposite direction to the finger. If the finger be moved over the tube, the luminous concentration also moves over the tube in an opposite direction.

From this the author concludes that the anti-anode system is intimately connected with anode light.

2. This system gives rise to a curious phenomenon due to the condensation of electricity by the vacuum tube.

If the finger be held against the tube when it is working, and the finger be removed after breaking the current, a flash is observed in the tube, which emanates from the anti-anode system, and becomes very phosphorescent the moment the finger is removed.

It is further observed that if the finger be held just over the anti-anode, and the circuit be broken, a similar flash is obtained, and another if the tube be touched again at the same place; in this manner five to six flashes can be obtained.

Further experiments which were made indicate the existence under certain conditions of anode rays, which excite the visible and invisible phosphorescence of glass, and which are diffused or do not reach the glass during the greater part of the time.

M. DESTOT—PHYSIOLOGICAL AND OTHER TROUBLES DUE TO THE "x" RAYS.

(Comptes Rendus, Vol. 124, *No.* 20, *May*, 1897, *p.* 1114.)

It was thought that there existed an allegory between the skin troubles experienced by those who experiment with the x rays, and the effects due to the sun-stroke; they, however, differ in several ways.

1. The rays are not felt when one is first exposed to them.

2. From the moment of exposure to the rays to the time when the troubles appear, there is sometimes a considerable lapse of time, varying from 48 hours to 20 days. It becomes difficult to admit of a direct action on the tissues, producing physical and chemical changes, which have effect at the end of such a period.

3. The distance from the tube to the skin plays a very important part. Beyond a certain range the troubles do not appear at all.

4. The effects on the skin may, however, be stopped by interposing a sheet of aluminium connected to earth, which, however, allows the x rays to pass.

The troubles do not occur when the static machine is employed as a source of electricity. In this case, however, the x rays possess the same physical properties.

These observations were confirmed by Frei (*Electrical Engineer*, February, 1897). These troubles are, then, due to the electric wave, its frequency, and its shape, and not to the x rays themselves. There is produced around the tube an electrical atmosphere possessed of characteristic properties, and which acts on the nervous extremities. The electrostatic field thus produced has a limit which varies with the apparatus employed, and which can be shown by the radiometer, which lights up under its influence.

The author does not consider that experiments carried out on animals are of any value.

A. NIZZOLA—THE TRANSMISSION OF ELECTRICAL ENERGY BY POLYPHASE CURRENTS AT ROMAGNANO.

(*L'Éclairage Électrique, Vol.* 11, *No.* 23, *p.* 447.)

This installation is driven from the water power of the Sesia, and is used for driving the paper mill of MM. Vonviller & Co., where wood is made use of for the manufacture of paper.

This hydraulic station is situated at Giapola, where a fall of 10½ metres is available, with 8,000 litres of water. Messrs. Brown & Boveri contracted for the electrical plant, working on the two-phase system.

This station has worked for 2½ years without intermission either day or night.

The power available at the turbines is 810 H.P.

The all-round efficiency amounts to 80 per cent., consequently 648 H.P. are available at Romagnano.

Three generators are installed, working at 3,600 volts, and a frequency of 83·3.

An overhead line, with four conductors, transmits the power to the works.

Seven transformers of 90 kilowatts each are used for reducing the pressure to 230 volts, and two transformers of 15 kilowatts are used for the lighting circuits.

There are installed six motors of 120 H.P., and a number of small ones varying between 3 and 15 H.P.

The hydraulic installation was designed by Ling.

The turbines, of 270 H.P. each, are of the Theodore Bell type, and are direct coupled to alternators of the so-called umbrella type, running at 180 revolutions per minute. The armatures are stationary, and the cast-steel revolving field magnets are so designed as to have half the number of coils as there are poles. The armatures are built up of laminated iron discs, and are tunnel-wound. The pole-pieces are solid. The weight of the complete alternator is about 12 tons. The exciters are of the Manchester type, direct coupled to 25-H.P. turbines running at 500 revolutions per minute. One exciter suffices for three alternators.

The overhead line has four conductors of 8 mm. diameter. The secondaries of the transformers are so arranged that they can be connected to a balancing circuit having only three wires of equal section for the sake of simplicity, although one of them has only to carry $\sqrt{2}$ times the current of the two others. It was essential that the motors should start at full torque without taking too large a current, this being effected by placing resistances in the rotor circuit. These resistances are

carried on the rotor itself, and are automatically cut out by a special device fitted to the rotor spindle, thus obviating the use of collector rings.

No trouble was experienced by running lamps off the same circuits as the motors.

L. HOULLEVIGUE—ON THE THEORIES OF RESIDUAL ELECTRICITY.

(*Journal de Physiqve, Vol.* 6, *May*, 1897, *p.* 253.)

The author corrects an interpretation of one of M. Blondlot's well-known experiments on residual electricity, which he lately published (p. 113, *Jour. de Phys.*).

In this experiment the electro-magnetic waves do not pass through dielectric plates of glass or sulphur normally to their thickness, since the electric force is itself normal to these plates. The author consequently corrects his previous reasoning. This, however, does not concern the general ideas developed by him in the portion of his work which is independent of M. Blondlot's experiments.

L. HOULLEVIGUE—EXPERIMENTAL STUDY OF ELECTROLYTIC IRON.

(*Journal de Physique, 3rd Series, Vol.* 6, *May*, 1897, *p.* 246.)

Iron obtained by electrolysis of its salts in aqueous solution, contains hydrogen, and differs clearly from soft iron. It is nearly as hard as hardened steel, very brittle, and capable of being permanently magnetised. The author's experiments were carried out with the object of determining the electric and magnetic properties of electrolytic iron, and to clearly differentiate the different kinds of iron, cast iron, and steel.

The properties of electrolytic iron depend on the composition of the bath, and on the current-density. Deposited iron contains more hydrogen when chlorohydrate of ammonia is added to the protochloride of iron which serves as electrolyte; the amount of hydrogen also increases with the current-density.

The density of the iron is not easy to measure; the author employed the flask method, using petroleum maintained at 0°, and found 7·324 as the value of the density.

For measuring the specific resistance it was found impossible to obtain wires of uniform section; the author therefore had recourse to the method of depositing iron over a metal spiral, dissolving this off with sulphuric acid, and then depositing copper over the same surface. The two deposits of iron and copper are not uniform, but their thicknesses are everywhere proportional, as the current-density is the same at any point for the two deposits.

The result works out to $\rho = 127{\cdot}5$ in microhms-centimetres. Assuming that $\rho_1 = 1{\cdot}45$ for the specific resistance of copper, this result would then place electrolytic iron beyond the hardest of cast iron.

For soft iron $\rho = 14$ approximately, and ranges between 30 and 60 in the case

of steel; and for cast iron the value is higher than this, and may reach 114 for hard castings.

The coefficient of variation of resistance with temperature works out to $X = 0{\cdot}000958$.

For soft iron $X = 0{\cdot}0045$; for hard steel, $0{\cdot}0020$; and for hard cast iron, $0{\cdot}00105$.

The magnetic tests were performed by the oscillation method, working in a known magnetic field, produced by a solenoid. The results obtained are embodied in curves. These curves are exactly of the same shape as those obtained by Rowland, but the maximum magnetisation is lower. This is no doubt due to the method employed. The point to be observed by closely examining these curves is that electrolytic iron, as far as its total magnetisation as well as its permanent magnetisation are concerned, stands between soft and hard steel.

The author has also studied the effect of transverse magnetisation on the electrical resistance, and has shown that the temporary transverse magnetisation produced by a field of 2,500 C.G.S. units, does not cause that property to vary by more than 1-20,000th of its value. On the other hand, the permanent transverse magnetisation produces an appreciable diminution of resistance.

The conclusion which the author draws from his work is that the analogy between carburetted and electrolytic iron is perfectly justified by the comparison of physical properties of these two materials, and is such that the quality of the steel reveals more the molecular structure of the molecule than the chemical nature of the body associated with iron.

J. L. ROUTIN—SOME CONSIDERATIONS ON THE DISTRIBUTION BY POLYPHASE CURRENTS.

(*L'Éclairage Électrique, Vol.* 11, *No.* 23, *p.* 439.)

Where polyphase currents are well adapted for long-distance transmission, continuous-current installations are becoming more restricted to lighting circuits of small radius (3 kilometres) and for traction work.

There exists an advantage with continuous currents in a small station where accumulators are employed, and by the use of which the working efficiency of the station can be increased.

A three-wire continuous-current system is no less complicated than a polyphase circuit. With alternating currents it becomes possible to transmit 10,000 H.P. for 100 kilometres at 20,000 volts at a cost below 15,000 francs per kilometre, and with an efficiency of 82 per cent.

In the case of arc lighting there is an alleged advantage in favour of continuous currents. It must, however, be borne in mind that the resistances employed sometimes absorb 20 per cent. of the energy, whereas there is very little loss in choking coils; and, further, in the case of large installations the cost per kilowatt works out less with alternating currents than with continuous currents.

To avoid accidents with high-tension circuits, the author recommends that in thoroughfares where insulated conductors are used, these should always be lead-covered, with the covering connected to earth. Where overhead high-

tension leads are used, it is advisable to always place them below the telegraph and telephone wires, and to place a protecting net below these latter wires.

Beyond a pressure of 5,000 volts it is not wise to use simple armoured cables for underground work. It is well to have recourse to a special conduit; or the Brown system of oil insulation may be employed, which consists in placing the bare conductors in pipes filled with oil and keeping them apart by means of porcelain rings. The author discusses the advantages of synchronous motors over continuous-current motors.

Three-phase generators are, under the same conditions, slightly cheaper than two-phase generators, and their efficiency is a little higher. With the former system there is also a higher efficiency with the transformers. With regard to regulation, there is no difficulty with the three-phase system when the circuits are approximately equally loaded. At Rheinfelden and Lyons the regulation at constant speed and excitation with $\phi = 0{\cdot}8$ is within 12 per cent. With regard to the motors, there is not much difference between the two systems, for the same proportions; there is, however, a greater leakage with the two-phase, and consequently smaller starting torque. With the three-phase system there is also an economy of 25 per cent. on the cost of the line.

With regard to frequency, it is advantageous to work between 20 and 40 ∿ as far as the line and generators are concerned; but, considering the transformers, their size, efficiency, and price, a frequency between 40 and 50 is found to be advantageous.

The best frequency for motors is between 30 and 50 ∿. The limiting frequency where incandescent lamps are used is 35 ∿. For public arc lighting 40 ∿ may be employed, but complete steadiness can only be obtained at 45 to 50 ∿.

Taking all these considerations into account, 40 to 50 ∿ is the best to choose.

The reactions due to self-induction on the line should be considered. These can be diminished by opposing mutual induction to self-induction, and working with a number of conductors in parallel. To avoid these troubles it is well to work at high tensions, by which means the loss may be reduced to an insignificant percentage. The capacity of overhead lines is not of great importance, and is compensated by the lag due to the motors.

At Frankfort it was observed that the current decreased on the station ammeter when the service line was connected to a 100-H.P. motor running light.

The author hopes to publish the results of observations on the behaviour of underground conductors.

H. PELLAT—MEASUREMENT OF THE FORCE WHICH ACTS ON FLUID UNELECTRIFIED DIELECTRICS IN ELECTRIC FIELDS.

(*Beiblätter, Vol.* 21, *No.* 4, *p.* 351.)

The measurement of the force is effected by measuring the difference of the levels in two vessels, of which one is subjected to the action of the field, and the other is outside the field. The observed values agree well with those deduced theoretically.

F. POCKELS—ON THE OPTICAL PHENOMENA PRODUCED IN AN ELECTRIC FIELD DUE TO THE VARIATION OF THE SPECIFIC INDUCTIVE CAPACITY WITH THE FIELD STRENGTH.

(Beiblätter, Vol. 21, *No.* 4, *p.* 352.)

The measurements of specific inductive capacities heretofore obtained, do not allow it to be determined whether the usually accepted relation between the dielectric polarisation and the electric field strength holds exactly throughout the widest attainable limits of the latter. If this is not the case, according to the electro-magnetic theory, certain double-refraction phenomena should be produced in the electric field, these phenomena being discussed in the present paper. The simplest case of a straight-line law of variation of the specific inductive capacity with the field strength can only occur in crystalline media without a centre of symmetry. It causes an optical action of the electric field which is actually observed in such crystals (sodium chlorate, quartz, Rochelle salt); still, for the constants relating to any optical action, certain quantitative relations must hold, which, with quartz—the only one heretofore tested—do not appear to be fulfilled. In media with centrically symmetrical structure, especially therefore with isotropic bodies, certain variations of the specific inductive capacity, proportional to the square of the field strength, are observed. From this it follows that, in such isotropic media, double refraction must take place in the electric field, such as Kerr actually observed with a number of liquids. The quantitative relation which is thus obtained between the variations of velocity of the two waves, does not, however, agree with the results of Kerr's later observations, which, according to the author, are not quite established. It is therefore improbable that double-refraction phenomena in electric fields can be explained entirely by the variation of the specific inductive capacity with the field strength; and, from the electro-magnetic theory of light, this variation, if it exists, can only be extremely small.

A. KLEINER—ON CONDENSERS.

(Beiblätter, Vol. 21, *No.* 4, *p.* 353.)

Condensers without hysteresis and air can be very easily made from paraffin. On a base of paraffin (melting point 76°) 14 grooves are cut at distances of 2 mm. apart, and in these are placed 14 coaxial cylinders of thin sheet copper of about 18 cm. height. The alternate cylinders are connected by wires. The system is then immersed in melted paraffin (melting point 42°), the latter being filtered and exhausted. The setting of this paraffin must take place slowly from below upwards. A condenser of this kind has a capacity of about 0·003 microfarad.

KLEINER and SEILER—ON THE PROCESS OF CHARGING CONDENSERS.

(Beiblätter, Vol. 21, *No.* 4, *p.* 353.)

Similarly to the discharge, the charge of a condenser proceeds in an oscillatory manner. Seiler has followed the action exactly by means of a Helmholtz pendulum.

A. CHASSY—ON SOME ELECTRO-CAPILLARY EXPERIMENTS.

(*Beiblätter, Vol.* 21, *No.* 4, *p.* 355.)

A basin contains a layer of Hg as cathode, and above this dilute acid with a positive electrode of platinum; through the acid, in any convenient position, a glass tube is dipped slightly into the Hg: On passing a sufficiently strong current, the liquid overflowed from the basin between the Hg and the wall of the glass tube. It rose here to such a degree that its pressure was sufficient to drive the Hg out of the glass tube. If the latter be bent into the form of a siphon, and the level of the liquid in the basin be maintained constant, continuous "filtration" can be effected; in an experiment with a tube of 4 cm. diameter 700 c.c. of liquid passed over in an hour. The author attributes the phenomenon to the tangential force which, according to Lippmann, exists between places of different surface tension on Hg; here it drives the liquid into the tube, where the surface tension is greater on account of the vanishingly small polarisation.

G. NANNES—THE CHARGING OF OBJECTS BY MEANS OF THE "x" RAYS.

(*Beiblätter, Vol.* 21, *No.* 4, *p.* 365.)

The author finds that if a zinc plate be charged with negative electricity and connected to the needle of a quadrant electrometer, and then subjected to the action of the x rays, the needle will not only return to zero, but will be deflected on the other side. The action of the x rays was found to be inversely proportional to the square of the distance between the plate and the vacuum tube.

W PEUKERT—ON THE INFLUENCE OF THE RATE OF DISCHARGE ON THE CAPACITY OF LEAD ACCUMULATORS.

(*Elektrotechnische Zeitschrift*, 1897, *No.* 20, *p.* 287.)

As is known, the capacity of an accumulator increases as the discharge current is reduced, so that higher capacities can be obtained if the discharge be allowed to take place with weaker currents than the highest allowable In discharging with weaker currents, and consequently with lower current-densities, the active material takes part in the chemical changes, in greater quantity than is possible with higher current-densities.

The author has carried out a number of experiments to determine the relation between the capacity and the strength of the discharge current. From these experiments it was found that this relation was of the form $J^n \times t =$ a constant, where $J =$ the discharge current, and $t =$ the time of discharge in hours. The mean value of the exponent n was found to be 1·47 for the Correns accumulator, type No. 3. The values obtained for other types of accumulator experimented upon are given in the following table, viz. :—

System.	Type.	Value of n.
Tudor	E	1·35
,, ...	E S	1·48
Pollak	S K	1·36
,, ...	R	1·51
Correns...	H	1·72
,, ...	Q	1·64
G. Hagen	A	1·39
,, ...	B	1·39
De Khotinsky	N	1·55
,, ...	X	1·55
Gülcher	A	1·38
,, ...	C & E	1·36

From the above equation the capacity of an accumulator for any desired discharge current can be calculated if the capacity for a definite current be given. Let K be the capacity for the discharge current J and the time of discharge t, and K_1 the capacity for the discharge current J_1 during the time t_1 : then the following relation holds, viz. :—

$$J_1^n\, t_1 = J^n\, t;$$

or, since

$$K = Jt, \text{ and } K_1 = J_1 t_1,$$

$$K_1\, J_1^{n-1} = K\, .\, J^{n-1};$$

and, therefore,

$$K_1 = K\left(\frac{J}{J_1}\right)^{n-1}.$$

A. EBELING and E. SCHMIDT—ON THE MAGNETIC PROPERTIES OF RECENT KINDS OF IRON, AND THE STEINMETZ CO-EFFICIENTS OF MAGNETIC HYSTERESIS.

(*Elektrotechnische Zeitschrift*, 1897, *No.* 19, *p.* 276.)

The following are results obtained in the Physikalisch-Technischen Reichsanstalt. It may be noted therefrom that cast steel of very high quality, from the magnetic point of view, can now be obtained.

Out of 45 cast samples,

24	per cent.	had a coercitive force	of	1·5–2·0
44	,,	,,	,,	2·1–2·5
13	,,	,,	,,	2·6–3·0
18	,,	,,	,,	3·1–5·3

In the following tables, B_{max} = the highest observed induction, B, for the corresponding strength of field, H_{max}; B_{100} the value of B for H = 100; C the coercitive force; $E = \frac{1}{4\pi}\int B\, d\, H$, the loss of energy per cycle due to hysteresis; $\eta = \frac{E}{B_{max}^{1\cdot6}}$, the Steinmetz coefficient of magnetic hysteresis; μ_{max} = the maximum value of the permeability observed with the field strength, $H\mu$.

Table I.

Material.	B_{max}	H_{max}	B_{100}	C	E	η	μ_{max}	H_{μ}
Swedish wrought iron ...	17,990	134	17,400	0·8	6,300	0·0010	4,200	1·3
,, ,, ...	18,020	141	17,300	0·9	7,500	0·0012	3,700	1·3
Cast steel	18,020	144	17,300	1·5	11,100	0·0017	2,550	2·3
,, ...	18,080	139	17,500	1·7	13,600	0·0021	2,590	2·7
,, ...	18,040	133	17,450	1·9	15,900	0·0025	1,860	2·9
,, ...	18,000	123	17,500	2·1	18,900	0·0029	1,540	3·6
Cast Siemens-Martin steel	17,650	124	17,200	1·7	16,400	0·0026	1,900	2·
,, ,,	18,030	140	17,350	1·8	14,500	0·0023	2,150	2·7
,, ,,	18,030	131	17,530	1·8	12,400	0·0019	2,390	2·8
,, ,,	17,660	130	17,140	1·9	17,500	0·0028	1,690	2·8
,, ,,	18,180	142	17,480	1·9	15,800	0·0024	2,080	2·7
,, ,,	17,920	131	17,430	2·0	13,500	0·0021	2,170	2·5
Cast ingot iron	17,650	121	17,280	1·5	12,900	0·0021	...	...
,, ,, ...	18,230	141	17,540	2·0	14,300	0·0023	2,100	3·3
,, ,, ...	17,760	121	17,400	2·1	16,500	0·0026	...	...
Cast steel	17,960	141	17,260	2·5	20,000	0·0031	1,700	3 5
,, ...	17,950	139	17,280	5·3	34,700	0·0054	900	8·3

Experiments were made on the effect of annealing, the results of which are as follows, viz. :—

Material.	Condition.	B_{max}	H_{max}	B_{100}	C	E	η
Swedish cast steel	unannealed	17,900	135	17,300	2·5	18,200	0·0029
,, ,,	annealed ...	18,080	126	17,600	1·0	9,750	0·0015
German ,,	unannealed	17,780	130	17,240	2·3	21,000	0·0033
,, ,,	annealed ...	18,430	162	17,440	1·2	11,200	0·0017

The authors also investigated the variations in the value of the coefficient η with different values of B. This coefficient was found to increase with the induction until a certain point was reached, viz., 16,000 to 18,000 C.G.S. units, and then to decrease slightly, the variations ranging from 3·6 per cent. to 43 per cent. in different samples.

CLASSIFIED LIST OF ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM

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S. denotes a series of articles. I. denotes fully illustrated.

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S. Hanappe—The Three-Phase Installation of the Laboratory of the Special School of Mons.—*Ecl. El.*, vol. 11, No. 19, May, 1897, p. 256 (S. I.).

G. Richard—Mechanical Applications of Electricity. The Aspinal Luggage Elevator. The Herdman Lift. The Parkinson Lift. The Aiken Crane. The Wellmann Furnace Trolley. The Richards Weighing Machine.—*Ibid.*, No. 21, p. 343 (I.).

J. L. Rontin—The Distribution of Electrical Energy at Rheinfelden.—*Ibid.*, No. 22, p. 390 (I.).

F. Guilbert—The Tests of a Series of American Transformers.—*Ibid.*, No. 22, p. 405.

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NOTICE.

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2. The Library is open (except from the 14th August to the 16th September) daily between the hours of 11.0 a.m. and 8.0 p.m., except on Thursdays, and on Saturdays, when it closes at 2.0 p.m.

An Index, compiled by the late Librarian, to the first ten volumes of the Journal (years 1872–81), and an Index, compiled under the direction of the Secretary, to the second ten volumes (years 1882–91), can be had on application to the Secretary, or to Messrs. E. and F. N. Spon, 125, Strand, W.C. Price Two Shillings and Sixpence each.

INSTITUTION OF ELECTRICAL ENGINEERS.

GENERAL RULES FOR WIRING FOR THE SUPPLY OF ELECTRICAL ENERGY.

These rules embody the chief precautions and requirements which the Institution considers necessary to secure satisfactory results.

They have been drawn up to meet the ordinary cases of dwelling-houses, offices, or business premises in which it is desired to lay the conductors, and fix the fittings and appliances necessary for utilising electrical energy either for lighting, for heating, for motive power, or for other purposes.

They are arranged in such a form that they may be used as a specification of requirements and precautions which must be strictly enforced if a user of electrical energy wishes to have his house or premises supplied in such a manner that he may be as free as possible from risk of fire, of extinction, failure of supply, or danger to person, and at the same time have his work carried out with due regard for economy both in first cost and in after cost of maintenance.

The rules are framed to meet all ordinary cases, but they are not intended to take the place of detailed specifications drawn up by consulting engineers to meet individual requirements.

They are confined to a statement of well-ascertained requirements, and do not recommend any special system or form of apparatus by which these may be best fulfilled.

For convenience the rules are grouped as below.

Conductors.	1.	Conductivity and size.
	2.	Insulation.
	3.	Joints.
	4.	General arrangement.
	5.	Precautions where they pass through walls or partitions.
	6.	Do. at points of connection.
Fittings.	7.	Do. as to switches, fuses, and other appliances.
	8.	Switches.
	9.	Switch-boards.
	10.	Fuse boxes and fuses.
Generating and Utilising Appliances.	11.	Dynamos and motors.
	12.	Accumulators or other batteries.
	13.	Transformers.
	14.	Arc lamps.
Testing.	15.	Testing the whole and parts.

CONDUCTORS—CONDUCTIVITY AND SIZE.

1. They should be of high-conductivity copper, not less than 100 per cent. conductivity, and, where sulphur or other substance liable to attack bare copper is contained in the insulation, they should be tinned with pure tin.

NOTE.—The standard of conductivity here referred to is, that the resistance of a copper wire weighing 100 grains, 100 inches long, should be 0·1516 ohm at 60° Fahr.

Sectional Area. Their sectional area should be proportional to the heating effect of the current required for the maximum number of lamps, or other current-using apparatus, that can be used simultaneously on the circuit; but in no case should the sectional area of any conductor be less than that of a No. 18 S W.G. wire. All conductors having a sectional area larger than that of a No. 14 S.W.G. wire should be stranded.

Temperature Limits. They should be of such size that, when the maximum current is passing continuously through them, their temperature shall not exceed 130 degrees Fahr. It will, however, generally be found

that if the conductors are worked up to a density of current corresponding to this increase of temperature, the resulting fall of potential or drop in volts will be inconvenient and uneconomical. It is imperative that this temperature of 130 degrees Fahr. should never be exceeded, and therefore it is necessary to take into account the maximum temperature to which they may be subjected, independently of electric heating, in each particular locality, and the greatest increment above this temperature should not be more than will raise them to a temperature of 130 degrees Fahr.

If the maximum temperature of the British Islands be taken as 100 degrees Fahr., then the increment due to electric heating must not exceed 30 degrees Fahr.; that is to say, the size of the wires should be such that, when carrying the maximum current continuously for many hours, the temperature does not rise more than 30 degrees Fahr. above the temperature, for the time being, of the place in which they are situated. In specially hot places the wires should be so large that the electric heating should be almost nil, and the wires should be specially insulated with insulating material which does not deteriorate at the highest temperature to which it will be subjected.

The Table appended shows size of conductors which will safely carry currents up to 740 amperes, and the length in yards of single conductor in circuit for each volt of fall of potential when the maximum current is in use.

INSULATION.

2. Insulated conductors may be broadly classed under two heads— *Insulation.*

A. Those insulated with a material as a dielectric which is itself so impervious to moisture that it only needs further protection from mechanical injury or from vermin.

B Those insulated with a material as a dielectric which, in order to preserve its insulation qualities, must be kept perfectly dry, and therefore needs to be encased in a water-proof tube or envelope, generally of soft metal, such as lead, which is drawn closely over the dielectric.

When class A is used, the dielectric must be perfectly damp-proof, and not in any case less in thickness, measured radially, than 30 mils plus 1-10th of the diameter of the conductor; it should not soften at a lower temperature than 170 degrees Fahr.; the minimum insulation of a test piece cut from it should be that given in column 7 of the Table, the test being made at 60 degrees Fahr. after one minute's electrification, and after the test piece has been immersed in water for 24 hours.

When class B is used, the same conditions as to minimum thickness and softening temperature of the dielectric should be enforced as in class A; its covering should be such that a test piece cut from the conductor and immersed in water will not break down when an alternating pressure of 2,500 volts having a frequency of from 40 to 100 periods per second is applied for 10 minutes between the conductor and the water, the test piece previous to immersion having been bent six times (three times in one direction and three times in the opposite direction) round a smooth cylindrical surface not more than 12 times the diameter of the conductor, measured outside the dielectric. The coil from which the test piece was cut should be tested in a similar manner to class A, but the minimum insulation resistance should be that given in the Table, column 8.

Conductors of class A must be protected from mechanical injury by being covered with stout braid or taping, prepared so as to resist moisture, and must be further protected by casing, or by being drawn into pipes or conduits.

In the case of conductors insulated as in class B great care must be taken to protect exposed ends of conductors where they enter the terminals of switches, fuses, and other appliances, from the possible access of moisture which might creep along the insulating material within the water-proof covering.

Concentric Conductors.

Concentric conductors should in all respects conform to the requirements herein laid down for single conductors; the insulation resistance of the outer dielectric should be that given in the table for single conductors having the same diameter as the outer conductor. The insulation resistance of the dielectric separating the two conductors should be twice that of the outer dielectric.

The bending test of concentric conductors, class B, should be made round a cylinder 12 times the diameter of the outer dielectric.

Flexible Cord Conductors.

Flexible cord conductors—*i.e.*, those made up of a number of wires not larger than No. 29 S.W.G., which are then insulated (in many cases two such conductors are twisted together so as to form a double conductor)—should only be used for attachment to portable appliances, or for the wiring of fittings; the insulating material used as the dielectric should be either pure rubber or vulcanised rubber of the best quality. If pure rubber be used, it should be laid on in two laps, care being taken that these should lap-joint. The radial thickness of the dielectric should never be less than 16 mils for pressures up to 125, or 20 mils for pressures up to 250 volts. Each coil should bear a certificate that a piece one yard in length cut from it has withstood for five minutes an alternating pressure of 1,000 volts having a frequency of from 40 to 100 periods per second applied between the two conductors twisted together, the piece being subjected during the test to the vapour arising from a pan of boiling water placed at a distance not exceeding 3 feet, and immediately below it.

JOINTS.

Soldering Fluids Prohibited.

3. All joints in conductors must be mechanically and electrically perfect, to prevent heat being generated at these points. The use of soldering fluids containing hydrochloric acid, sal ammoniac, or other corrosive substances, should be absolutely forbidden. The insulation of all joints in insulated conductors should be most carefully attended to, the object being to make the insulation of the joints as nearly as possible equal to the insulation of the remainder of the conductor.

Jointing Precautions.

In jointing rubber-insulated cable, care should be taken that the braiding or taping is carefully removed without damage to the india-rubber, which latter should be laid bare, and tapered for sufficient length to ensure a water-tight union with the insulating substance used as a covering. It should be remembered when arranging for any system of wiring that joints constitute a

source of weakness, and they should, therefore, be avoided as far as possible.

General Arrangement.

Distributing Centres.

4. The arrangement of conductors should be carried out as far as possible from distributing centres, the cable conveying the current to them being free from joints; from these centres of distribution the use of small circuits carrying up to 5 amperes, and also free from joints, except at the branches and connections to switches and other appliances, is recommended, in order that the fuses at these centres of distribution may amply protect every conductor beyond them, even if only a "flexible" for a single lamp.

This will ensure safety, although the ideal system is to carry a conductor from each point of use back to the distributing centre without joint or tapping.

Draw-in Systems.

The use of a draw-in system in mhich both conductors are drawn into one strong incombustible tube or chamber, or their equivalent, is preferable to wood casing with spaced conductors, as safety is better obtained by the use of suitable insulation of the wires themselves than by trusting to the wood casing, or to the spacing for insulating purposes. The composition of the tubing or conduit used must depend on the character of the structure in which it is embedded; tubes or conduits which minimise condensation or sweating are to be preferred. When tubes are used no elbows should be employed, but corners should be turned either by means of slow bends or by the fixing of a suitable box.

Conductors Spaced from Walls.

Conductors spaced and separated away from the walls should not be permitted unless they are mechanically protected throughout their entire length. Where the distribution is effected by circuits not carrying more than 5 amperes, conductors of the same polarity may be "bunched" together, providing a double-pole fuse, arranged to sever the circuit before any perceptible rise of temperature can take place, is inserted at the point of distribution; conductors of opposite polarity may also be "bunched," provided that they are placed in an incombustible tube or conduit.

Precautions where Conductors pass through Walls or Partitions.

5. Cables or wires passing through walls require additional protection, such as a porcelain or other tube which can be filled up with sand or other chemically inert incombustible material, so as to prevent the spread of fire through these openings. Wherever conductors cannot be in sight they should be made as accessible as possible; and it is recommended that wires which must be buried within walls should not be fixed, but drawn into channels previously prepared for them, and they should preferably not be drawn in until any dampness which may exist in these channels has dried out of them. *Porcelain Tubes in Walls.* *Conductors drawn into Channels.*

Conductors should not be placed near gas pipes. *Gas Pipes.*

Precautions at Points of Connection.

6. Wherever conductors are connected on to switches, fuses, or other appliances, great care must be taken that the whole of the separate wires forming the stranded or flexible conductor are neatly twisted together and clamped into the terminal, so that no loose wire or strand can project; the insulating material or dielectric should only be bared back sufficiently to allow of the conductor entering into the terminals properly, and the ends of the insulation should be thoroughly sealed, to prevent moisture creeping along the copper beneath the insulation. *Jointing on to Switches.*

The braiding, lead, or other non- or semi-insulating material, should be cut back for a distance of not less than ¾ inch from the end of the insulating material.

Precautions as to Switches, Fuses, Connectors, and other Appliances.

7. These should be mounted on bases made of porcelain or other non-combustible material. If any difficulty arises through damp, this may be overcome by inserting a second base or backing of specially prepared material. *Bases to Switches, &c.*

In excessively damp places, such as cellars, all fittings attached to walls should, as far as possible, be dispensed with, the wires being carried direct from the distributing board to the lamps. *Damp Places.*

Frames for Resistances. Resistance coils should in all cases be carried on frames or supports made of incombustible material, and preferably should be enclosed in metal cases, to prevent accidental derangement.

Wiring of Fittings. Wherever fittings, such as brackets, electroliers, or standards, require to have the conductors threaded through tubes or channels formed in the metal work, these should be designed so as to avoid sharp angles or rough projecting edges which would be liable to strip or cut or damage the insulating material in the act of drawing in the conductors, or in fastening them to the outside in the case of adapted fittings. *Adapting Gas Fittings.* The use of combined gas and electric fittings should not be permitted; where gas fittings are adapted, they should be insulated from the gas pipe.

Jointless Conductors. Where possible, the conductor should be carried without joint through the fitting to the lamps; but where connections at the back are unavoidable, special care must be taken to make this joint equal in quality, as regards conductivity and insulation, to the rest of the work.

Switches.

8. Every switch, whether fixed separately or combined with lamp holders or fittings, should be constructed to comply with the following requirements:—

(*a*) That no overheating can take place at the point of contact or elsewhere.

(*b*) That when being switched off it is impossible for a permanent arc to be formed.

(*c*) That it cannot be left in an intermediate position between on and off.

(*d*) The base should be of incombustible material.

(*e*) The cover should also be of incombustible material, and should preferably be either made of or lined with non-conducting material.

(*f*) Covers of all switches should be kept clear of all the internal mechanism.

(*g*) The handles of all switches should be efficiently insulated from the circuit.

(*h*) In order to ascertain that switches comply with the above requirements, samples should be selected from each pattern and size used, and should be tested at an E.M.F. and current 50 per cent. in excess of that which will be used on the circuits for which they are intended.

Main switches should be placed close to the generators if the supply is generated within the building, or at the transformer if transformed within the building, or at the point of entrance of the conductors into any building supplied from an external source.

When all three wires of a three-wire system are brought into a house, the member of the switch connected to the middle wire must not make contact later, or break contact sooner, than the other two members; preferably the middle member should make contact *sooner* and break contact *later* than the two outer members. Single-pole switches should not be on the middle wire of a three-wire system. In a five-wire system the same principles will apply.

Switch-Boards.

9. Wherever main or centres of distribution switch-boards are provided, these should be constructed of incombustible material, preferably with front connections, with circuits arranged as far as possible to form their own diagram of connections, and so labelled that they may be easily identified. Where back connections are permitted, they should be carefully soldered. Exposed metal work of different polarity on switch-boards should be well separated, and preferably mounted on separate bases.

Diagram of Connections.

Exposed Metal Work.

Fuse Boxes and Fuses.

10. Branches from all circuits should have fuse boxes made of porcelain or other incombustible material on both poles, and the fuses in these fuse boxes, if on the same base, should be in separate compartments. Where the tree, or tapered, system of wiring is allowed, fuses should be introduced at such intervals that each fuse protects the smallest branch between it and the next fuse; or, if

Fuses on both Poles.

Fuses on Tapered Mains.

there is no other fuse, then it must protect right up to the end of the circuit. If the above precautions are taken, it is not necessary to protect the ceiling roses which support flexible pendants, by fuses at the ceiling point of junction.

Fuses in Ceiling Roses.

Fuses in Portable Fittings.

Whenever circuits not exceeding 5 amperes have fuses in each pole at the distributing point, fuses in the connectors (see Section 7) are not necessary; should the current, however, exceed 5 amperes up to 125 volts, or 3 amperes up to 250 volts, all portable fittings requiring flexible cords, or adapted fittings wired with flexible cords, must be protected with a fuse at the point of junction with the circuit.

Any fitting containing many lights and wired with flexible cord should be supplied by conductors carried back to the distributing centre.

Where one of the conductors is connected to earth, all switches and fuses which will be single-pole should be arranged on the insulated side of the system.

No fuses or switches should be placed in or at any point of the earthed conductor.

Standard types of fuses should be so designed as to avoid the risk of inserting fuses intended for large circuits into the fuse carriers of small circuits, and *vice versâ.*

Ventilation of Fuse Boxes.

The covers of all fuse boxes—whether these be separate or grouped on switch-boards—should be efficiently ventilated, so as to avoid risk of fracture by the sudden expansion of the air within them at the time the fuse melts, the covers being arranged to catch and retain the fused metal.

All connectors should be capable of withstanding a test at an E.M.F. and current 50 per cent. in excess of that for which they are intended. If used in damp places special precautions must be adopted to exclude moisture. In cases where the fixed part of the connector is attached to a floor it must be so arranged that no dust or water can accumulate in the cavity, and should have all contacts well below the floor level, to prevent any possibility of danger from contact with the carpets.

Connectors in Floors.

Concentric Connectors.

When concentric connectors are used they must be constructed so that they cannot be readily short-circuited by a piece of metal,

such as a pin or a metal pencil-case. Clearances should be such that an arc cannot be started if the connector is pulled out at the time that the current is flowing. The insulation used between opposite poles should be such that it will not readily break or chip.

DYNAMOS AND MOTORS.

11. Dynamos and motors should be protected from damp and dust, and should be so placed that no woodwork or inflammable material is within a distance of 12 inches from them measured horizontally, or within 4 feet from them measured vertically above them; and the same precautions must be adopted in placing and fixing the starting switches or regulating resistances used in connection with any of these appliances. The coils of these resistances must be so designed that in no case do they heat above 212 degrees Fahr. even if left continuously in use; and the coils must be protected by suitable metal casing or guards, which must not interfere with free circulation of the air round the coils. *Spacing of Dynamos, &c., away from Woodwork.* *Spacing of Resistance Coils.*

The frames of dynamos or motors employing an E.M.F. of 250 volts or upwards should be connected to earth. *Earthing the Frames.*

Continuous-current transformers are to be classed with dynamos and motors.

ACCUMULATORS OR OTHER BATTERIES.

12. Both accumulators and primary batteries should be placed and used under the same precautions as above described for dynamos and motors, and the room in which they are placed should be well ventilated. The accumulators and batteries should themselves be well insulated from the earth, and should be protected by fuses at both poles, and at all points of connection between the circuit and the regulating cells.

TRANSFORMERS.

13. When these are used to transform either direct or alternating currents of high E.M.F. down to the E.M.F. allowed by the Board of Trade on the consumer's premises, they, together with their switches and fuse boxes, must be placed in a fire- and water-proof structure, preferably outside the building for which they are required, and their frames must be connected to earth.

No part of such apparatus should be accessible except to the person in charge of them. In all cases conductors conveying currents of high E.M.F. inside a building must be specially insulated and encased in a fire-proof conduit. Under no circumstances should transformers be allowed to heat under normal conditions of load to a temperature of 150 degrees Fahr. Transformers should be so protected by suitable apparatus that a leak between the primary and secondary coils raising the pressure to 400 volts above that of the earth should cut the transformer out of circuit.

Low-Pressure Transformers. Low-pressure alternating transformers or choking coils may be placed within buildings, but the same precautions as regards heating of the coils, distance from woodwork, and guarding must be adopted as in the case of resistances used for motors.

Arc Lamps.

14. Arc lamps must always be guarded by lanterns or netted globes, so as to prevent danger from ascending or descending sparks, and from falling glass or incandescent pieces of carbon. All parts of the lamps which are liable to be handled should be well insulated, and, in addition, an insulator must be inserted between the lamp and its support. Resistances for arc lamps should have a similar double insulation; their coils should be designed so as not to heat above 212 degrees Fahr.; they should be protected by metallic ventilating guards, and should be so placed that no woodwork is within 6 inches of them measured horizontally, or within 2 feet of them measured vertically above them. When arc lamps are supplied from constant potential mains, fuses on both mains are necessary.

Arc lamps in which air can have access to the carbons during burning should on no account be used in places where inflammable vapours or explosive mixtures of dust or gas are liable to be present.

Testing.

Insulation Test. **15.** The conductors, fittings, and appliances must be tested in the following manner before the current is switched on:—The

whole of the lamps or appliances for utilising the energy having been connected to the conductors, and all fuses being in place, an E.M.F. equal to twice the E.M.F. which will be ordinarily used is to be applied, and the insulation resistance between the whole system and earth must be measured after one minute's electrification. The insulation should then not be less than 10 megohms, divided by the maximum number of amperes required for the lamps and other appliances. The installation may be then set to work, and a second and similar test should be made after an interval of 15 days. In each test, if the insulation of the whole is below standard, the work should be divided up by the departmental switches and tested separately, in order to locate the faulty section.

The value of systematically testing and inspecting apparatus and circuits cannot be too strongly urged as a precaution against fire. Records should be kept of all tests, so that any gradual deterioration of the system may be detected. Cleanliness of all parts of the apparatus and fittings is essential. No repairs or alterations should be made when the current is "on."

Explanation of Table.

Column 1 gives the sizes of the conductors in common use. Cables are shown thus:—19/14, viz., 19 wires of No. 14 Standard wire gauge.

Column 2 gives the maximum current for situations where the external temperature is above 100 degrees Fahr.

The current for any conductor may be calculated from the formula—

$$\text{Log } C = 0{\cdot}775 \log A + 0{\cdot}301,$$
$$\text{or } C = 2\,A^{0{\cdot}775}$$

(where C = current in amperes, A = area in 1,000ths of a sq. in.).

The maximum rise in temperature will be about 10 degrees Fahr. on large sizes.

Column 3 gives the total length in yards of lead and return of each size of conductor causing a drop of 1 volt when transmitting the current shown in column 2.

Column 4 gives the maximum current allowable in any situation. The current for any conductor may be calculated from the formula—

$$\text{Log } C = 0{\cdot}82 \log A + 0{\cdot}415,$$
$$\text{or } C = 2{\cdot}6\, A^{0{\cdot}82}$$

(where C = current in amperes, A = area in 1,000ths of a sq. in.).

The maximum rise in temperature will be about 20 degrees Fahr. on large sizes.

Column 5 gives the total length in yards of lead and return of each size of conductor causing a drop of 1 volt when transmitting the current shown in column 4.

Column 6 gives the minimum thickness of dielectric. This may be obtained for any conductor by adding 30 mils to 1-10th the diameter of the conductor.

Columns 7 and 8 give the insulation resistances in megohms for one mile of cable of classes A and B respectively.

By Order of the Council.

F. H. WEBB, *Secretary.*

Offices of the Institution,
28, Victoria Street, Westminster,
July, 1897.

SHOWING M... AND INSULATION RESISTANCE FOR COPPER ... CASING OR TUBING.

	5.	6.	7.	8.
Si...	Total Length in Yards of Lead and Return giving 1 Volt Drop.	Minimum Thickness of Dielectric in Mils or Thousandths of an inch.	Minimum Insulation Resistance in Megohms for One Mile of Class A.	Minimum Insulation Resistance in Megohms for One Mile of Class B.
18 or 62/38	18	35	1,200	300
3/22	17	36	,,	,,
17 or 130/4...	19	36	,,	,,
3/20	19	38	,,	,,
16 or 110/3...	19	36	,,	,,
15	20	37	800	,,
7/22	20	38	,,	,,
14 or 172/3...	21	38	,,	,,
3/18	20	40	600	,,
7/20	22	41	,,	,,
7/18	25	44	,,	,,
19/20	26	48	,,	,,
7/16	27	49	,,	,,
19/18	29	54	,,	,,
7/14	29	54	400	,,
19/16	32	62	,,	,,
19/14	35	70	,,	,,
37/16	37	75	,,	,,
19/12	39	82	300	,,
37/14	40	86	,,	,,
61/15	42	95	,,	,,
61/14	43	102	,,	,,
37/12	44	103	,,	,,
61/12	47	124	,,	,,
91/12	51	144	,,	,,
91/11	53	158	,,	,,

THE LIBRARY.

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JOURNAL

OF THE

Institution of Electrical Engineers.

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The Three Hundred and Fifth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, November 11th, 1897—Sir HENRY MANCE, C.I.E., President, in the Chair.

The Minutes of the Ordinary General Meeting held on May 27th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

J. E. Neale.	W. A. Purdom.

From the class of Students to that of Associates—

Ernest Arthur Bayles.	C. H. Marshall.
H. H. Harrison.	A. P. O'Brien.

G. P. Roy.

Donations to the Library were announced as having been received since the last meeting from the Aluminium Supply Company, Lord Armstrong, the Astronomer-Royal, Dr. A. C. Crehore, the Director-General of Indian Government Telegraphs, Messrs. Griffin & Co., London Chamber of Commerce, Mr. J. McDonnell, Mr. W. G. McMillan, the Pittsburgh Reduction Company, Mr. D. R. Walker; Mr. Philip Dawson, Mr. E. Garcke, Mr. A. A. Campbell Swinton, Sir Charles Todd, Members; Mr. D. K. Morris, Mr. J. Nicolson, Mr. E. K. Scott, Associates; to whom the thanks of the meeting were duly accorded.

The SECRETARY read the following letter acknowledging the receipt of the address presented by the Institution to Her Majesty the Queen :—

WHITEHALL, *12th June*, 1897.

SIR,

I have had the honour to lay before the Queen the loyal and dutiful address of the President, Council, and Members of the Institution of Electrical Engineers, on the occasion of Her Majesty attaining the Sixtieth year of Her Reign, and I have to inform you that Her Majesty was pleased to receive the same very graciously.

I have the honour to be, Sir,
Your obedient Servant,
(Signed) M. W. RIDLEY.

F. H. WEBB, Esq.,
Secretary to the Institution of Electrical Engineers,
Victoria Mansions, 28, Victoria Street, London, S.W.

The PRESIDENT : I will now ask the Secretary to announce, in accordance with No. 43 of the Articles of Association, the names which have been proposed by the Council as suitable for office during the year 1898.

The SECRETARY read the list of nominations as follows :—

MEMBERS NOMINATED BY THE COUNCIL FOR OFFICE IN 1898.

As President:

For Election. JOSEPH W. SWAN, F.R.S.

As Vice-Presidents (4):

Remaining in Office.	Professor S. P. THOMPSON, D.Sc., F.R.S. Professor JOHN PERRY, D.Sc., F.R.S.
New Names.	W. E. LANGDON. JAMES SWINBURNE.

Ordinary Members of Council (15):

Remaining in Office.	S. L. BRUNTON. Professor J. A. EWING, F.R.S. W. P. J. FAWCUS. Major R. HIPPISLEY, R.E. Professor J. A. FLEMING, M.A., F.R.S. E. MANVILLE. JOHN S. RAWORTH. DANE SINCLAIR. HERBERT TAYLOR. CHARLES HENRY WORDINGHAM.
For Re-Election.	HENRY EDMUNDS.
New Names.	ROBERT KAYE GRAY. P. V. LUKE, C.I.E. W. M. MORDEY. A. A. CAMPBELL SWINTON.

Associate Members of Council (3):

Remaining in Office.	H. W. MILLER. SYDNEY MORSE.
New Name.	SYDNEY EVERSHED.

OFFICERS NOMINATED BY COUNCIL FOR 1897.

As Honorary Auditors:

For Re-Election.	F. C. DANVERS.
New Name.	E. GARCKE.

As Honorary Treasurer:

For Re-Election.	Professor W. E. AYRTON, F.R.S., Past-President.

As Honorary Solicitors:

For Re-Election.	Messrs. WILSON, BRISTOWS, & CARPMAEL.

The following Paper was then read:—

ACCUMULATOR TRACTION ON RAILS AND ORDINARY ROADS.

By L. Epstein, Member.

Mr. Epstein. While electric traction on the trolley system has proved on the whole an unqualified success, both from a technical and financial point of view, traction by means of accumulators could, until quite recently, only be pronounced a failure, and all that even its most ardent advocates can plead for is the substitution of the term "qualified success." However, at the present time, signs are not wanting that promise at last success for accumulator traction. The progress made in the manufacture of secondary batteries, and the experience gained with regard to the best mode of using them, not only warrant that belief, but, what will no doubt prove more convincing, relatively good results have already been obtained.

It will be remembered that ever since accumulators were produced on a commercial scale attempts were made from time to time to use them for traction purposes, but all these experiments up to a short time since—although frequently hailed with great enthusiasm—only led to disappointment, from a commercial point of view.

An investigation of the causes which militated against success might prove useful, and will show that they may be divided into two classes, viz., inherent defects in the accumulators themselves and mistakes in the mode of their application.

Dealing with the method of working first, it will be seen, on analysing the expenses, that, apart from the cost of repairs and renewals, a heavy expenditure was incurred by the handling of the batteries; and, in view of the success of trolley traction, it is natural that the remedy should suggest itself to imitate, as far as possible, the latter system—that is to say, to employ accumulators in such a manner that they require a minimum of attendance and handling.

It was, as we all know, the common practice to sub-divide the battery into a number of groups, each contained in a tray, and

the connection between one tray and the next, as well as at the terminals both on the cars and in the lifts for charging, was most imperfect. Sliding contacts were generally used, which were open to attack by the acid spray, necessitating almost incessant cleaning and repairs. Again, the pulling out and pushing in of these heavy trays was not always done with the requisite care, much to the detriment of the batteries, cars, and lifts. A further considerable source of waste consisted in the electrical leakage when charging, especially as many tiers of the cells were underground, and therefore out of sight, whilst they were not easily accessible, and were thoroughly saturated with acid spray. Mr. Epstein.

These difficulties and losses can be obviated by adopting a system which will allow of the batteries being treated as a mechanically and electrically well connected whole, either by being placed in the cars or preferably slung to the frame, or carried on a separate car—apart from the sub-division in groups for coupling in parallel or series during discharge. Where the conditions of working render it desirable, the motor, or motors, could also be fixed in the battery car, which would then assume the character of an electric locomotive; but in any case, whether carried in the car itself or slung to the frame, or carried on separate wheels, the battery should not be sub-divided and handled in the old way, but should always be treated as an indivisible unit. The obvious advantages gained by this method are the absence of lifts or similar contrivances, good connection between cell and cell, no corroding contacts, no loss of current through leakage, less wear and tear of the batteries and consequently easier management and reduced expenditure. The importance of obviating the exchanging of batteries has been fully recognised on the Continent, and the methods of working in Hanover and Paris offer interesting illustrations of how this object has been attained.

In Hanover, as is well known, a combined system of trolley and accumulators is in use. The batteries are charged from the trolley *en route*, and an additional charge is given to them on their return to the car shed. This installation is on a sufficiently large scale to render the financial results of commercial value, and

Mr. Epstein. it is gratifying to see from the official report that the results for the year 1896 were pronounced to be in every respect satisfactory. The cost of maintenance is said to have been accurately ascertained, and found to average per car and month 40s., which, at an average mileage of 90 per car and day, corresponds with ·177d. per car mile. The directors anticipate that this cost will be increased somewhat, but they are also confident that it will in no case exceed 60s. per car and month (which amounts to ·266d. per car mile) even in those years when the quicker deterioration of the plates will occur. It must be understood that the cost of maintenance includes renewals of plates to keep the latter always in good condition, so that the additional depreciation is reduced to a rate not higher than that of the renewal of other parts of the machinery. Based on the actual experience gained, this rate has been fixed at 6 per cent., and with an accumulator car covering between 31,000 and 34,000 miles during the year on this mixed system, accumulator traction incurs an additional expenditure of ·4d. per car mile, as compared with the trolley. Taking into account that in the absence of the accumulators the trolley system would have to be installed throughout the whole line at an outlay of £2,000 per mile of track, and further considering the maintenance of the overhead system and the saving in the wear and tear of the trolley, which is, of course, at rest while the accumulators are supplying the current, it is computed that even on the most unfavourable assumption the extra cost of the combined system compared with the overhead system alone does not in Hanover exceed ·2d. per car mile.

It may be of interest to mention here that the running expenses of the electrical system, including driver, amounted to 2·22d. per car mile.

This system, although so far satisfactory, is open to certain objections, the foremost being the dead weight of the accumulators carried on the trolley wire section. It might be found more advantageous to place the batteries in a frame slung to the car, or in dummy cars, either of which could be picked up at the end of the trolley section, while the charging of the accumulators could still be effected in the same manner from the trolley wires,

the only difference being that the charging would take place at fixed points instead of *en route*. Mr. Epstein.

A different system is employed in Paris, where the Société des Moteurs have installed and are working a line about 12 miles long at the same cost, including depreciation, as horse traction. Here the plan has been adopted of charging the batteries at the stopping places, the time of charging varying ordinarily between 8 and 10 minutes; and the charging is effected, not, as is usual, at constant current, but at constant potential, consequently very high current-densities are applied at the beginning of the charge. Six feeder circuits are projected, of which three are working at present. These circuits are underground, and, although of different lengths, are all of uniform resistance. I am not aware that any figures showing the actual cost of working are available.

It will be seen that, however different these two systems may be in detail, they have the following points in common :—

1. The batteries are dealt with as a whole.
2. The batteries are once for all installed in the cars where they are charged and discharged.
3. Each of the two systems permits the use of batteries of less weight than if the old practice had been followed of charging them either at the journey's end or at constant current.

Never losing sight of the object in view, viz., to approach in the mode of working as far as possible the overhead or conduit system, the special method to be adopted will depend upon local circumstances. While in one case the batteries may be advantageously carried in or slung to the car, in others it will be found desirable to place them in dummies, which latter might also carry the motors, as an alternative to the motors being fixed in the cars. A dummy with a battery sufficient to propel a 52-passenger car for about eight hours will weigh, complete, about 3 tons, the dummy itself, with axles and wheels, weighing about three-quarters of a ton. As each ton propelled under normal conditions incurs an expenditure of about ·4d., the extra weight of the dummy would increase the expense by about ·3d. per car mile.

Mr. Epstein. It is, however, obvious that this increase represents only a small fraction of the expenditure which is incurred when the accumulators are handled in the old manner, apart from the saving in wear and tear of the cars and of the batteries.

There are many lines on which the traffic varies considerably, not only as is usual during the different hours of the day, but perhaps in a much more marked degree, during different seasons of the year, such, for example, as tramways in market towns or seaside places. By the use of dummy cars the exigencies of a varying traffic can most effectively be dealt with, as they may be attached to one or more trailers, provided the batteries have the required qualities, and may safely be discharged at high rates.

However excellent the method of using the battery may be, it will avail but little if the battery itself be lacking in the necessary qualities, and it may safely be asserted that the success or failure of accumulator traction will be decided by the merits of the accumulator. In order to lend itself satisfactorily to traction purposes, an accumulator must, in addition to all the good points possessed by a successful lighting cell, present special features of its own. Although lightness may not be the main consideration, yet it is a very important one. Again, the cell must not only be able to withstand jolting without shedding of material and high discharge rates without buckling, but, moreover, it must not decrease in capacity while in use, as this would necessitate alterations in the time table relating to the charging of the batteries, and interfere with the general arrangement. The battery, furthermore, should not require frequent overhauling, and the repairs should be neither more numerous nor more costly than those of any other part of the machinery. We all know that in the past batteries left very much to be desired. Some of the positive plates deteriorated much sooner than others, and this led first to the practice of replacing them by less damaged plates taken from other cells, and finally to that of converting part of the negative sections into positives. After some time the original array had dwindled down to a small fraction consisting of the least injured remnants, which were occasionally referred to as a proof of the long life of the type they represented. As a battery

naturally consists of a large number of cells, and each cell again consists of a large number of plates, the overhauling and repairing proved a very serious item—in fact, in many cases the heaviest item of expenditure. Mr. Epstein.

The nearest approach to an ideal cell for traction purposes will probably be one in which the positives are of the Planté type, with a large extent of surface, the layer of active material relatively thin, but in most intimate contact with the metallic lead out of which it has been formed. As is well known, while the capacity of a plate is determined by the volume of active material, the rate of charge and discharge depends upon its surface, and as it is quite feasible by means of ribs or protrusions, without unduly interfering with the mechanical strength of the plate, to extend its area to about ten times that of the plain surface, it follows that the current-densities obtainable will be increased in the same ratio. It is further essential that the acid should have free access to all parts of the active material, and such is naturally much more the case with a Planté plate offering a largely extended surface with a thin coating of peroxide than it would be in a plate with a plain surface, where the active material forms a relatively thick layer. Not only is the contact between active and conducting parts less perfect in the pasted plate, but, in addition thereto, as the acid in the pores of the active part is liable to become exhausted, especially with heavy discharge currents, while the interstices are too narrow to allow of a quick diffusion of the electrolyte, the electric action may be seriously interfered with, although there may still be a sufficient quantity of undischarged active material.

As regards good negative electrodes, positives of the above description, after having been reduced, may be employed with advantage, or lead oxides or salts may be used, as the finely reduced lead is a much better conductor than peroxide of lead; but care should be taken to secure the best possible contact between such material and its support, and so to select the materials intended to become active that they will, after full formation, produce a layer of the highest possible porosity consistent with mechanical coherence.

Mr. Epstein. It has been found by experiment that the exhaustion of a positive plate or section of a cell effects a gradual falling in the E.M.F., while as soon as the negative electrode is exhausted the E.M.F. falls very rapidly. For this reason, and as it is obviously advantageous to have as little variation in the E.M.F. of the battery as possible, I would suggest, for traction purposes, to use negative sections of higher capacity than that of the corresponding positive sections.

In the interest of economy the formerly much-recommended overcharging must not be too freely indulged in. Such overcharging can easily be guarded against by using recording instruments, or by other suitable means, while when charging at constant potential the battery will itself guard against overfeeding by effectively opposing, at the proper time, the charging current. The recommendation to overcharge the cells *occasionally* may, however, be followed with advantage, the beneficial result consisting not so much, as was formerly supposed, in the actual improvement of the electrodes, but in the thorough mixing of the electrolyte, which is liable to vary in S.G., being densest at the bottom of the cell and decreasing towards the surface, thus leading to unequal action in different parts of the plate. Another device for equalising the action over the whole surface consists in making the electrodes taper towards the bottom, with the object of compensating for the higher density and better conductivity of the electrolyte there by increasing the distance between electrodes of opposite polarity.

We are now, however, confronted with the all-important question of the cost of depreciation of the batteries. If we consider a battery in a car for 52 passengers with an average running weight of about 12 tons and an average mileage of 100 per day, equal to 1,200 ton miles per day, we find that at the rate of 80 watt-hours per ton mile the daily electrical expenditure is 96 units, or, if working at 400 volts, 240 ampere-hours. It is, of course, immaterial for our calculation what the voltage is, as the cost of renewal of either a small number of large electrodes or a larger number of small electrodes will be practically the same for the same watt-hour capacity. The cost of renewing the positive

section of a cell of such capacity should not exceed, including manufacturer's profits, 10s., and, assuming two discharges or their equivalent a day, we may reasonably expect a useful lifetime extending over 1,500 discharges. The battery doing work, therefore, during 750 days and covering 75,000 car miles, the cost of renewing the positive sections in 220 cells will amount to £110, or ·35d. per car mile. That this estimate errs on the safe side is borne out by the experience gained in Hanover, although it seems that the batteries used there are of greater weight than is necessary. Mr. Epstein.

Given a good battery and having adopted a system best suited to the conditions of a given line, and in any case obviating the necessity for handling the batteries, we should arrive at a working cost approximately the same as on the trolley system—the extra expenditure caused by the greater weight of the self-contained car being balanced, as is shown in Hanover, by corresponding advantages gained.

While on tram lines accumulator traction must prove its superiority over rival systems in order to be adopted, there is another large field in which the use of batteries is a matter of necessity. I refer, of course, to traction on ordinary roads.

The tractive force on ordinary roads is naturally subject to much greater variations than that on rails. I think, however, that on good roads paved with asphalt or wood and in fairly good condition, the tractive force of vehicles as hitherto built should, on the level, not exceed 60 lbs. The weight of an electrical vehicle to carry from two to four passengers, with motor and battery complete, will be about 30 cwt., which includes a battery with a weight of 9 cwt. A battery of such weight ought to be sufficient to supply current for a four to five hours' run, at an average speed of 8 miles on the level and 4 miles up a gradient of 1 in 24. The power on the driving axle will be about 2 effective H.P. in the former case, and about 3 effective H.P. in the latter; and, assuming a combined efficiency of motor and gearing of 65 per cent., the battery will have to furnish discharges at the rate of 2,208 watts and 2,812 watts respectively.

With a battery efficiency of 70 per cent., the charge to

Mr Epstein. supply one hour's actual run requires about 3 units, which, at an estimated cost of 2d. per unit (a sum that should certainly not be exceeded, whether the current be generated at the company's own stations or taken from a public supply), corresponds with a cost of ¾d. per mile. It may therefore be anticipated that electric carriages plying for public hire should be able successfully to compete with horse-drawn vehicles for a similar purpose, provided that the wear and tear of the accumulators is not excessive. The favourable financial results would not be impaired even if the cost of maintaining the accumulators should exceed the rate of 10 per cent. per year, said to be quoted by some manufacturers. The time during which such accumulators have been at work is probably too short to prove whether the batteries can be maintained at such a remarkably low rate.

Reliability of the battery, obviating the necessity for frequent examinations and tests and for remedying partial defects, is again of much higher importance than—within reasonable limits—the lifetime of the whole battery, as will become evident from the following consideration. Assuming the average mileage made with one charge to be 40, and estimating the cost of renewing the positive section at 10s. per cell, or £20 per battery of 40 cells, the cost of renewal per mile run would be as follows:—

If renewed in 8 months = 240 days, after 9,600 miles, ·5d. per cab mile.

If renewed in 12 months = 360 days, after 14,400 miles, ·33d. per cab mile.

If renewed in 16 months = 480 days, after 19,200 miles, ·25d. per cab mile.

If renewed in 24 months = 720 days, after 28,800 miles, ·166d. per cab mile.

If the average cost of a battery be £60, and if it should become necessary to renew all the positive sections even after as short a time as 12 months—after having run 14,400 miles—it is true that the cost of renewals (£20) will equal 33⅓ per cent. of the first cost, but this only equals one-third of a penny per cab mile. On the other hand, even with batteries having a longer life but necessitating supervision and slight repairs, the wages

incurred and the cost of material would, judging from the experience gained in tramway work, certainly amount to a much larger sum than the cost incurred in renewing the positive sections, even in a comparatively short time. Besides, in such case more sets of batteries per car would become necessary, increasing not only the first outlay but the charge for interest and depreciation, and thus considerably swelling the total cost. Mr. Epstein.

Lightness of batteries for ordinary roads is of much more importance than it is on tram lines, not only on account of the heavier energy expenditure which is necessary to propel a given weight, but the more so as the battery will represent a larger portion of the total weight of the vehicle as compared with tram-cars. In large towns it would be an advantage to use batteries which are interchangeable, and to make arrangements with public supply companies to charge the batteries and always keep a number ready for use.

The drivers would then be able to exchange the batteries at the nearest station instead of being compelled to return to the one station, which may be a long distance off; time would thus be saved, and the earning capacity of the electric carriages increased.

The figures relating to tramways contained in this paper are the results of my own tests; but such is not the case with those having reference to traction on ordinary roads. I should have preferred to submit data, based on personal observations on a motor-car which I am making, and which I think embodies several improvements tending to reduce the necessary tractive force. But, unfortunately, as is often the case with new constructions, the completion of that car has taken longer than anticipated, and, although I endeavoured to postpone my paper until I should be in a position to bring something which might have the charm of novelty before this Institution, yet, wisely or otherwise, I gave way to the request to read a paper on electric traction at the beginning of the session. If—as I fear—I have failed to bring anything very original or interesting before you, this must be my excuse.

Mr. Manby. Mr. E. MANBY: I rise with some diffidence, as I have not the honour of belonging to your Institution, and I have to admit that I always feel somewhat overawed when dealing with volts and amperes. As a civil engineer I am more familiar with their poor relations, the good old-fashioned pressures and currents, which are not above travelling even inside a cast-iron pipe at a respectable rate of speed, and with less mystery surrounding their movements.

However, in connection with the subject now under discussion, I am able to lay before you a few figures which may prove of some interest. They refer to a new accumulator which has been used during the last few months on the Dresden tramways with considerable success. Perhaps you may have heard of it: it is called the Marschner accumulator. The patent consists in the paste, which is composed of the ordinary salts of lead, incorporated with powdered amber and an essential oil. This mixture acquires a wonderful degree of hardness when formed into plates, which are only supported at the edges like a whole metal plate. These plates have been subjected to very severe tests in Dresden, and have shown very large electrical capacity; and have never cracked or broken up under the severest treatment. In the cells which are used on the Dresden tramways the plates are 11 centimetres by 27. There are 13 of those plates to each cell, and they weigh 13½ kilos., so that each plate, positive or negative, weighs about a kilo. With this small surface they have given a very high efficiency, working at a normal rate of discharge of 65 amperes, and on emergency have risen to 120 amperes, without the slightest deformation of the electrodes, or without the slightest injury to the battery. They have run the Dresden tramway cars for a whole working day of 15 to 17 hours, over a distance of 130 miles, without recharging. In one case an accident happened. The whole of the force of the town current, 500 volts, was, through the mistake of the foreman who was superintending the charging, led suddenly through the cells, causing an explosion, which set fire to the car. When the fire was put out, two of the cells were removed, connections were made, and the car was taken out and ran several hours, which is a test that perhaps very few other

batteries would stand. After six or seven months' trial the Dresden Tramway Company are so satisfied with the results of this accumulator that they have decided to fit the whole of their cars with it, and I believe in a very short time this will have been done. The maximum discharge per kilo. of plate is nearly 9 amperes. The normal discharge is 1 ampere for about 8½ square inches of positive plate, and the maximum discharge 1 ampere per 4·6 square inches of positive plate, which I think may probably be considered to have exceeded in surface efficiency anything that could be claimed for other batteries. I have thought it worth while to bring these few facts and figures before you, because the results obtained from those batteries have made a considerable sensation on the Continent. They are being adopted in Belgium, and they will probably be adopted very shortly in France. Experiments will be made within a few days in Ostend and Ghent, and, of course, results are expected from them as satisfactory as those which have been observed during the last six or seven months in Dresden. Mr. Manby.

Mr. R. E. CROMPTON: Although I have had considerable experience in the use of accumulators, this has not extended to traction work, hence I am greatly interested in the present paper. I think it is quite possible that we are on the verge of an extended use of accumulators for traction purposes both on rails as well as on the roadway. In the latter case we are enormously indebted to Mr. Manville and those who have worked with him in carrying out the Electrical Cab Company. We electrical engineers ought to be greatly interested in the success of this company, as the experience they must daily obtain will enable them to answer many of the questions which can only be answered by experience of practical working, which they are now having. My own knowledge of the working of accumulators leads me to believe that the great difficulty will be in enabling the plates to retain the active material, as the jar and vibration of traffic on rails, and to a still greater extent on traffic over heavy roads, will greatly aid the naturally powerful disintegrating action of the charge and discharge. We know that this disintegrating action has been kept within limits by steady improvement Mr. Crompton.

Mr. Crompton.

in the methods of manufacture and formation of the plates used for stationary accumulators, but whether these improvements will of themselves be sufficient to make the same plates suitable for traction purposes only experience will tell us. I may here mention that an American gentleman showed me in my office an improvement in accumulator plates which is likely to be useful for traction purposes. He showed me plates which hold up the active material against the lead backing by soft rubber supports. The novelty appeared to me to be in the use of soft elastic rubber in place of the hard vulcanite or celluloid which have been heretofore used or experimented upon. These soft rubber supports were arranged to form a diaphragm, so placed and so pierced with openings that it performed the rather conflicting duties of giving free access of the electrolyte to the surface of the active material, and at the same time of giving sufficient support to this active material. Some device of this kind would appear to be very useful to us at the present time.

I think that, although Mr. Epstein's figures for the power required to drive an electric carriage on the road appear at first sight to be rather understated, yet, when one comes to check them carefully, we find that this is not the case, and that it is quite possible to work a vehicle over an ordinary roadway, if that vehicle is fitted with pneumatic tyres, at the speed of 10 miles an hour for rather over 1 actual H.P. developed; and I think there is very little doubt that we could make our machinery and our gearing so efficient that we should not require more than 1,400 watts at the terminals of a battery of accumulators to give this power. In order to do this we ought to be able to see our way to design a vehicle to carry two persons the weight of which, including these two persons, will not exceed 22 cwt. The vehicle used by Mr. Manville's company are at present about 30 cwt., and as these are the first of their kind, and do not appear to be specially designed for lightness, I think that it is quite possible, or probable, that the above figures will be realised.

Gen. Webber.

General C. E. WEBBER: I think we are much indebted to Mr. Epstein for this paper. When I read it prior to the meeting I felt some regret that he had not referred in any way to the

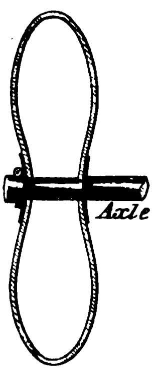
Gen. Webber.

experience with his own accumulators on the Birmingham tramways. That line is the only example in this country of such form of electric traction. It fell to my lot about five or six years ago to have to make a very careful examination of the system and of its working, and Mr. Snell, who is now the electrical engineer of the Sunderland Electric Light Works, and who was my assistant, stayed for about three weeks at Birmingham making observations. The conclusion which we came to—and it was then most carefully considered, but was not made a matter of publication—was, that the result of the use of accumulators in that case had been much misrepresented, because of circumstances to which I think Mr. Epstein alludes in a part of his paper to-night. When he speaks of what the system of charging and moving the cells should not be, I have little doubt that he had on his mind the arrangements that were made for handling the accumulators. At that time I think I knew more than he did, and I remember informing him of the results of those tests, which proved distinctly that it was not having fair play, because at least 60 per cent. of the energy that might have been generated did not find its way into the cells. I have since then frequently felt regret that the faults of that installation were not brought to light, and had not been taken into account as conditions easily avoidable, and that others, including Mr. Epstein himself, have not been able to induce any of the tramway owners in the United Kingdom to try accumulators with the knowledge which that example afforded, because I believe if anyone had done so we should ere now have had as good results as are described to exist in Hanover and in Paris.

Mr. Epstein refers to an ideal cell, and I hope that in his answer he will tell us if it exists, and with what degree of success it can be produced by himself or other makers of accumulators for commercial use. But there is one point that struck me in his paper as being almost the subject of a paper in itself, and that is the question—a subordinate question, of course, to the actual use of accumulators for the propulsion of vehicles—I mean the reduction of the tractive force. It points, no doubt, in the minds of all who have studied the subject, to the question of the

Gen Webber.

wheel. I do not know whether any of those gentlemen who are present to-night saw the experiments that were made with an omnibus by the Electrical Power Storage Company some two or three years ago. It was exhibited on the Chelsea Embankment, and for a few minutes at one time carried three Cabinet Ministers. Fortunately for the Ministry, when the bus (which I may say was most excellently designed) on that occasion stopped to set down, those gentlemen did not alight immediately, and did not mix with the crowd of spectators, chiefly composed of little boys, because one of the wheels suddenly burst, and everybody thought it was a genuine explosion. All the little boys fell on the flat of their backs on the ground, and of course the omnibus could not go any farther. I do not describe this as evidencing any want of precaution, care, and ingenuity in the work of the designer of the omnibus, who was Mr. Frank King, the able and well-known engineer of the Electrical Power Storage Company, and a gentleman who had given an immense amount of time and trouble in producing what was then a novel kind of road car. It only failed in the manufacture of the wheels. They were strengthened with canvas covered with rubber, in section something like this [*diagram*]. There was a round dished steel plate on each face in the centre, through which the axle passed. When the omnibus was loaded there was probably at least a ton on each of the four wheels, and the length of the rectangle which the wheel covered on the ground was probably four or five times its width. Here was an important element of resistance to the tractive force, which, on consideration, you will perceive was difficult to overcome in a wheel of the kind, the form of which, however, had many merits. I hope I may be pardoned in referring to wheels instead of accumulators used in traction, but, as it is a very important matter in connection with their use in road traction, I would like the meeting to consider the section of a wheel which I cannot help thinking may be a solution of the difficulty—for it is a difficulty when great weights are carried on



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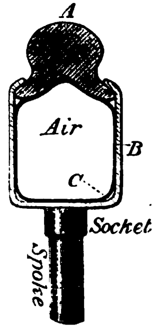
spring vehicles. The section of the tyre is of this shape, made of hollow steel [*diagram*]. A solid rubber tyre, A, is used, and inside the steel tyre, B, there is an ordinary india-rubber air bag, C, which is inflated in the manner usual in bicycles. The inflated bag, or concealed tyre, presses the solid tyre against the two shoulders of the steel tyre, and keeps it tightly in its place. Being solid rubber maintained as a circle by the pressure on its inside periphery, the width and length of the rectangular area that rests on the ground are dependent on the weight of the load, and the



inflation of the concealed tyre, which is protected. The additional elasticity which this provides saves the solid rubber tyre, its life being five or six times longer than that of a tyre which covers a solid wheel. The spokes are joined to the steel tyre by a socket. This arrangement of tyre, which has been made in various forms (I examined one to-day), is, I think, in the right direction, and ought to be carefully studied by those engaged in these matters, and who require an elastic tyre that is very free from ordinary liability to injury, and who are seeking to reduce the traction even to a greater extent than that indicated by Mr. Crompton. In again thanking Mr. Epstein for his paper, I can only add that I wish he had given us a great deal more of his own experience, and I hope that in his reply to the remarks which may fall from speakers, he will tell us a good deal more than he has told us in his paper, particularly as regards the traction on tramways, which is a question that has not been nearly sufficiently thrashed out. Considering the objection that some of our municipalities have to overhead wires, and considering the extremely strong objection, on cost account, which exists to the slot system—for instance, as now being constructed in New York, which I examined carefully the other day—it is necessary for us electrical engineers to bestir ourselves. We want a system without the overhead wires, which is not very much more costly than with them. Therefore, the question of accumulator traction is one of much interest to us, and I trust that this paper is only

Gen. Webber. the beginning of a revival of the subject, and that as it is being worked out in Dresden it will also be tried here. Although I foresee that the need for an inexpensive underground system may yet be furnished by my friend Professor Perry in another way, still, there is ample room for what he is able to do, as well as for accumulators.

Mr. Manville. Mr. E. MANVILLE: I think Mr. Epstein is really deserving of our warmest thanks for this paper. I have often had an ambition to read a paper on accumulator traction myself, but I have never had the opportunity, and I now find that Mr. Epstein's ideas exactly express mine on that subject. I find there is hardly a point that experience could show pointing to success or failure in the use of accumulators for traction purposes that has not been referred to here by Mr. Epstein, and he really leaves very little for anybody else to say. In connection with the Birmingham Central Tramway accumulator system, which has been already mentioned on two occasions, I had the fortune—or shall I call it the misfortune?—to be responsible for the running of that system for two of the later years of its life, and I believe I owe many of the grey hairs I have now to that fact. At the outset I thought, as most people seem to think, that the expenses of running that system were due almost entirely to the rapid deterioration of the accumulator plates. I wish that was so. If it were only a question of deterioration of the accumulator plates—although that was a moderately heavy item—the thing would not be unsuccessful commercially. The real expenses incurred in handling such a system, and which, in my own opinion, make it commercially unsuccessful, are the effects produced by the accumulators rather than the accumulators themselves. For instance, carrying cells within enclosed spaces within cars is most destructive, and, unless you have actually seen the way in which the car deteriorates inside, you would never believe how rapidly this takes place, and what it costs to keep it in repair. It costs you a certain amount to renew the plates as they actually wear out, but the staff required to handle them—to make changes of the plates of a cell, and so forth—is also very considerable, and quite an appreciable cost to be added to the maintenance of the plates themselves.

Ever since I had anything to do with that company—and I got some experience there—I came to exactly the same conclusion that Mr. Epstein has been pointing out to you this evening, that the cells should certainly be carried independently of the inside of the car, either underneath or on an entirely separate vehicle not likely to be damaged by the cells or the acid. A system equipped in that way would have many more chances of commercial success than in the other way. We also used to believe that the deterioration of the accumulator plates was due to the vibration incurred in carrying the cells about. I believed that firmly for a long time, but lately, in connection with this other concern which Mr. Crompton was referring to, I have made a number of tests of accumulator cells of all descriptions. These tests were carried on for a number of months—I do not suppose there is any other certain method of testing accumulator cells than that spreading the test over a length of time—and they were carried on in two ways. In each type of battery a stationary cell was used, as well as one which was violently vibrated all the time during which it was discharged. I think I am safe in stating that it was more violently vibrated than it would be on a carriage with solid rubber tyres on the road and not on tram lines. Some of the different types of cells proved to be good and some to be bad, and there were a large number of types altogether. But one fact was patent to everybody connected with them, and that was that none of the cells that were vibrated were any worse than the corresponding cells that were still, and in many cases were even slightly better. This latter effect, no doubt, was due to causes in the individual cells. In any case, neither the vibrated cell nor the stationary cell could be called better or worse than the other. That is a very important fact, because I think we have all thought that vibration had a good deal to do with the deterioration of accumulators used for traction purposes, and I now for my own part do not believe this in the least. Mr. Manville.

I think the chief reason for the rapid deterioration of cells used for traction purposes is due to the stringent electrical conditions under which they are handled. I mean the heavy occasional discharges. That brings one to the difference in the

Mr. Manville. probabilities of success between an accumulator tram-car wholly propelled by accumulators and a light vehicle propelled by accumulators. As Mr. Epstein has pointed out, the proportion of weight of accumulator to tram-car up to the present has been something like 1 to 4—I think you may take it at that. It is quite feasible, without using lighter plates in the same type of cell, to put a much heavier weight on your small motor cars, which is exactly what you ought to do, and to increase the proportion of accumulator weight to total weight in the proportion of, say, 1 to $1\frac{1}{2}$. That means a corresponding difference in the maximum duty that the cell is called upon to perform, and I believe that that difference will have a very considerable effect on the life of the cells used in that way. Mr. Epstein pointed out that in a combined trolley and accumulator system a very good plan would be to have the cells on separate trucks congregated near the entrance to the centre of the towns, whence they would be used and charged from the trolley wire there. There is a further advantage, which he omitted to point out, which will be obvious to all of you, and that is, it will materially improve the regulation of the trolley line, and decrease the cost of the feeders. It might, at least, be arranged to do so.

As to the traction of rubber-tyred vehicles on roads, that is certainly a surprising thing when you investigate it for the first time in a practical manner. I think electricity often helps us to investigate things in a way that would be very difficult otherwise. It is not an everyday experiment to ascertain what the traction of an ordinary carriage is. You might pull it through a dynamometer with a horse—I do not know whether the horse would run sufficiently smoothly to enable you to do it—but with an approximate figure for the efficiency of your motors and gearing, which is easily ascertained with comparative accuracy, your instruments tell you all the rest. I quite agree with Mr. Epstein that on hard London roads—it is not so much a question of smoothness as hardness; roads the reverse of country roads, which are often very soft and very thick—the traction certainly does not exceed 60 pounds a ton. I think we should be safe in putting it at nearer 40 pounds per ton.

Professor W. E. AYRTON: I should like to ask Mr. Manville if he could give some actual figures as to his cabs: for example, firstly, were the motors 1-H.P. or 5-H.P.? secondly, what was the average power the cells actually gave out? and, thirdly, what was the maximum power they were called upon to give out in propelling the cabs? I should also like to ask what were the types of the cells Mr. Manville had tested? Were they pasted or non-pasted, and what was the nature of the vibration they had been subjected to? Prof. Ayrton.

Mr. MANVILLE: Certainly. The motors used are those capable of an input of 3 kilowatts, without unduly heating. Mr. Manville.

Professor AYRTON: That is, about 4 H.P.?

Mr. MANVILLE: Yes; as a maximum. The motors have double-wound armatures, and fields which are connected together in various combinations to produce different speeds, the accumulators always being left joined up in series with each other as a battery. There are 40 cells used, and from my own experience in one cab—I should not like to vouch for it as the average of all of them—the current taken on level wooden pavements (I am giving these purely as approximate figures) is about 30 amperes E.M.F., 80 volts.

Professor AYRTON: 80 volts?

Mr. MANVILLE: Yes; and it increases from 40 to 45 amperes on the roughest kind of hard roads. That is about the maximum variation on level roads. I have seen the current as high as 120 amperes up a very steep hill.

Mr. S. MAVOR: May I remind Mr. Manville that there is one question Professor Ayrton asked which he has not replied to, viz., the method adopted for vibrating the cells? I think this may have had an important bearing upon the performance of the pasted plates. Some experience I have had during the last fortnight over granite-paved streets on an oil-driven motor car has given me a very good idea of the extremely rough usage an accumulator used for ordinary country roads or granite-paved streets, such as we have in the northern cities, must endure. It is not the mere matter of vibration, but the very rude and rugged shocks and jolts which the plates must be subjected to. In that Mr. Mavor.

Mr. Mavor. connection I should like to ask Mr. Manville what method of vibrating the cells he adopted in his test.

Mr. Manville. Mr. E. MANVILLE: I forgot to answer that question. The cells were practically all the types that can be purchased in the market. They included both Planté cells and pasted cells, or partially Planté and partially pasted. There was no difference in respect of the effect of vibration between any of them. The method of vibration was somewhat an heroic one. There were two countershafts in the same plane, and from these was hung a shelf by four eccentrics, and the eccentrics on one shaft were of course placed out of centre with each other. One counter-shaft was driven from the other by a loose rope, and the first one was driven by a loose rope off a small motor, so that the whole thing was joggling in a most extraordinary fashion, as the people in the building knew to their cost.

Mr. Carter. Mr. E. TREMLETT CARTER: I should like to ask Mr. Epstein whether the practice of connecting the batteries in parallel for starting and in series in running is to be recommended? If a series-parallel controller could be used on the batteries in this way, it would greatly simplify matters where only one motor could be used, such as in very small motor cars, &c. But I am not sure if the practice of disconnecting the cells and arranging them in various groupings is to be recommended, as regards the action on the cells themselves, and I would like Mr. Epstein to tell us something about this matter.

The meeting then adjourned.

The Three Hundred and Sixth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, November 25th, 1897—Sir HENRY MANCE, C.I.E., President, in the Chair.

The minutes of the Ordinary General Meeting held on November 11th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—
John Joseph Hatt.

From the class of Students to that of Associates—
C. F. Hesketh.

Mr. H. W. Handcock and Mr. L. L. Robinson were appointed scrutineers of the ballot.

The SECRETARY announced that a donation to the Library had been received since the last meeting from Mr. Swinburne, to whom the thanks of the Institution were unanimously accorded.

The PRESIDENT: We will now resume the discussion on Mr. Epstein's paper on "Accumulator Traction on Rails and Ordinary "Roads." Mr. Manby has already spoken on the subject, but he wishes to make a few additional remarks.

Mr. E. MANBY: I think that my statement about the Marschner accumulator at the last meeting was rather incomplete, and I wish to add a few further figures. The Dresden trial car, containing 48 people, provided with a single motor, working under 250 volts and hauling another car containing 32 people, has travelled on occasions 130 miles with one charge, developing 68·8 Mr. Manby.

Mr. Manby. kilowatt-hours. It carried a battery of 144 cells, placed under the seats of the motor carriage. This battery weighed 2,200 kilos gross, or nearly 5,290 lbs. The gross weight of the battery would thus appear to have been about 77 lbs. per kilowatt-hour, or 13 watt-hours per lb. However, I understand a still lighter form of battery is now being manufactured. The batteries are charged *in situ*, without removing, at the rate of 60 amperes. The efficiency is said to be very high, and to exceed 90 per cent. The sum paid to the City Electrical Works at Dresden is a fraction over 9s. for a full charge, which works out at 0·84d. per mile, as power cost for the traction of two cars containing together about 80 people; but this cost, of course, includes a considerable profit on the supply. With reference to Mr. Epstein's remarks upon what might be expected from accumulators in ordinary traction, I should like to offer the following observations. Mr. Epstein suggested in his paper that an ideal traction battery should not require handling, and should be treated as an indivisible unit; such is the case with the Marschner battery, which is charged *in situ* only once in a working day. The capacity of the Marschner battery is said not to have been impaired at all by the seven months' uninterrupted trial, and has required very little supervision and very little upkeep. Mr. Epstein assumes that the life of a battery is dependent upon the number of chargings. In this respect the Marschner batteries would show considerable advantages over others, as, in practice, they have only been charged once a day, and they have given a very large mileage under those conditions. So that, instead of a life of 750 days, as Mr. Epstein states in his paper as what might be expected of a battery, they should actually work 1,500 days without renewal. Of course, they have only been tried for about six months, and, though this efficiency has been unimpaired so far, time only can tell if such a result will be attained. The hauling car provided with the Marschner battery in Dresden weighs with the battery complete, when fully occupied by passengers, about 12 tons; but it hauls over the flat section another car weighing, with 32 people, complete, 5½ tons; and on one occasion, as already stated, over the flat parts of the line, it travelled 130 miles in the course of the

day without recharging—that is to say, with a load of 17½ tons, Mr. Manby. or 2,275 ton-miles, on that day if the cars were full, 1,917 ton-miles if they were half full, and 1,800 ton-miles if one-third full of passengers. On that occasion the battery gave out about 68·8 kilowatt-hours, so that, if we assume the cars to have been one-third full, the output per ton-mile would have been about 38 watt-hours; whereas, Mr. Epstein assumes that 80 watt-hours would be the proper proportion per ton-mile haul. Of course, this is obviously dependent upon other considerations than the merits of the battery; but in one particular trial that was made by the Dresden Tramways Company over specially selected parts of their tramways, where they had gradients of 1 in 23 and very sharp curves, the actual output was about equivalent to 66 watt-hours per ton-mile haul, still considerably under Mr. Epstein's allowance.

Professor W. E. AYRTON: The propulsion of vehicles along Prof. Ayrton. ordinary roads with accumulators is an old love of Professor Perry's and mine, and it may be in the memory of some of you that in November, 1882, there was an illustrated description given in the *Electrical Review* of a tricycle which we were then running in the streets of London propelled with some of the first Electric Power Storage Company's accumulators that were ever made. It was also lighted electrically.

Mr. Epstein has rightly said in his paper that all the early experiments—in fact, it may be said all the experiments until perhaps quite recently—were commercial failures, although, from a scientific point of view, they were very interesting. He gives us in his paper some particulars of the Hanover traction. It would be very interesting if he would add the proportion of the lengths of line where the cars receive currents from a trolley wire and from accumulators respectively, so that we could see how far the possibly not entire success of the accumulator traction might be compensated for by the very great success of the trolley wire portion. I should also be glad if he would explain a point which I do not understand in his paper. He says, in speaking of the probable cost of maintenance of the accumulators: "The "directors anticipate that its cost will be increased somewhat,

Prof. Ayrton.

" but they are also confident that it will in no case exceed 60s. " per car and month (which amounts to 0·266d. per car mile)." A little later on the author says " accumulator traction incurs an " additional expenditure of 0·4d. per car mile as compared with " the trolley." There is some difference, no doubt, intended, but it is not quite clear what that difference is.

I cordially join with those who spoke on the last occasion in congratulating Mr. Epstein for bringing before us a paper connected with the subject of vital importance to electrical engineers at the present time. If I had been asked, I should—indeed, I may say when I was asked some eight months ago to advise on this question I did—advise against the probability of the use of accumulators being commercially successful with ordinary road traction; and if Mr. Manville can succeed with the London electric cabs in making them successful, there will be nobody who will be more delighted than myself—indeed, I may add I long for Mr. Manville to prove me to have been in error. But we must all feel that the time has gone by for trying to get success by not having all the facts before us. The time has gone by for Edison's statement to be any longer applicable, viz., that the inherent capacity of man for lying comes out when he deals with accumulators, and, therefore, if Mr. Epstein will allow me, I am going to ask some questions about his paper, for I know that he is a man who is very anxious to learn the truth about accumulators, and what is more, I am sure that he is anxious that others should know the truth about his own accumulators.

When I made a series of experiments, lasting over several months, for him some years ago, he never thought of suggesting that I should present a rosy report. Perhaps he would not have found it conducive to his happiness if he had ventured on such a suggestion. However, no such hint came from him; but, on the contrary, what he said was, " Test the cells not to get the best " results, but treat them as badly as you can, and learn what are " their good points by submitting them to the roughest possible " treatment." I think I may add that nobody would have felt more hurt than Mr. Epstein if, in the drafting of my report, I had applied in the most delicate way even the tip of the varnish brush to my statement.

Therefore I know that he will not mind my throwing down the gauntlet, and breaking a lance with him about some points in his paper. Prof. Ayrton.

The calculations that Mr. Epstein makes as to the weights of accumulators, &c., needed in any particular case depend, of course, on two things, viz., on what is the storage capacity of an accumulator, and what is the resistance per ton to traction on an ordinary road. As regards the first point, the storage capacity of an accumulator: many of you are aware that a great many experiments have been conducted for some years past in my laboratory on all kinds of accumulators. A single set of experiments has lasted over several weeks or even months, charging and discharging incessantly day and night, so as to get the cells into a really normal working condition. And I was under the impression that I had some sort of knowledge of what might be expected from the storage capacity of an accumulator. But a few days ago a pamphlet was put into my hands issued by the Lamina Accumulator Company. In that pamphlet there are extracts from reports of several persons who have presumably made tests. No accounts, however, are given as to how the tests were conducted, whether they included a cycle of charging and discharging, or over what period the tests extended, or what was the size of the cells tested; but I was struck with one statement in one particular report which said that, with reference to this Lamina accumulator, "Under normal conditions its capacity for "storage of work is about 14 watt-hours per lb. weight (gross) "of the cell. This figure is as high as that of any accumulator "yet on the market, and higher than that of any accumulator of "equal durability."

On reading this statement I rubbed my eyes. I thought, have I been going on all these years in a sort of London November fog on the subject of storage capacity, and is it really a fact that you can obtain something like twice what I have ever succeeded in getting with any accumulators? Well, I next examined the results of some tests which had been made on the Lamina accumulators, and certainly the storage capacity obtained was not nearly as large as 14 watt-hours per 1 lb. gross weight.

Prof. Ayrton.

Then I turned to the circular of the Lamina Accumulator Company itself, and I found that the company only claimed half as much storage capacity for its own five-plate accumulators as was stated to be found by the expert for this Lamina type. It is quite possible, of course, that, although our ordinary experience with manufacturers had led us to conclude that it is not their custom to decry their own wares, the modesty of the Lamina Accumulator Company may be such that it only claims for its accumulators less than half the storage capacity they actually possess. If that be the case, well, it is interesting to know that the Lamina Accumulator Company is unique in the whole existence of electrical engineering.

On the other hand, we have heard to-night that the accumulators used in Dresden have approximately the same storage capacity. I have just worked it out, and it comes to 13 watt-hours per lb. I am not quite clear whether the figures Mr. Manby gave, when he talked about 77 lbs. per kilowatt-hour, meant 77 lbs. gross weight, or of the plates alone.

Mr. MANBY: It is gross weight.

Professor AYRTON: Then that would lead to a number not differing very much from the number I have referred to, and it comes to about—if I have done it rightly—13 watt-hours per lb. gross weight.

In this connection it may be interesting to give the results of the capacity in watt-hours per lb. gross weight of *traction* cells which I have had calculated from their capacity in ampere-hours, as given by some of the most important English accumulator manufacturers in their own price lists.

To pass from the capacity in ampere hours to that in watt-hours, it has been assumed that the mean value of the P.D. between the terminals of a cell is 1·9 volts during the whole of the discharge. This I find by actual experiment is correct to within a few per cent. for any ordinary storage cell when discharged at such a rate that the P.D. begins to fall rather rapidly at the end of about eight hours. And since my criticism of the statement, "14 watt-hours per lb. weight (gross)," is not a criticism of a few per cent., but of a 100 per cent., it is immaterial

for my purpose whether in the case of some of the cells given in the following list 1·98 volts would have been a more accurate value to have taken for the mean P.D. during the entire discharge. Prof. Ayrton.

The period eight hours I have selected because it is the one used by Mr. Epstein in his calculation for the tram-car on page 669 of his paper.

CAPACITY IN WATT HOURS PER ONE POUND GROSS WEIGHT OF TRACTION CELLS WHEN DISCHARGED IN EIGHT HOURS.

Name of Cell.	Type.	No. of Plates.	Box Material.	Watt-Hours Capacity per Pound (gross).
Tudor*	P. L.	7	Ebonite	3·7
" *	"	11	"	4·0
Chloride	T. B.	7	"	5·7
"	"	17	"	7·2
I. E. S.	A.	7	Teak and Lead	6·4
"	"	17	" "	7·4
"	"	7	Vulcanite	9·2
"	"	17	"	10·5
F. P. S.	Faure-King	7	Ebonite	8·6
...	...	19	"	10·2
Lamina	C. Motor Car	5	"	7·6
"	"	7	"	8·4
"	"	9	"	8·6
"	T.B. Tramcar	7	"	10·8
...	...	9	"	11·8

* These Tudor cells are specially made for very rapid working rates, and, therefore, would not be used when an eight hours' discharge was desired. They are, however, included in this table as they are the *traction* type of cell as manufactured by the Tudor Company.

Prof. Ayrton.

It is to be specially remembered that the figures in this table refer only to perfectly new cells, and must certainly be diminished if we desire to know the capacity that may be expected after the cells have been in constant use for a few weeks. The highest result that has been obtained in my laboratory for an eight-hours discharge, with a comparatively new seven-plate cell, is the 9·2 watt-hours per lb. given above, but, even with this type of cell, I should not like to rely on more than eight watt-hours per lb. gross weight after the cell had been in steady use for some weeks.

The other point is the question of the resistance to traction. Mr. Epstein, Mr. Crompton, one or two others, and myself, had a little talk after the last meeting as to what Mr. Epstein exactly meant by his statement "that on good road paved with asphalt or "wood, and in fairly good condition, the tractive force of vehicles "as hitherto built should, on the level, not exceed 60 lbs." And he told us that he meant 60 lbs. per ton. This Mr. Crompton thought was too large.

I therefore decided to make tractive force experiments with some of the vehicles propelled with accumulators that are actually in use in London at the present time. These have not yet reached me, but, in the meantime, three of my students, Messrs. Evered, Jacomb-Hood, and Fawdry, have carried out a series of tests with a Beeston-Humber bicycle fitted with Dunlop-Welsh tyres and a Humber block chain, and in good order as regards friction of bearings, gearing, &c.

In some of the tests this bicycle was drawn by an elastic cord wound up by an electro-motor, in others by means of another bicycle, to which it was attached by an elastic cord, a spring balance being inserted in each case. The results are given in the following table:—

Prof. Ayrton.

Tabulated Results.

Speed Miles per Hours.	Load.	Distance Moved.	Tractive Force per Ton required on		
			Concrete.	Macadamised.	Wood Block.
	Lbs.	Feet.			
3·18	164	70	18·5	...	...
3·53	164	90	27·4	...	...
3·95	164	95	29·4	...	...
4·02	164	100	29·4	...	...
4·55	164	100	34·2	...	...
4·65	164	80	31·4	...	...
4·65	164	100	31·4	...	...
5·05	164	90	37·5	...	...
5·20	164	90	37·5	...	...
5·27	164	90	32·8	...	...
5·35	164	80	29·4	...	...
7·0	181	154	...	49·5 * W G	...
7·6	181	360	...	...	30·9 D
7·65	181	280	...	37·2 W G	...
8·5	181	154	...	30·8 W G	...
8·7	181	248	...	43·2 W G	...
8·85	181	570	...	...	49·4 * W
9·15	181	334	...	...	49·4 * W
9·6	181	365	...	...	30·9 D
9·65	181	460	...	...	30·8 W
9·8	181	365	...	...	27·8 D
10·5	181	154	...	61·8 * W G	...
11·0	181	158	...	37·2 D	...
11·3	181	158	...	43·2 D	...
12·4	181	154	...	61·8 * W G	...
12·5	181	220	...	49 5 W G	...
12·5	181	220	...	43·2 W	...
12·5	181	365	...	...	41·4 D
12·7	181	289	...	...	43·2 D
12·9	181	100	39·	...	...
12·9	181	110	39·5	...	...
13·1	181	154	...	61·8 * W G	...
13·1	181	384 (?)	...	...	34 D
13·7	181	365	...	...	41·4 D
13·8	181	434	...	...	43·2 D
14·9	181	207·5	...	...	58·7 * D
17·2	181	365	...	...	58·7 * D
17·4	181	158	...	61·9 * D	...
17·9	181	158	...	49·5 D	...
Mean Values	...	...	32·1	48·5	43·1

D = Dry road. W = Wet road. G = Good road.

* = Decided wind tending to increase the force required in these cases. There was, however, an appreciable wind in nearly every case.

In view of the fact that the friction of the gearing and of the wheels on their axles of an auto-motor is probably not on the average as small as in the bicycle, it follows that an estimate of

Prof. Ayrton.

60 lbs. per ton for the necessary tractive force is not too large. With this value Mr. Epstein calculates that a vehicle carrying from two to four passengers, and running at an average speed of eight miles on the level and four miles up a gradient of 1 in 24, requires at the driving axle about 2 effective H.P. in the former case and about 3 H.P. in the latter. On the other hand, Mr. Manville has told us that with the London electric cabs, which, as we know, carry two passengers and the driver as a maximum, the power furnished by the cells varied from 2,400 watts to 3,600 watts on the level, that is, from 3 to 5 H.P., and reached 8,000 watts, or 11 H.P., when the cab was going up a steep hill. Either, then, the London cab is heavier than Mr. Epstein's vehicle, or the resistance to traction is more than 60 lbs. per ton, or the efficiency of the motor and gearing on the London cab cannot exceed some 50 per cent.

I next come to the tram-car carrying 52 passengers and pulled by a dummy carrying the accumulators. Mr. Epstein has concluded that 2¼ tons of accumulators will be sufficient to propel such a car and the dummy, whereas, for the following reasons, which have been put together for me by one of my assistants, Mr. Allen, I judge that the accumulators necessary for the purpose will weigh twice as much.

A tram-car carrying 52 passengers in it will weigh at least 8½ tons, and frequently a great deal more. Therefore, taking 33 lbs. per ton as the tractive force on a clean, level, straight tram line, it will require certainly not less than 7½ H.P. at the wheel tyres to propel such a vehicle at 10 miles an hour. Now, under the most favourable conditions—that is, with the motors in series without any added resistance—the efficiency of ordinary tram-car motors and gearing at this speed is in practice about 70 per cent., so that the power furnished by the accumulators must be about 11 E.H.P.

From what I have said at the beginning of my remarks, I consider that it would be certainly unwise to count on more than eight watt-hours per lb. gross weight for an eight-hours discharge with accumulators as at present manufactured in England. But I will take nine to give accumulator traction this advantage.

Prof Ayrton.

Then a preliminary calculation shows that the weight of the accumulators necessary will be about 5 tons. Hence, with the accumulators, dummy, and the loaded passenger car, we have to pull not less than 14 tons. Consequently, the actual weight of accumulators required will be at least

$$\frac{14}{8 \cdot 5} \times \frac{11 \times 746 \times 8}{9 \times 2240}, \text{ or } 5 \cdot 4 \text{ tons.}$$

Next, how far is this result borne out in actual practice? The Paris Tram-Car Company uses cars to seat 50 passengers, weighing 11 tons empty and 14 tons loaded. The weight of the battery is only 3 tons, but one charge only lasts for a run of 37 miles in, say, four hours, instead of the eight hours taken by Mr. Epstein in his example.

Using the efficiency for gearing and motor referred to above, this 3 tons of accumulators corresponds with eight watt-hours per lb. gross weight, the value which I have been accustomed to employ.

Consequently, if these Paris tram-cars carried accumulators to serve them for an eight-hours run, the weight would certainly have to be doubled and become 6 tons, if we remember that, although the capacity will be greater at a slower rate of discharge, the power exerted will also have to be greater, since the weight to be propelled will be increased.

Such a tram-car as I have been considering would, further, when running on a trolley line be fitted in practice with two "G.E. 800" motors, each rated to stand an input of 40 amperes at 500 volts for one hour. The efficiency of such a motor is a maximum when it is taking a current of 25 amperes and running at a speed corresponding with 12 miles an hour. Two such motors are then producing a "drawbar pull" of 860 lbs., which corresponds with a resistance to traction of 100 lbs. per ton, if we consider the loaded car as weighing 8½ tons.

Such a resistance to traction is not uncommon on sharp curves, or on steep grades combined with curves. And even when the car is running on a clean, level, straight line, and when, therefore, the real resistance to traction is only some 30 lbs. per ton, the "drawbar pull" exerted by the motors must be about

Prof. Ayrton. 100 lbs. per ton if the car is being accelerated per second with a velocity of 1 foot per second.

At starting the current through each motor frequently is 50 amperes, corresponding with a "drawbar pull" of 2,000 lbs., or an acceleration per second of nearly 3 feet per second.

My statement, therefore, that Mr. Epstein's estimate of 2¼ tons of accumulators for his tram-car must be more than doubled is certainly not over the mark.

There is another point, to which reference was made last time, which is of considerable interest, viz., what is the best way of regulating the speed of a car propelled with accumulators. With the London cabs there is, as doubtless you know, a double-wound armature with two commutators and two sets of brushes, and the two halves of the winding of the field magnets can be dealt with separately, so that the armature windings may be in parallel, and the field windings in parallel, with the two sets in series with one another; or one set of double windings may be in series with one another and with the other pair of windings in parallel, or all four may be in series.

The regulation of speed is effected entirely by altering the arrangement of the windings, and not at all by altering the arrangement of the cells. But had Mr. Manville been present I should like to have asked him whether he had considered the latter possibility. Of course, we all know, now, that it is unwise to treat cells so as to allow of a greater discharge from some than from others of the same batch. As long ago as 1882, Professor Perry and I patented a method having a special reference to the system which was then first being tried by the Brighton and South Coast Railway of electrically lighting the carriages with a dynamo driven from the axle of the guard's van. With that device every cell was brought into use successively by means of a pair of travelling contact fingers, although the number attached to the lamps at any one time was not the total number carried by the train.

But without using one cell differently from another, it was clear that with 40 cells, the number in a London cab, there were three useful combinations, viz., all in series, or 20 in series and

two in parallel, or 10 in series and four in parallel. It might be objected that, in consequence of the different resistances at the contact of the wires and the terminals of the cells, or in consequence of the difference in the resistance of the cells themselves, they would not discharge uniformly when placed in parallel. This contact resistance was mainly due to corrosion by the acid spray, and this could be prevented by finding out a really successful varnish or coating that was quite impermeable to the spray. The difficulty arising from inequalities in the resistances of the cells themselves was more difficult to overcome, and was exaggerated by the fact that the larger the current passing through a cell the lower became its resistance. Prof. Ayrton.

Finally, I would like to ask why was it necessary for the cab company to go to America for its electro-motors? Doubtless I shall be told because the Lundell motor is lighter for its power than any other motor. And I fear that is a fact, for recently a firm of London instrument makers told me that, requiring to fix a motor to an existing wall in order to drive part of their shop, they purchased a Lundell motor, for if they had obtained an English motor they would have been compelled to build a special wall to hold up the heavier motor.

Mr. THOMAS PARKER: This is an interesting subject, and the difficulty of it is that it takes a long time to get experience with accumulators. There has been a great deal said in regard to the installation of the Bournbrook Birmingham trams. I do not know that there has been anything definitely written about that work, but, as I designed it, and ran it for a time, and have heard partial statements respecting it, as it is probably the best thing we have to refer to, possibly a few words from me on the subject might be interesting. We have all been in love with accumulators at various times. My love for them began about 17 years ago, but in 1891 I was asked to construct a locomotive with accumulators to compete on the lines at Birmingham with the steam-tram motors. That locomotive was constructed and put to work in Birmingham with accumulators, and for months it held its own with the tramway engines of Birmingham, and to the satisfaction of those who were appointed by the Council Mr. Parker.

Mr. Parker. of Birmingham to watch it in its work. The locomotive was 8 tons weight, and its accumulators were all fixed. They were charged stationary, and it was designed and carried out by Elwell-Parker, Ltd., without interference. The success of this locomotive determined the Birmingham Tramway Company to put down a station at Bournbrook, and we were all sanguine enough to hope that accumulators might do as well on tram-cars as they would in stationary installations. The contract was accepted by me for the Construction Corporation, but we were not left with a free hand with regard to the mechanisms and electrics of that installation. There were several points imposed which have been the cause of great trouble and inefficiency of the tramway. Firstly, the Birmingham Council determined that we should use the identical cars used on the cable tramways. As mechanics you will all know that these cars are double bogie cars; they have a small bogie on each end with four wheels. The gauge is very narrow, and upon one of these bogies had to be put the motor for driving the very heavy car, and to get tractive force enough it was necessary to have double gearing on the one bogie for that purpose. There was another restriction—this time imposed by the mechanical engineer. He said, "You must run these cars "at 15 miles an hour if they are required." We were then between two fatal impositions. There was another thing which might perhaps be mentioned which has told very severely upon that installation, and that is the cost of the installation. It was, in my opinion, abnormally large. These features brought about the circumstances which have over-handicapped that installation. We built the whole of the electrical plant and put it to work. Since that time I believe every battery maker has been there, and in the same box as we were; I do not know whether any one of them discovered the evils or not, but happily I did not have to suffer them very long. But the fact was this: On the cars we did not get the accumulators to do what they would do in stationary installations. Without entering into any discussion on accumulators, up to the present minute I have found no accumulator free from the defects of the original accumulators used there, and still being used there, namely, when those accumulators are

abnormally discharged they lose their capacity, not slowly, but suddenly, and they cannot be recovered again. It was horrifying to me to find that a car which to-day would run six or eight journeys to and from Bournbrook to Birmingham, in two or three days' time would not do more than three or four at the outside. This broke down my hope of accumulators for traction, and since that time you have not seen me meddle with them very much, or having much to say about them. Still, if we had been given a free hand on that tramway, I believe it would have been successful; or given a free hand to-day, I would guarantee to make it a respectable engineering installation and make it pay. By simply taking a car which would not be too heavy, and given a better opportunity for the mechanisms on the car, and reducing the speed limit, you would then be able to get the accumulators to work out a fair amount of life, and keep up their capacity. Whatever accumulators have been used, up to the present time you will find this sudden falling off of capacity is the thing you have to meet; and if you do not get your conditions such that the accumulators can be discharged within that range of capacity at which they fall off, you cannot succeed. There is no time to enter into the thing fully now. It would probably be well if there were a paper written upon it, but perhaps the subject is getting a little old. In regard to some of the other points which have been mentioned, my son has been building a car in Wolverhampton, and has there obtained a great deal of data which would perhaps be useful. Before he started to do it, he amused himself some time in pulling a brougham about with a horse and spring balance in it to see what tractive force the brougham required at different weights and grades and speeds. The motor has been built some four months, weighs about 24 cwt., and carries nine people. Those who know Wolverhampton and Tettenhall would be able to understand what I mean when I say it has run morning and noon from Wolverhampton to Tettenhall, doing the inclines on the way; and we have some measurements of the work. The traction is rather surprising. The traction required on a good macadamised road is not so much as it is upon granite-paved setts: about 40 to 44 or 45 lbs. per ton Mr Parker.

Mr. Parker. is the traction we find by measurements electrically. It is also borne out by work required to move the load I have just named.

Professor PERRY: Is the vehicle fitted with ordinary iron tyres?

Mr. PARKER: No; solid rubber tyres. We have had a good deal of difficulty with the wheels, but I think that has been surmounted now. We have a wheel that will carry 15 cwt.,—and when I say "we," I mean my son, because, as a good father, I naturally take a great interest in him. He, I think, has succeeded in getting a different steering gear to what we are familiar with. Many people who have seen the vehicle praise it, and I should be glad if you could get him to read a paper here. With that car we have succeeded in starting it on the top of Tettenhall Bank and running it into the square without any brake on it at all, and it does not vary in speed more than about 6 to 8 per cent. on the road. Running down Tettenhall Bank, which is something about 1 in 16 for a quarter of a mile, will not increase the speed more than to 8 miles an hour. It will mount the hill back again at about 6 miles an hour with the whole power in the same position. Professor Perry nearly made me forget what I was going to say when he began that doubling business. I thought he was going to suggest the old story of doubling the nails in the horse-shoe. I have come to the conclusion that that motor and arrangement must have been invented by the Siamese twins, there is so much doubling. They do nothing of the sort in the cab I am speaking about, and no doubt you will be familiar in London with it before very long. The time of charging and discharging is an exceedingly important item in batteries. I do not care to discuss any particular batteries, because there are many lovers of various kinds, but the question is one of very great importance. With regard to the Lamina battery that Professor Ayrton mentions, from my knowledge of the Planté work it may be almost made to hold what you like; but as to how long it will live is another matter.

Professor AYRTON: What is the weight of the batteries on this motor?

Mr. PARKER: Between 8 and 9 cwt.

Professor AYRTON: And it carries nine people?

Mr. PARKER: Yes.

Professor AYRTON: And 24 cwt. is the car and cells alone without the people?

Mr PARKER: Yes, alone, without the people.

Professor AYRTON: At what speed does it go?

Mr. PARKER: It is set to run 8 miles an hour, and it will not run more—not downhill without a brake.

Prof. Wilson.

Professor E. WILSON: In the following remarks I confine my attention to the words in paragraph 4 on slip 4: "The weight of "an electrical vehicle to carry from two to four passengers, with "motor and battery complete, will be about 30 cwt., which "includes a battery with a weight of 9 cwt. A battery of such "weight ought to be sufficient to supply current for a four to "five hours run, at an average speed of 8 miles on the level, and "4 miles up a gradient of 1 in 24. The power on the driving "axle will be about 2 effective H.P. in the former case, and about "3 effective H.P. in the latter; and, assuming a combined "efficiency of motor and gearing of 65 per cent., the battery will "have to furnish discharges at the rate of 2,208 watts and "2,812 watts respectively."

About six months ago I carried out in the Siemens Laboratory, King's College, London, a series of experiments upon traction

No. of Test.	5	6	7
——	Discharge.	Charge.	Discharge.
Initial volts without current	...	...	2·18
Initial volts with current ...	2·04	2·135	2·02
Final volts with current ..	1·85	2·6	1·85
Amperes constant at ...	15	17·8	14·9
Total time of test in hours and minutes	5 h. 20 m.	5 h. 30 m.	5 h. 15 m.
Ampere-hours	80	97·9	78·2
Watt-hours	157	228	155
Quantity efficiency per cent.	82	...	89·7
Work efficiency per cent. .	68·8	...	76·1

Prof. Wilson. type cells for the purpose of a small work entitled "Electrical "Traction." Mr. H. H. Hodd gave me much assistance in these experiments, which are given in full in the book, and I have extracted the results in the accompanying table, which refer to an Electrical Power Storage Company's Faure-King traction type five-plate cell [*diagram*]. The cell was fully charged when delivered. The two positive plates measure each $7\frac{1}{4} \times 8$ inches, and have a thickness of $\frac{1}{4}$ inch, not including the perforated envelope in which they are placed. They weigh, with lug, when just taken out of the acid, 7 lbs. 7 oz. The total weight of the cell in ebonite box and acid is 21 lbs. 2 oz. The specific gravity of the acid in the fully-charged cell is 1·275.

Mr. EPSTEIN: That includes the acid too?

Professor E. WILSON: Yes; the 21 lbs. 2 oz. includes the acid. Tests Nos. 5 and 7 in the table show for a net drop of about 0·2 volt, with current passing, a work efficiency of about 72 per cent., which agrees with Mr. Epstein's figure. It must be clearly understood that these tests have been made upon new cells, and therefore no conclusions can be drawn as to the life of such plates; and, further, tests made upon isolated cells like these may not represent the average results which may be obtained from many.

It is interesting to see what weight of these storage cells would fulfil the conditions put down by Mr. Epstein. Take his lower rate of discharge at 2,208 watts for five hours, then the watt-hours are 11,040. The F.K. type cell to which I have just referred, when working under the conditions given in the table, gives 156 watt-hours. Therefore, 156 divided by 21 = 7·43, which is the work in watt-hours per lb. of total weight of cell. Dividing this into 11,040 should give the total weight of a battery of these cells to fulfil the conditions put down: that is, 1,490 lbs., as against Mr. Epstein's 1,000. The number of such cells would be 1,490 divided by 21, which equals 71; and, since 2·6 volts are required for the fully charging of the cell at the rate given in the table, the total charging volts would be $2·6 \times 71 = 185$ if the cells be kept in series.

The number, 71 cells, just given can be considerably reduced

if the capacity be increased, and still give currents convenient to deal with in motor and controlling apparatus. Prof. Wilson.

This is important, since the weight of an equivalent battery, so far as work is concerned, comes out smaller, and the limiting conditions are naturally the currents to be dealt with. Let us look at the same type of cell, namely, the Faure-King, but take a larger size. Take the nine-plate instead of five as above. This cell is capable of discharging 150 ampere-hours at 30 amperes, the time being as before, five hours. Its weight is 35 lbs. complete with acid, as against 21 lbs. in the five-plate cell. We have watt-hours per lb. $= \frac{300}{35} = 8{\cdot}57$, as against $7{\cdot}43$. Also $\frac{11040}{8{\cdot}57} = 1{,}290$ lbs., as against Mr. Epstein's 1,000. The number of such cells would be $\frac{1290}{35} = 37$, and, allowing $2{\cdot}6$ volts for each, the voltage would be 96, if the cells be charged in series. This makes no allowance for diminished capacity after continued working, if such exists in this cell. Under these conditions, then, 1,000 lbs., as given by Mr. Epstein, seems low, but he may have assumed a smaller number and different conditions of working. I think it would add to the value of the paper if the author would give exact details as to number of cells and conditions of working.

The experiments which I have made on the Chloride Electrical Storage Syndicate's cell, representing the Planté type, as against the pasted type above discussed, show the same character of result, but with somewhat greater weight. Mr. Epstein's higher rate quoted, namely, 2,812 watts, would naturally increase the weight of the battery, and make the difference greater than I have shown above. It is premature to discuss which type of cell will eventually survive for traction purposes. Another year's experience will, no doubt, go a long way towards settling this point. The cell of the future must be capable of being over-discharged without serious injury.

Professor AYRTON: The figures you mention are 7·43 watt-hours per lb. gross weight with one cell, and 8·57 with the other.

Professor E. WILSON: Yes; 7·43 and 8·57 watt-hours per lb. gross weight—that is, the total weight of the cell.

Prof Wilson. Professor AYRTON: You think those figures very good?

Professor E. WILSON: The tests I am making now have only been continued for six months.

Professor AYRTON: You have never had experience of cells giving twice that storage capacity?

Professor E. WILSON: I do not remember having experimented upon an economical lead traction type cell weighing 21 lbs., and capable of discharging in about five hours more than 7·43 watt-hours per lb. of gross weight.

Mr. Hall. Mr. CUTHBERT HALL: We have heard several times to-night about the watt-hours per lb. of cell. It seems to me, for the purpose of scientific comparison, it is rather important to point out what the exact size of the cell is, because, obviously, if you take a cell of one positive and two negatives and acid in proportion, and give the watt-hours per lb. of cell of that construction, you get a less good result than you do if you take, say, seven positives and eight negatives, because the proportion of useless negative to positive is greater in the first instance than in the second. There are only two or three points in this paper to which I intended to refer. The first is with regard to charging at constant potential. Mr. Epstein says that the Société des Moteurs of Paris is charging the cells at constant potential, and he mentions that this method of charging is advantageous, because it forms what is practically an automatic method of preventing over-charging. I think it is rather important to point out also, that charging at constant potential gives a more efficient result than charging at constant current. I have noticed that if you put exactly the same number of watt-hours into two cells of similar construction, first at constant potential and then at constant current, you find the efficiency is higher at constant potential than at constant current. I attribute this to the fact that, when charging at constant current, at the end of the charge a very large amount of the energy is expended in producing what is commonly called "gassing;" whereas, if you charge at constant potential, consequently allowing your current to drop, very little "gassing" goes on, and the plates are capable of absorbing the comparatively low current which we get at the end of the charge. When

charging at constant current, the current-density seems to be too great for the cells, when almost fully charged, to take up. The next point is with reference to Mr. Epstein's remark: "As is well "known, the capacity of a plate is determined by the volume of "active material." It appears to me that this statement requires considerable qualification. It would lead one to suppose that in the case of two plates of similar dimensions as to length and breadth, but one plate of twice the thickness of the other, the thick plate would give double the capacity of the thin plate. This is contrary to my experience. I made a series of rather elaborate experiments in this connection some years ago, and I found that if one took a plate, say, 10 inches in length by 10 inches in width, and 5-16th inch in thickness, and then a similar plate of double the thickness, the output from the thin plate was just as great as from the thick plate. Therefore, this statement that the capacity of the plate is determined by the volume of active material is not correct, as there is a certain thickness of active material which the electrolytic action will not penetrate. This, I think, brings us to one of the most important considerations in connection with accumulators. To my thinking the ideal accumulator is one which has such a thickness of active material that nearly the whole amount undergoes chemical change at each charge and discharge, and which also has a conducting plate or grid of just sufficient thickness to conduct the current to the active material without much loss due to resistance. In most of the plates on the market only a very small proportion of the active material undergoes complete chemical change on charge and discharge; and this is more or less proved by the fact that if you take the output which is theoretically possible from a cubic inch of active material, say peroxide of lead, you find it is about three times as much as you get from the same quantity of active material on the very best types of plate on the market. If you estimate the active material on one of the best plates on the market, and calculate what output should theoretically be obtainable from it, you find that it is about three times the output which is actually obtainable. I am quite aware that it is necessary to have a conducting frame, and that that is so much loss, Mr Hall.

Mr. Hall. but, in addition to that, there is a great deal of loss due to the fact that there is a lot of active material which does not undergo chemical change. The only other point I wish to refer to is the remark Mr. Epstein makes about the negative plates, which I very cordially endorse. I notice that if you take almost any cell and discharge it, and then divide the positive and negative plates, and put with the negatives fully-charged positives, and with the positives fully-charged negatives, you find it is the negative plate which has given out first, and not the positive.

Mr. Raworth. Mr. J. S. RAWORTH: I appear to-night in the character of one who wishes to learn, and in that capacity I think I represent the majority of members present.

I notice from the reports in the papers, that, at the last meeting, the question was raised as to the effect of vibration on the plates of accumulators, and Mr. Manville was so good as to bring before the Institution the results of some experiments which he had made, and to which it was demurred that they were not conclusive.

I think most of you know—and, if you do not, I will tell you—that there is a case on record, and it has been constantly on record for the last eight or ten years, which absolutely settles the point. I refer to the cells which have been, and are being, used by the Brighton and South Coast Railway Company for lighting their carriages. Those cells are exposed to the most violent shaking that is possible. Yet, it is a well-known fact that those accumulators have not deteriorated by vibration.

This case is not complicated by the question of pulling the accumulators in and out, because they are all fixed, and, therefore, if there were any deterioration it would be entirely traceable to the vibration and not to careless handling.

Mr. Swan. Mr. J. W. SWAN: I have listened to this discussion with exceeding interest, and I share with Professor Ayrton the earnest desire to find that the obstacles which stand in the way of the accumulator being made useful for road traction are not insurmountable. I confess to feeling reassured in reading over Mr. Epstein's paper, remembering how large his experience is, not only in the manufacture, but in the use of secondary batteries. It is

satisfactory to find him pointing out causes of failure in reference to the Birmingham experiment, and expressing the opinion that most of these were of a remediable kind, such as imperfect mechanical arrangements which necessitated movement of cells, and other objectionable treatment that contributed to the non-success of that experiment. I concur with him in his remarks on charging cells. The method of charging by constant potential rather than uniform current is, I think, correct in principle, and is a step towards making the accumulator more useful in connection with motor traction, since it shortens the time of charging so considerably. There was a point brought up in the discussion that struck me as worthy of notice—the fact that 16 years have elapsed since the invention of the Faure cell. At that time hope seemed to be almost given up of the possibility of the Planté type of cell competing with the Faure type. Now we have the Planté type again coming to the fore, and more than that, appearing to have shown superiority in practice, more particularly in connection with the formation of the positive plate as opposed to the Faure method of formation. Mr. Manby, if I have not misunderstood him, has referred to the cell used in connection with the Dresden traction as of the Faure type, and with the Faure principle carried to an extreme; that is to say, the plate is almost entirely composed of a paste forming the active material—a cell, in fact, of the Lithanode type. It is remarkable that, notwithstanding the experience of those 16 years, we are still debating the point whether the Faure type or the Planté type is the better. I notice that some of the largest British makers of batteries have apparently come—as the result of long experience—to the conclusion that, after all, the Planté method of formation for the positive plate is the better. Whatever credit or honour there is in connection with the origination of the idea of plates composed, the + wholly of lead peroxide and the − of spongy lead, is due to Tribe. Alfred Tribe was the colleague of Dr. Gladstone in making researches into the action of the secondary battery, and, with Dr. Gladstone, published the best explanation of its action. It ought to be remembered that it was Tribe who first devised and constructed a cell of the kind referred

Mr. Swan.

Mr. Swan. to by Mr. Manby. I entirely agree with Mr. Epstein when he says: "In any estimation of the cost of maintenance the only safe "course is to assume complete renewal of the plates which from "any cause have become inefficient, rather than to reckon on the "doubtful economy of patching up." I hope that this paper and discussion may assist towards the attainment of more successful results in connection with motor traction by means of secondary batteries.

Mr. Carter. Mr. E. Tremlett Carter: I should like to add one word to the remarks I made on the last occasion. Professor Ayrton has raised a point of importance as to the advisability of using a series-parallel arrangement for the cells rather than for the motor fields and armatures. Since the last meeting I have inquired into this matter, and I find that the method of rearranging the cells has on many occasions been adopted for traction work. Only a few days ago I came across a motor car now running about the streets of London having a series-parallel controller on it, by which the 40 cells are arranged in groups of 10. The car is started with four groups in parallel. The cells are then put 20 cells in series in parallel with the other 20, and they are then thrown all in series. The arrangement works admirably, I am told, and certainly, from what I could see, it is very effective. I have made no test to ascertain whether it is *efficient*, but it is a very *effective* way of starting. No resistances are used, and, besides giving low volts for starting, intermediate volts for intermediate speeds, and high volts for rapid speeds, the method has the further advantage that the excessive current on starting a car is not sent through all the cells, but is subdivided amongst groups of cells. At starting, you may have from 2 to 2½ times the normal current through the motor, but if you have four rows of cells in parallel instead of all the cells in series, then when you start the car each cell will not have as much as its normal current, but will have something considerably under its normal current. The name of the car I have referred to is the Headland's motor car. It is equipped with the Headland's storage battery.

Mr. Smith. Mr. M. Holroyd Smith: I should like to accord my thanks to Mr. Epstein for the very interesting paper he has given us.

There is a savour of strict honesty running through the paper Mr. Smith. that is most refreshing and encouraging, and I hope it will be followed by a good many contributors of future papers. He has not hesitated to tell us where he has had difficulties and where he has had failures. He has not brought to us here only his successes. The paper reminds me of a little incident which took place when I was a boy at school. A certain lady offered a prize to the boy who could get most arrows in the bullseye on a target. We shot all the arrows that were given to us, and no boy got a single arrow into the bullseye. The lady was generous, and gave the prize to the one who got the arrows nearest the bullseye. Now, I do not think that anybody has been able to hit the bullseye of success in the use of accumulators for traction work, but, as far as I have been able to watch the exploits of the various people who have patiently, studiously, and laboriously persevered in this work, I am certainly of opinion that Mr. Epstein has come nearer to the mark than anyone else.

I wish to make one or two remarks respecting secondary batteries, because, like many other engineers, I am deeply interested in the subject at the present time. If we could only get a battery to fulfil our hopes, then, of course, steam and petroleum, and all the other things, would have to go to the wall; but, unfortunately, we have the important question of economic user, which has to be brought in, as well as the mere question of luxury and comfort. I have had occasion to investigate several batteries lately, having been asked, as far as possible, to aid in the exploiting of certain batteries by gentlemen who had a wonderful invention and wished to sell it to some large financial company. Perhaps it is an unnecessary precaution, but I wish to emphasise the statement that the real value of no battery can possibly be ascertained by a few tests. Only time and use can prove its duration and fitness to stand the strain and hardship it must be subjected to when employed for traction purposes on common roads. Another battery was offered me only last summer, with a great amount of laudation by the person who possessed it. Summing up all the statements made concerning it, and putting them into practical shape, the battery was to give

Mr. Smith. 2 effective H.P. for 2½ hours, and the total weight was to be under 300 lbs. I will not dispute that such a thing is obtainable for a few trial runs, but I doubt endurance. I said distinctly, "Even admitting you can accomplish this result, how long will "your battery last?" and, having no evidence on this point, I positively declined to give any report for his benefit, or for the benefit of anyone else, unless he could show that there was the question of duration over at least 12 months' regular work. I take it that what we say in these societies—the gist of it, at any rate—is not confined to ourselves, and I wish, therefore, to make a distinct caution to those who do not understand these electric questions, but who are carried away with what is everybody's desire, viz., the hope that the battery of the future is close at hand,—I wish to repeat the caution that when anything is brought for consideration, this question of endurance must form one of the first items for the consideration of any user or purchaser. There is another point in reference to the batteries with regard to which I also wish to caution both the sellers and the purchasers or users. In the batteries that have been offered to me for traction work, it has been always with a statement that so many miles' run can be obtained from them. Now, that is an exceedingly rash statement for any one to make, because, when you say that so many car miles are to be obtained from a particular weight and size of battery, that presupposes the road upon which you are going to travel, the rate at which you are going to travel, and the number of stoppages which are going to take place in the time. My caution, therefore, is this: that no maker of batteries should make any statement whatever as to the car miles that his battery will produce. Let the statement merely be confined to the discharge of that battery at varying rates of discharge, and to the duration of that battery and its ability to stand rough handling, and let it devolve upon the user—the engineer who takes the construction of the carriage in hand—to see how much mileage can be obtained. It is obvious that you may take the very best battery that is produced and apply it to a car where a bad motor or indifferent gearing was employed, or where the coefficient of friction for

traction was very high. Such circumstances would be against the interest of the battery. On the other hand, the motor and gearing may be as near perfection as possible, in which case an indifferent battery would show to advantage. Therefore, let the engineer specify what power he requires out of the battery, and the conditions under which it has to work, and then himself become responsible for the motor gearing and all construction work. Of course, those who sell the car to the user must make some statement as to the distance it is likely to run on ordinary roads, and for how long it will go without recharging. Another caution I would utter to those who are going to use batteries. I agree with Professor Ayrton in his doubts as to the tractive force necessary for road cars. In all my calculations on this matter, instead of being content with 40 or 60 lbs. per ton, I never make it less than 100 lbs. per ton, and I believe that will in practice be very much nearer the mark than the low figures given to us by some of the speakers. (I am discussing road carriages now, not tramway work.) You have not only got to consider the force required to maintain the movement of the carriage at a certain speed, but you have to consider the force required for starting from a state of rest and attaining that speed. The power required for acceleration is more than one would at first suppose, especially when running in the streets of London, where you have to be starting and stopping over and over again on account of the traffic. Therefore, when making calculations as to the tractive force required to propel a carriage at a certain rate, it is better to base the estimate on 100 lbs. per ton than on lower figures. Mr. Smith.

Referring to another remark of Professor Ayrton, I understood him to advocate that it would be very much better to control the speed of a motor carriage by using the batteries in parallel and series, instead of making any attempt at using the armature and fields of a motor in parallel and series. I do not say that Professor Ayrton is wrong, but I do not agree with him at present. Following the experience of others using batteries, gentlemen well known to you, most of them, it was found that in the early days of tramway work the attempt to vary the speed and power by varying the coupling of the batteries was never satisfactory or successful.

Professor AYRTON: Why not?

Mr. Smith. Mr. M. H. SMITH: Because they were always having varying discharges from various cells of the batteries. You never maintain the cells at the same strength, so I am informed, and so I should suppose. They have been used in various groups, sometimes one group, sometimes another, sometimes all in series, or all in parallel, or various changes between.

Professor AYRTON: You should always use every cell of the car.

Mr. M. H. SMITH: That is the only safe way as far as the duration of the battery is concerned, but you have not changed the power of the battery; the watts remain the same. And you must remember that it is not only the speed, but also the power of the motor that has to be varied, and, unfortunately, the greatest power is required when the car is running slowest, viz., when mounting a steep hill. At present we are assuming no change of gearing, and, as far as my experience goes, instead of changing the speed by varying the combination of the cells, it is preferable to employ a change in the combination of the windings of the motor. By employing a dual armature motor the possible combinations are increased, and you gain a very important and practical advantage, one that I have used with no small amount of success, viz., you are not only able to vary the power of the motor, but also to dispense with differential gear in driving the car by having the two driving wheels separately driven by the two armatures. A mechanical advantage is therefore gained, as well as an electrical one. It is a matter we can hardly conclude to-day. Still, I venture to disagree with Professor Ayrton on that point, for, so far as my experience goes, it is preferable to employ the variation in the winding of the motor instead of in the groupings of the battery cells.

Mr. Brown. Mr. J. BROWN: I just wish to direct attention to the fact that the difference of opinion in regard to the tractive force may be largely due to the difference in the quality of the roads on which the force has been measured. Experiments on this tractive force were made on stage coaches a long time ago. Of course, the wheels were different from our modern wheels. These experiments direct attention to two points, viz., that the force varies in some

degree with the velocity at which the carriage runs, and that it depends upon a constant, which constant varies from two on very good wooden pavements such as you have in London to 14 on very inferior roads such, perhaps, as those we have in Ireland. Indeed, if anyone in Ireland invents a motor car to run on the roads there, he may be perfectly sure that it will run everywhere. However, necessity is the mother of invention, and if the bad roads in Ireland have done nothing else, they have given you the Dunlop tyre. Mr. Brown.

In a little book on "Roads and Streets," one of Weale's series, the tractive force is given as from 40 to 80 lbs. per ton, if I remember rightly, on a macadamised road, but what kind of macadamised road this particular one was is not stated. I quite agreed with what Professor Ayrton said, that the power at present calculated for electric motor cars is too small, and, indeed, that has been the case with all kinds of motor cars. In France the earlier petroleum cars when intended to carry four persons were provided with 3¾ H.P., but now they are driven, I think, with 5 H.P. engines. The Panhard and Lavassor car that won the race to Bordeaux had, I believe, a 7-H.P. engine. There is just one other point which I should like to touch upon. As a would-be designer of a carriage for private use, not for running on the streets all day for hire, I would point out that what we want is a battery that will give us a very large discharge for a comparatively short time. We do not want a battery that will last for over five hours, since it is only on very exceptional occasions that anyone wants to drive in a private carriage for five hours. One or one and a half hours' actual running would be usually quite sufficient. If we could get a cell weighing, say, 30 lbs. to give 80 or 100 amperes for 1½ hours, such a cell would, I think, be more suitable for use in private carriages than those usually met with, where the same charge must be spread over five hours in a current of 20 or 30 amperes only.

The PRESIDENT: Mr. Epstein informs me that it would be quite impossible for him to reply to-night to all the criticisms on his paper—there are still some other members who desire to speak. The subject is one of the most important which have The President.

The President. come before us for discussion this year, and I am sure you will all agree with me that it would be better to adjourn now and devote another evening to it in December; meanwhile I would remind you that, so far as we have gone, nearly the whole of the discussion has turned upon the question of batteries. There are many other points connected with the subject regarding which we should like to hear something. Would Professor Smith kindly give us some particulars about gearing at the adjourned discussion? We should be very much obliged to him if he would. I would also suggest that, if members can induce any of their friends who are qualified to speak to give us the benefit of their experience regarding other parts of the system which are almost, if not quite, as important as the storage cells, it would probably enhance the value of the paper and the discussion to which it has given rise.

I have to announce that the scrutineers report the following candidates to have been duly elected:—

Member:

James Loftus Owen.

Associates:

Fred Beanland.
Hugh Marriner Brigg, B.A.
William Pollard Digby.
James Enright, B.Sc.
John William Flower.
A. Goodsir.
Herbert C. Gunton.
Charles Frederick Parkinson.
Walter George McMillan, F.I.C.
Arnold Philip, B.Sc., F.I.C., Assoc. R. Sch. Mines.
R. W. L. Phillips.
Robert Cecil Pierce.
Walter Wood (Captain).

Students:

Keppel Archibald Cameron Creswell.
Alfred Sherwood Esslemont.
Mendel Finkelstein.
Herbert A. F. King.
Gordon Layton.
Alexander W. Norris.
Hugh Almack Pearson.
Clement T. Stephenson.
Andrew Cyril Weber.

The Twenty-sixth Annual General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, December 9th, 1897—Sir HENRY MANCE, C.I.E., President, in the Chair.

The minutes of the Ordinary General Meeting held on November 25th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfer was announced as having been approved by the Council, viz.:—

From the class of Associates to that of Members—

Mr. Robert Arthur Smith.

Mr. D. Cromartie, Member, and Mr. W. P. Whitehead, Associate, were appointed scrutineers of the ballot for new members and for the election of Council and Officers for the year 1898.

The SECRETARY read the Annual Report of the Council, as follows:—

REPORT OF THE COUNCIL TO THE ANNUAL GENERAL MEETING, 9TH DECEMBER, 1897.

ELECTIONS AND TRANSFERS.

The total of the additions to the register during the year has been 216, comprising 3 Foreign Members, 17 Members, 120 Associates, and 76 Students; and 53 candidates have been approved for ballot next month.

24 Associates have been transferred to the class of Members, and 82 Students to the class of Associates.

DEATHS AND RESIGNATIONS.

The losses sustained by the Institution during the year through death amount to 22, viz.:—2 *Honorary Members*—Mr. Jacob Brett and Dr. Von Stephan; 2 *Foreign Members*—Dr. W.

P. Brix and G. Verrier; 6 *Members*—John Aylmer, W. S. Graff Baker, W. H. Blakeney, W. W. Cargill, H. C. Forde, and W. Moir; 10 *Associates*—J. Auchinachie, F. V. Andersen, L. H. Chase, Paul Dimier, Captain A. B. Hawes, F. Lilley, E. Oldroyd, A. E. Pond, G. H. Thornton, and T. B. Webber; 2 *Students*—Edward Ray and C. Yatman.

Special allusion has previously been made to Mr. Jacob Brett, whose name must always be remembered as that of one of the very earliest pioneers in telegraphy, and as one of the founders of submarine telegraphy; to Dr. Von Stephan, who, in his position of Postmaster-General of Germany, did so much for telegraphic science; and to Mr. John Aylmer, one of the most zealous and indefatigable of the Institution's Local Honorary Secretaries.

Mr. H. C. Forde was one of the original members, and a former Member of Council. His name was very prominently connected with submarine telegraphy, and his death was deeply deplored by all who knew him.

Two Foreign Members, 2 Members, 20 Associates, and 6 Students have resigned during the year.

Papers.

In addition to the very interesting inaugural Address of the President, the following papers have been read during the year:—

Date. 1897.	Title.	Author.
Jan. 28.	—Electrically Interlocking the Block and Mechanical Signals on Railways ...	F. T. Hollins, Member.
Feb. 25.	—The Relative Size, Weight, and Price of Dynamo-electric Machines	E. Wilson, Member.
Mar. 11.	—On some Repairs to the South American Company's Cable off Cape Verde in 1893 and 1895	H. Benest, Associate.
April 8.	—Some Recent Developments in Electric Traction Appliances	A. K. Baylor.
,, 22.	—The Generation of Electrical Energy for Tramways...	J. S. Raworth, Member.
May 13.	—The Disturbance of Submarine Cable Working by Electric Tramways ...	A. P. Trotter, Member.
,, 27.	—Dynamoes	W. M. Mordey, Member.
Nov. 11.	—Accumulator Traction on Rails and Ordinary Roads	L. Epstein, Member.

Annual Premiums.

In respect of papers read during the session 1896–97, the Council make the following awards, viz.:—

The "Institution Premium," value £10, to Mr. W. M. Mordey, Member, for his paper entitled "Dynamoes."

The "Paris Electrical Exhibition Premium," value £5, to Mr. John Gavey, Member, for his paper on "The Telephone "Trunk Line System in Great Britain."

The "Fahie Premium," value £5, to Mr. Benest, Associate, for his paper on "Some Repairs to the South American Company's "Cable off Cape Verde in 1893 and 1895."

They also award an Extra Premium of £5 to Mr. A. P. Trotter, Member, for his paper on "The Disturbance of Submarine Cable "Working by Electric Tramways."

The "Students' Premium," value £3 3s., they award to Mr. P. S. Sheardown for his paper entitled, "Notes on Electric "Tramway Traction;" and they further award two Extra Premiums of £2 2s. each to Mr. F. Johnston for his paper on "Accumulators," and Messrs. R. M. Sayers and S. S. Grant for their paper on "The Working of Long Submarine Cables."

The Council desire to remind members of the increase which they have decided to make in the number and values of the Annual Premiums, commencing with the session 1897–98, viz.:—

The "Institution Premium" to be increased from £10 to £25.

The "Paris Electrical Exhibition Premium" to be increased from £5 to £10.

The "Fahie Premium" to be increased from £5 to £10.

Two additional Premiums for original communications not read, but accepted for publication in the Journal of the Institution—viz., one of £10 and one of £5.

For papers by Students, read at the Students' Meetings—One Premium of £10, two Premiums of £5 each.

The Willans Premium.

Looking to the fact that the award of this Premium is, according to the Trust, to be made triennially, your Council,

by whom the first award is to be made, have considered themselves justified in selecting for such award any one of the papers on the subject specified in the Trust Deed, which have been communicated to the Institution since December, 1894, the date of the Trust being January, 1895; and they accordingly award the Premium, the value of which they have increased to £25, to Mr. Mark Robinson, Member, for his paper entitled, "On the "Recent Development of the Single-Acting High-Speed Engine "for Central-Station Work," read May 9th, 1895.

Salomons Scholarship.

The Council have awarded a Salomons Scholarship of £50 to Mr. Edward Ernest Tasker, a Student of the Technical College, Finsbury.

Students' Class.

The Council are glad to be able to report that the improvement in the merits of the papers read at the Students' Meetings to which they alluded last year, was sustained during the past session.

Visits to the following places were arranged for the Students by your Secretary during the year, viz.:—

The Works of the Electric Welding Company.
" " of the India-Rubber, Gutta-Percha, and Telegraph Works Company.
" " of Willans & Robinson.
The Stations of the City and South London Railway.
The Central Electric Lighting Station, Islington.
The Electric Lighting Works of the Great Eastern Railway.

The Council have pleasure in acknowledging the facilities thus kindly afforded to the Students of visiting works of so much interest to them.

Wiring Rules.

The new Wiring Rules as finally approved by the Council were issued at the end of July last.

The revision of the former rules was carried out by the Technical Committee, comprising the whole of the Council and some other members whose experience was such as to render their advice and assistance of special value.

The Committee and its Sub-Committees held many meetings, and obtained, both by personal interviews and by correspondence, very valuable information and opinions, not only from municipal engineers and the engineers of electric lighting and power stations, but from the leading manufacturers of cables and apparatus.

The rules consequently are based upon the experience and knowledge of representatives of all classes in the electrical profession and the electrical industry, and can scarcely fail, therefore, to be of material service to all who are engaged or interested in the employment of electric energy for lighting, heating, or motive power, &c.

Although in certain respects somewhat more detailed than the old rules, they are not intended to take the place of specifications drawn up by consulting engineers, or to be regarded as other than an enunciation of the general principles which should be observed in the wiring for the supply of electrical energy.

Annual Dinner.

The Annual Dinner was this year held at the "Hotel Cecil," on the 24th November, and the number of members present was larger than usual.

Annual Conversazione.

The Conversazione this year was, by permission of the Trustees of the British Museum, held in the galleries of the Natural History Museum, South Kensington, and was very numerously attended. Much of the success of the entertainment was due to the temporary installation of the electric light for the occasion by the Kensington and Knightsbridge Electric Lighting Company, who, at the request of the Council, kindly undertook to carry out the work, at a minimum of cost to the Institution. Your Council are glad of this opportunity of acknowledging their indebtedness to their colleague Mr. H. W. Miller, the engineer to the company, for the great amount of trouble taken by him personally in respect of the work, which had to be carried out under considerable difficulties.

Building Fund.

The amount standing to the credit of this account at the date of the last Annual General Meeting was £3,528 4s. 2d. This year the Council, in pursuance of the desire unanimously expressed by the members that the fund should be augmented from time to time as far as the finances will permit, have transferred to it from the General Fund a further sum of £1,000 out of the surpluses of 1895 and 1896, which sum, together with dividends and income tax recovered, brings the total amount standing to the credit of the fund on the 30th September last up to £4,639 13s. 7d., of which £63 15s. 11d. remains to be invested.

Annual Accounts and Financial Position.

The Statement of Accounts and Balance-Sheet for the year ending 30th September, 1897, shows that the financial position of the Institution continues sound and satisfactory, the excess of Receipts over Expenditure during the year being £1,080 11s.

A further amount of £185 1s. 9d. has been invested on account of Life Compositions, leaving at the date up to which the accounts were made a balance of £131 6s. 5d., which has since been also invested.

The Council have the satisfaction of reporting that the amount already received in respect of subscriptions outstanding on September 30th, 1896, exceeds the estimate made in the last Annual Report, viz., £532.

The estimated realisable amount of subscriptions outstanding on the 30th September, 1897, is £555, and none of this is taken into account in the Balance-Sheet now presented.

The Secretaryship.

On the 25th March last, your President announced that Mr. Webb desired to retire in February next from the office of Secretary, which he will then have held for a period of 20 years.

The applications received in response to the advertisement inserted in the technical journals for a successor to Mr. Webb were 48 in number, and were very carefully considered by a Committee specially appointed by the Council; and certain of the

candidates whose qualifications appeared to best meet the requirements were personally interviewed by the Committee in the first place, and subsequently by the Council, who finally selected Mr. Walter George McMillan, F.I.C., F.C.S., late Lecturer on Metallurgy at Mason College, Birmingham.

Although Mr. McMillan will not actually take office until the 12th February, his engagement commenced on the 1st October, so that he might have ample time to make himself acquainted with the duties of his office.

The Council feel that they would be failing in their duty were they to omit to place on record their high appreciation of Mr. Webb's services to the Institution during the best years of his life. While they respect his single-mindedness, his high personal character, and those qualities which have made every member a friend, they deeply deplore the loss of his services.

THE LIBRARY.

REPORT OF THE SECRETARY.

I beg to report that the accessions to the Library during the year numbered 55; of these, 1 was purchased, the remainder having been kindly presented either by the authors or the publishers.

The specifications of all electrical patents continued to be supplied to the Institution, by the kindness of H.M. Commissioners of Patents, who have also been good enough to present the abridgments published by the Patent Office of specifications relating directly or indirectly to electricity and magnetism.

The number of patents applied for this year up to November 24th was 27,302, of which 1,517, or 5·55 per cent., were electrical.*

The corresponding numbers last year were 26,425 and 1,314, or 4·97 per cent.

The periodicals or printed proceedings of other Societies received regularly are, with some additions, the same as last year, as may be seen by the list appended hereto.

* Up to December 31st the number applied for was 30,553, of which 1,736, or 5·68 per cent., were electrical.—Sec.

The number of visitors to the Library to the end of November has been 725, of whom 50 were non-members.*

The corresponding numbers last year were 660 and 76 respectively.

I am glad to be able to report that, owing to the kind assistance of Mr. McMillan, the card subject catalogue of the Library is now fast approaching completion.

F. H. WEBB,
Secretary.

APPENDIX TO SECRETARY'S REPORT.

TRANSACTIONS, PROCEEDINGS, &c., RECEIVED BY THE INSTITUTION.

ENGLISH.

Asiatic Society of Bengal, Journal and Proceedings.
Cambridge Philosophical Society.
Engineering Association of New South Wales.
Greenwich Magnetical and Meteorological Observations.
Institute of Patent Agents, Transactions.
Institution of Civil Engineers, Proceedings.
Institution of Mechanical Engineers, Proceedings.
Iron and Steel Institute, Proceedings.
King's College Calendar.
Liverpool Engineering Society, Proceedings.
Northern Society of Electrical Engineers, Proceedings.
Physical Society, Proceedings.
Royal Dublin Society, Transactions and Proceedings.
Royal Engineers' Institute, Proceedings
Royal Institution, Proceedings.
Royal Meteorological Society, Proceedings.
Royal Society, Proceedings.
†Royal Society, Philosophical Transactions.
Royal United Service Institution, Proceedings.
Society of Arts, Journal.
Society of Chemical Industry, Journal.
Society of Engineers, Proceedings.
University College Calendar.

AMERICAN.

American Academy of Science and Arts, Proceedings.
American Institute of Electrical Engineers, Transactions.

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* Up to December 31st the numbers were 783 and 53 respectively.—SEC.

† Presented by Professor D. E. Hughes, F.R.S. (Past-President).

Canadian Society of Civil Engineers, Transactions.
Franklin Institute, Journal.
John Hopkins University Circulars.
Library Bulletin of Cornell University.
Ordnance Department of the United States, Notes.
Technology Quarterly.

DANISH.

Den Tekniske Forenings Tidsskrift.

FRENCH.

Association des Ingénieurs Électriciens sortis de l'Institut Électro-Technique Montefiore, Bulletin.
Académie des Sciences, Comptes Rendus Hebdomadaires des Séances.
Société Belge d'Électriciens, Bulletin.
Société Française de Physique, Séances.
Société des Ingénieurs Civils, Mémoires.
Société Internationale des Électriciens, Bulletin.
Société Scientifique Industrielle de Marseille, Bulletin.

LIST OF PERIODICALS RECEIVED BY THE INSTITUTION.

ENGLISH.

Cassier's Magazine.
Electrical Engineer.
Electrical Review.
Electrician.
Electricity.
Engineer.
Engineering.
English Mechanic and World of Science.
Illustrated Official Journal, Patents.
Indian and Eastern Engineer.
Industries and Iron.
Invention.
Lightning.
Nature.
Philosophical Magazine.

AMERICAN.

Electrical Engineer.
Electrical Review.
Electrical World.
Electricity.
Journal of the Telegraph.
Physical Review.
Scientific American.
Street Railway Journal.
Western Electrician.

FRENCH.

Annales Télégraphiques.
L'Éclairage Électrique.
L'Électricien.
L'Industrie Électrique.
Journal de Physique.
Journal Télégraphique.

GERMAN.

Annalen der Physik und Chemie.
Beiblätter zu den Annalen der Physik und Chemie.
Electrotechnischer Anzeiger.
Electrotechnische Zeitschrift.
Verhandlungen des Vereins zur Beförderung des Gewerbfleisses.
Zeitschrift für Elektrotechnik.
Zeitschrift für Instrumentkunde.

ITALIAN.

Giornale del Genio Civile.
Il Nuovo Cimento.

The PRESIDENT: I have now to move the adoption of the Report, and ask your permission to have it included in the Proceedings of this Institution. In doing so I shall only detain you with one or two brief remarks. I think you will agree with me that the Report is very satisfactory, and that our progress during this last 12 months has been equal to that in any previous year. I should like to refer for a moment to some of the suggestions that were made at our last Annual Meeting. The first of those was that we should hold our Conversazione in more convenient rooms. I think there can be no question that the change made by the Council was very much for the better. It was also suggested that there should be exhibits. With regard to that point, I may inform you that the special committee appointed to consider the subject decided that the attractions of the Natural History Museum were so great that to bring anything fresh there would on this occasion be quite unnecessary.

Some remarks were made at our last Annual Meeting about the financial and sessional years not corresponding. I would remind you that we have only recently altered the date when the financial year should terminate. Some advantage might possibly be derived from these periods being made to correspond; but to do that we should probably have again to modify the Articles of

Association, and the point will be borne in mind when that contingency arises.

Another suggestion made was that the supply of papers issued to the members should be more plentiful, and issued with less delay. Well, gentlemen, during my year of office I have seen how difficult it is to get the papers available for distribution as soon as we should wish. It is the fault to a great extent of those who contribute papers. Naturally, they wish to keep them as long as possible, in order to add those finishing touches which are sometimes the most effective; but I may say this: When the proofs are corrected we distribute them as soon as possible to all those whom the author suggests, or who apply for them, or who we think would feel a special interest in the subject dealt with in the paper, and are therefore likely to come here and discuss it. Moreover, there is a plentiful supply of papers available for members who attend the meeting. I do not think we can do anything more in that respect. Another suggestion made by a member was that we should endeavour to affiliate with this Institution any Societies or branches which might exist or come into existence in other countries, for similar objects to our own. Many of us would like, if we could only see our way, to extend our operations in this direction; personally, I may say I should like to see the principle applied as soon as possible to branches established nearer home; but we feel that it would not be wise to move in the matter until the alterations which would be necessary in the Articles of Association have been well considered.

I have ventured to interpolate these remarks as it is right you should understand that we are glad to receive and consider any suggestions from members. I shall be glad to hear any remarks on the present Report, and I now move—"That the Report of the "Council, as just now read, be received and adopted, and that it "be printed in the Journal of Proceedings."

Mr. J. HOOKEY: I have much pleasure in seconding the motion for the adoption of the Report.

Mr. CHARLES BRIGHT: Believing that in the present day there is, if anything, rather a *plethora* of papers for this Institution, and in view of what—for want of a better word—I will call the

increased pay attached thereto, it occurs to me that this is an excellent opportunity to again bring forward the suggestion that at our meetings all papers be taken as read. It is not everyone who has the gift of reading his own paper, however excellent it may be in composition Again, there are many of us who come here to pick up information on subjects foreign to our own particular branch of electrical work, and it seems to me we should derive more comfort, as well as benefit, if the discussion alone was to be listened to. I quite foresee that there are many difficulties. Besides advance proofs of a paper being circulated in every quarter in which it is likely to be appreciated, a copy would require to be lodged in the Library for at least a week beforehand, as is done at the Institution of Civil Engineers. Moreover, I should be glad if arrangements could be made with the technical Press for publication of the paper a week or more in advance. By this means not only should we get through more papers in a session, but the discussion would be of a higher standard and of more permanent value. In my opinion no one can do proper justice to a paper without a quiet study of it beforehand. The circulation of proofs at the time of reading is of no use whatever; indeed, the turning over of leaves is only a source of disturbance to those who are trying to listen.

Would it not also be possible to take some steps for obtaining the views—for record, at any rate—of those of our members who reside abroad? We have lately followed the example of our parent, the Institution of Civil Engineers, in securing the services of provincial members, to represent their interests, on the Council. When the foundation of this Institution is considered, I think it will be seen that we are peculiarly, and still more, identified with those members holding official positions abroad. It must be remembered that by far the larger proportion of our telegraphic members—to whom this Institution originally owed its existence—rank amongst these.

Mr. C. H. WORDINGHAM: Perhaps I might be allowed to offer a few remarks on this subject, because in the Municipal Electrical Association we have already adopted the system of dealing with papers which has been recommended by Mr. Bright.

There is a great deal to be said for it where you have a large number of papers to get through. In our case we had three in one morning; but where you have only one paper to consider and discuss, I very much doubt whether the advantages balance the disadvantages. I think there can be no doubt that when the paper is not read at the meeting, but is simply circulated beforehand, a very large number of members—probably the majority—put off reading the paper until some convenient time—a long railway journey, perhaps—and as a result it never gets read at all. They arrive at the meeting, and the discussion begins, and a very large number of those present have not an idea what the discussion is about. Therefore I do not think the suggestion should be adopted in a hurry. As to the publication in the technical Press beforehand, I think it would rather take from the prestige of the Institution. Again, with regard to the convenience of members abroad, from what Mr. Bright said it would seem to necessitate publishing the paper three months beforehand, and everybody would have forgotten all about it by the time we came to discuss the paper.

Mr. W. Langdon: I support that which has fallen from the last speaker. Although I quite recognise the many advantages which would attend the proposal of Mr. Bright, I yet think it would involve much inconvenience to members who may come here purely with the object of hearing what is passing, and who may not be in possession of a copy of the paper under consideration. In such circumstances they would be at a loss to know what the discussion was about. The proposal is one which, in my opinion, should, before being adopted, be most carefully considered.

Mr. Bright: I should like to mention an alternative, and that is, that all papers be read by the Secretary. This has been the practice at the Institution of Civil Engineers for a long time. I venture to think a uniform style of reading furnishes a greater degree of comfort to the regular listener, besides increasing the chances of the paper being understood. Perhaps then also long tables and formulæ could be held back and reserved for the *Journal*.

Mr. A. A. C. Swinton: I believe at the Institution of Civil Engineers the papers are all read by the Secretary.

Professor PERRY: And I think that is a great mistake. I think it is a mistake to lose the personality of the author.

Professor AYRTON: The fact is well known.

The PRESIDENT: The point which Mr. Bright has brought forward is one which has been very carefully considered by the Council. There is no hard-and-fast rule on the subject. There have been occasions when a paper has been considerably abridged in the course of reading: we have to be guided by the wish of the author, the nature and length of the paper, and the time available for discussion. When an author reads his own paper he is able to interpolate remarks which are frequently both interesting and instructive. On the whole, I think it would be better to leave it an open question. There are certain papers which it would be better to take as read, and others which we should be very sorry to miss the opportunity of hearing. Many, like myself, have not the time to read and consider papers beforehand, and we come here for a quiet evening to listen to the author's expression of his views.

The resolution that the Report be adopted was agreed to *nem. con.*

The PRESIDENT: I have now to move—"That the Statement "of Accounts and Balance-Sheet for the 12 months ending "September the 30th, 1897, as just presented, be received and "adopted."

I presume that, in accordance with the usual practice, you will take them as read, a copy having been forwarded to all of you a week ago. It is quite unnecessary for me to go through them in detail; they show a substantial increase in our funds, which no doubt you will regard as satisfactory. It may be desirable to draw the attention of members to the fact that the balance-sheet now submitted covers a period of 12 months, whereas the previous accounts, passed at our last Annual Meeting, included nine months only; consequently it is rather difficult to compare the one with the other. If we take the accounts of 1895 for the purpose of comparison, we must bear in mind the fact that since that date there has been a substantial increase in the published proceedings. I have looked through the whole of the

items, and, so far as I can see, there is only the difference between the corresponding items—that is to say, between this and the last Statement of Accounts—which you might reasonably expect to find; the one report being, as I have said, for 12 months, and the other for nine. The Honorary Treasurer will, I am sure, be glad to afford any additional information, or answer any questions you may be pleased to put.

Mr. J. S. RAWORTH seconded the motion, which, having been put from the chair, was carried *nem. con.*

The PRESIDENT: The next thing on the agenda is a resolution which happily falls very seldom to the lot of the President to propose. It is a resolution with reference to the retiring pension which we propose, with your approval, to allow our present Secretary, Mr. Webb. I take it for granted that his desire to resign has been known to all for some time, and his successor has been appointed. The action of the Council in this matter has been explained in the Annual Report, in which they have also expressed the high appreciation in which Mr. Webb is held by all of us. I need not repeat what I have already said on other occasions with reference to Mr. Webb. My remarks will be brief, because I feel that other Members of Council would like to say something on the subject. Our Secretary, Mr. Webb, has been with us for nearly 20 years. During his stay in office the number of members has trebled. He was with us in the old days of the "Telegraph Engineers;" afterwards we became "Telegraph-"Engineers and Electricians," and subsequently, in order to adapt ourselves to the growing requirements of the time, we became what we are now, the "Institution of Electrical Engi-"neers."

The sphere of our operations has widened, the basis of the Institution has steadily broadened, and with it has grown increased reponsibility: we require at the helm a man with great energy to cope with this increase; and possibly these facts may have been in the mind of Mr. Webb when he expressed his desire to retire after so long a service. It is necessary that we should have a Secretary who will throw the whole of his energies into the work of the Institution, and with that I feel there is a great prospect before

us. Even now we are quite a young Institution, and have not yet attained anything like the proportions which in a few years we may hope to reach, seeing the immense progress which electrical science is making in the world. It is not too much to say that in every member of the Institution who has the slightest knowledge of him Mr. Webb possesses a personal friend.

I may inform you that this matter has received very careful consideration by the Council; and, in putting this resolution to the meeting, we feel that we are justified in recommending that his pension should be at the rate of £300 per annum. Of course we cannot be as liberal as our distinguished foster-parent, the Civil Engineers, but we are following to the best of our ability the example which has quite recently been set by them in the case of their own Secretary.

I think that the Council, in making this proposition, feel, from communications which they have received from members in all parts of the world, that they are putting forward a proposition that will be unanimously supported by everyone; and I trust we may be able, like the Civil Engineers, to arrange for the appointment of an Honorary Secretary, or something of that sort, so that we should not be altogether disassociated from Mr. Webb in the future, but still have the advantage of his advice. I should be glad to feel that the members present unanimously support the Council in their desire to deal with this matter in a liberal spirit.

I beg to move—" That, in further recognition of his valuable " services to the Institution during the past 20 years, a pension " of £300 per annum be allowed to Mr. F. H. Webb on his retire- " ment from the office of Secretary."

Major FLOOD-PAGE: When, some 48 hours ago, I received a letter from a distinguished Member of the Council, saying it was the wish of the President of the Institution that some outsider should second this resolution, and suggesting that I might like to undertake this duty, I responded with great alacrity; for I regard it as an honour that I should be asked to second the resolution, not only to myself, but also to what I can only call commercial electricity. For I have no claim, as an unlearned and

unlettered man amongst you learned men, to occupy so prominent a position to-night, except as I stand as a representative of the commercial side of electricity. I think there is a certain fitness that I should have been asked as a mere outsider to second this resolution. I have not had the advantage, which many members of this Institution have had, of knowing Mr. Webb in his telegraph, in his railway, and in his engineering days, when he laid the foundation which has made him so excellent a man of business, and has enabled him to do such good service to the Institution. My first acquaintance with Mr. Webb was in connection with the organisation of the Electric Lighting Exhibition at the Crystal Palace in 1881. When I first approached him he was, as always, very cautious. He did not quite know whether it would succeed, and therefore he did not quite know whether he ought to commit this Institution to support it; but when he found that nearly the whole of the Members of the Council were willing to give their names as Members of the Honorary Council we were forming—including General Webber, who was then the President, who had been one of the Commissioners at Paris at the 1881 Exhibition —he threw himself heart and soul into the work, and I have always believed that it was owing in a large measure to his influence and interest, and that of the Council, that we were able to make it so great a success. Since that date I have bothered Mr. Webb and the officials of the Institution pretty often. I have always found him in his place; I have always found him attending to his work. I have principally been at the Institution for some reference, or to verify some fact in recent electrical history, and I have always found the greatest readiness and willingness to help, and to give me information on everything one wished to know in the past history of the Institution or of the recent electrical developments. During the time that Mr. Webb has been Secretary the number of members has grown from 800 to nearly 3,000. I do not know whether you recognise how electricity has increased during the last few years; most of the time Mr. Webb has been connected with this Institution. You will find that about 40 years ago

there was only, as far as we know, £700,000 invested in public electrical industry in the United Kingdom. If you look at the book which probably you all know—and if you do not you should—that is, Garcke's most excellent book called "Electrical Undertakings"—you will find that at this present time there are more than £70,000,000 invested in public electrical enterprises. During the whole of this time Mr. Webb has seen this little mustard seed grow into a great big tree, which not only holds 3,000 members in its branches, but, as we beg to suggest to Mr. McMillan, has room enough in its branches for another 2,000. Permit me to say one word with reference to the value of permanent officials. We all know that the heads of the Government departments, and members of councils, and we directors, very often think we are doing a very great deal of work. Well, we lay down the policy, we are responsible for the administration; but, after all, it is the permanent officials, whether it is in the Civil Service or the Institution of Electrical Engineers, or in our commercial companies—it is the permanent executive officials who, after all, make or mar the business with which we and they are connected. I am sure there is no member of the Institution who will grudge to Mr. Webb, as the sole permanent executive official of this Institution, credit for a very large measure of the success and the reputation which it has gained during the last few years. Then, Sir, I would say again that Mr. Webb has exercised a wonderful judgment and discretion in knowing that the moment has come when it is fitting for him to retire. He carries his 73 years wonderfully well. It seems an incredible thing, when you see him, that he should be in his 74th year; but at the same time he has felt, perhaps more than we have felt, that age was creeping upon him, and, instead of waiting till men said that he ought to go, he has had the courage, whatever might be the consequence, to resign the responsible position he has so long and so honourably held. But, Sir, I am sure that I represent the opinion of everyone here, and also the opinion of the 2,500 members who are not here, when I say that we shall unanimously accept the recommendation of the Council, in whom we have complete confidence, and who represent every class of

this Institution, and grant this pension of £300 to Mr. Webb. And I say further that I am sure that I represent the opinion of everyone who is here present when I say that I trust that God may bless him and his, and that he may live to see 5,000 members, as Mr. Raworth has just suggested, in a house of our own (surely this is a legacy that Mr. Webb leaves to his successor); and in his leisure for the remainder of his life he, at any rate, will have the comfort, and the consolation, and satisfaction of knowing that he has earned the respect of all the thousands of members who have been through this Institution, and are in it still, and the personal affection and regard of all the Members of Council, and all of us other outside members who have been intimately connected with him. Sir, I have very great pleasure in seconding the resolution.

Professor SILVANUS P. THOMPSON: There is no need, after the feeling and eloquent way in which Major Flood-Page has seconded this proposal, that any other member should rise to support it; but I feel sure there are some here who would wish to say what a great personal debt we owe to Mr. Webb for many years past for the exceeding kindness, the great sympathy, the unflagging courtesy, with which he has met us whenever we have had to go to him on any business of the Institution. I should not like to be silent on an occasion like this, because I feel very deeply what I owe to Mr. Webb for the good work he has done in his capacity as Secretary in the past. As a member of the Institution, no less than as a Member of Council, I most heartily support the motion.

Mr. W. LANGDON: I should like to make a few remarks in support of this resolution. It seems hardly possible that 20 years have passed since the date when I, as acting Secretary to this Institution, handed over the duties of my office to Mr. Webb. But such, unhappily, is the case. Time is an inexorable master, to whose demands we all have to submit, whether we will or not. Having had some acquaintance with the duties of the office which Mr. Webb now performs, although they are much greater and more onerous now than when I was in that position, I can testify to the fact that they are exacting in the extreme; and we are more indebted to Mr. Webb than the circumstances may appear to warrant. Those, however, who are acquainted with the duties

which fall upon him in relation to Council and committee matters, and the correspondence in one direction and another which necessarily attaches to the secretarial branch of such an Institution as this, will recognise at once that the duty is one calling for all that courtesy and self-sacrifice which has been characteristic of Mr. Webb. And we all know how devoted he has been to that duty. We are truly indebted to him in a greater degree than we perhaps can conceive at the moment. He has devoted to us and our interests 20 of the best years of his life, and he has wound himself around our hearts. We feel that he has been to us something more than Secretary of this Institution: he has been our friend; and everybody will welcome him here amongst us whenever he is able to come here in that position which we all feel gratified is to be accorded him, viz., that of Honorary Secretary. We all wish him that ease and enjoyment in his retirement which the devoted discharge of the onerous duties entrusted to him so fully merits; and, Sir, the greatest pleasure we, on our part, can enjoy, will be to see him here amongst us, as at present, for many years to come.

Professor PERRY: With reference to a remark Dr. Thompson made just now, we all feel that we do not like to be silent at a time like this; but if all the friends of Mr. Webb were to make little speeches, I am afraid that we should not be able to get home for a week or two. I rise, therefore, representing all the others who feel like myself, but who do not quite like to take up time with speeches, to acknowledge his patience and unvarying kindness to us all the time we have known him.

Mr. W. M. MORDEY: Professor Perry has, I am sure, voiced the feelings of us all. We wish to give expression to our appreciation of Mr. Webb's work, and of his kindly courtesy in discharging the duties of his office; but we can only repeat what has already been said. I can speak from a long experience, and, I fear, a trying one to Mr. Webb, for I have given him a good deal of trouble. Whenever I have read any little paper, or taken any part in the discussions, I fear I have always given him a great deal of trouble in his capacity as Editor and Secretary,—in a great many ways I know I have given trouble to which any other man

would probably have objected; but I have always found Mr. Webb most indulgent and most courteous, even when I did not deserve any indulgence. As one of the speakers said, one always felt that Mr. Webb was not merely an official, but was a friend to all who had the interests of this Institution at heart. I will not say more, but I do not wish this occasion to pass without giving utterance to my own personal feelings in these few words.

The resolution was carried unanimously, by acclamation.

The PRESIDENT: Mr. Webb, I congratulate you.

The SECRETARY (Mr. F. H. WEBB): It is not the first or the second time, Mr. President, that I have had occasion to thank you for very kind words in reference to myself. For what has fallen from you this evening I thank you most gratefully and sincerely. I also beg to thank very warmly Major Flood-Page for the kind and very complete way in which he was good enough to second the resolution which you, Sir, on behalf of the Council, were good enough to put to the meeting. To the other gentlemen who have also so kindly spoken in reference to the motion—Professor Thompson, Mr. Langdon, Professor Perry, and Mr. Mordey—I feel very deeply grateful. Nothing could be kinder than the way in which all of these have spoken. To you, gentlemen, who have so kindly accepted the proposal put forward in the resolution, and who have been good enough to testify by your cheers that you endorse the kind things that have been said of me, I beg to express my deep gratitude. It certainly has been my earnest desire to perform the duties of my office to your satisfaction during the time that I have held it; and the knowledge that I have succeeded in doing so, as you have been good enough to imply, very much lessens the pain I naturally feel at resigning the Secretaryship.*

I have had the gratification of seeing the Society grow in numbers and greatly increase in importance; for although as the "Society of Telegraph Engineers" it did exceedingly good work in

* I hope I may be allowed to take this opportunity of testifying to the zeal, industry and devotion of the members of my small staff—Messrs. Richard Tree, Harley Hughes, and Alfred Tree—who have so ably assisted me in carrying on the continually increasing work of the office.

what was then almost the only branch of applied electrical science, it was nothing to what the Institution now is, and it was, we might say, of comparative unimportance; whereas now, as the "Institution of Electrical Engineers," it has taken rank amongst the foremost of scientific institutions. It certainly has been a very great gratification to me, and will be during the remainder of my life, that I should have had the honour of being officially associated with it during those 20 years of progress—a progress which is bound to continue. It is a satisfaction to me to know that I have such an able successor, one possessing so much more energy than I can claim to possess at my age. I have had the pleasure now of knowing Mr. McMillan long enough to call him my friend as well as my successor. I have been in association with him now for nearly two months, and I have seen quite enough to feel sure that he will carry on the duties, I will not say quite as well as, but very much better than I have done. I will not say more now. If I could give utterance to all I feel, I might detain you longer than you would care for. I will therefore only say once more that I thank you all very much, not only for the generous provision which you have made for me—which I feel, considering what the finances of the Institution are, is a very liberal one—but I thank you still more for the kindness with which you make it, and the kind feeling you have evinced towards me. I can only hope that I may not be altogether a burden upon you, but that I may be able, at all events, to render some services in return.

Mr. W. Langdon: I have the pleasure to submit the following resolution for your approval:—"That the members of this "Institution hereby express their deep sense of the obligation "which they are under to the President, Council, and Members "of the Institution of Civil Engineers, for so kindly continuing "to allow the meetings of this Institution to be held in their "lecture hall." That resolution, gentlemen, I am sure will require no recomendation at my hands. You are all perfectly acquainted with the liberal consideration accorded us by the Institution of Civil Engineers. We have enjoyed that liberality for a great number of years, and I feel that we cannot sufficiently express

our gratitude for the favours thus bestowed upon us. We enjoy all the advantages of this palatial building—its magnificent theatre for the reading of our papers, its beautiful council room for the executive work of the Council—added to which we have the privilege of the use of the library of the Institution. In fact, the members of this Institution are, by the kindly consideration of the President, Council, and Members of the Institution of Civil Engineers, placed on a level with the members of that Institution. I therefore feel that you will readily respond to this resolution.

Mr. DANE SINCLAIR: I have very much pleasure indeed in seconding that resolution. I would not add one word, but there is one thought that perhaps occurs to us all from year to year as we thank our friends who grant us the use of this great building, and that is, that we all look forward to the day when it will be unnecessary, not in any thankless spirit, but in the spirit of the engineers themselves, who would then be free. It is very gratifying to me, and I am sure it must be gratifying to all of you, to see that £1,000 has been added to our building fund during last year, making one step at least nearer the time when we shall have what we ought to have, viz., a home of our own. I have much pleasure in seconding the motion, as our thanks are very greatly due to the Institution of Civil Engineers for the kindly way in which they have treated us year after year.

The resolution was carried by acclamation.

Professor PERRY: I am asked to propose—"That the thanks "of the Institution are due to the Local Honorary Secretaries and "Treasurers for their kind services and continued attention to "the interests of the Institution." I believe that every Local Honorary Secretary is a Treasurer, so that it is only to one man in each place that these thanks are offered. I happen to have been Local Honorary Secretary and Treasurer in Japan, and therefore know what the duties are. I know that each of these men must have a very great deal of pleasure, and must feel honoured, in acting for us. At the same time, one has to feel that the only services that are ever done us that are of any great value are those services in which a man takes pleasure and feels honour in

doing. These feelings of pleasure and honour do not, therefore, take away from our sense of obligation, and I beg that these gentlemen may receive the thanks of the Institution for their services to us.

Mr. C. H. WORDINGHAM: I have much pleasure in seconding this resolution. I am afraid a good many of us are rather apt to forget the services rendered by these gentlemen, and yet they are very great. We have only to reflect for a moment what it would cost this Institution if we had to pay for those services, and from that point of view alone I am quite sure that we must value them highly, and also because they are rendered by men who probably have plenty to do. They are men in leading positions in those distant parts, and we all know what it means to give even a portion of the time of a busy man.

The resolution was carried unanimously.

Mr. E. TREMLETT CARTER: I have much pleasure in moving—"That the thanks of this Institution are due to Professor Ayrton "for his kind services as Honorary Treasurer during the year." Many words would be quite as inadequate as few to express our thanks and indebtedness to Professor Ayrton for acting in this capacity. Professor Perry has just told us that in Japan the two offices of Honorary Secretary and Honorary Treasurer are rolled into one. Messrs. Gilbert and Sullivan say they do that sort of thing in Japan! Professor Ayrton is a man of many parts, active both outside and inside the Institution. We know he is a very busy man outside the walls of this building, and outside the duties and interests of this Institution; besides which he serves on the Council, attending to the meetings of the Council at an hour when we ordinary members are comfortably dining. And then he comes upstairs and takes a very active part in the proceedings of the Ordinary General Meetings. We are therefore much indebted to him for his services in the capacity of Honorary Treasurer, and I take the opportunity of adding that we appreciate the many services which he renders, and has for many years past rendered, to this Institution.

Mr. CHARLES BRIGHT: I am glad to have this opportunity of very warmly seconding Mr. Tremlett Carter's motion. I feel

sure—as no doubt everybody else feels sure—that, if the work of Honorary Treasurer is done by Professor Ayrton with the same amount of zeal and energy that he displays in connection with everything he turns his hand to, it must be extremely well done.

The resolution was carried unanimously.

Professor AYRTON acknowledged the vote.

Mr. R. HAMMOND: I have great pleasure in moving—"That "the thanks of the Institution are due to Mr. F. C. Danvers and "Mr. A. Stroh for their kind services during the past year as "Honorary Auditors."

Mr. A. A. C. SWINTON: I have much pleasure in seconding this motion. I am sure that, if Mr. Danvers and Mr. Stroh only exercise in their accountancy the well-known accuracy of Mr. Stroh in other fields, members can have no reason to complain.

The resolution was carried unanimously.

Mr. W. M. MORDEY: I have very great pleasure in proposing— "That the best thanks of the Institution are due to Messrs. "Wilson, Bristows, & Carpmael for their kind services as "Honorary Solicitors during the year;" and our thanks in this case ought to be inversely as the work done.

Mr. A. STROH: I have much pleasure in seconding Mr. Mordey's proposal.

The resolution was unanimously carried.

The PRESIDENT: I have received a letter from our Honorary Solicitors this evening, regretting their inability to attend to answer any questions which should be asked. I shall have much pleasure in forwarding your vote of thanks to them.

I have much pleasure in informing you that the scrutineers report the result of the ballot for Council and Officers for the year 1898 to be as follows:—

President:

JOSEPH W. SWAN, F.R.S.

Vice-Presidents:

Professor S. P. THOMPSON, D.Sc., F.R.S.

Professor JOHN PERRY, D.Sc., F.R.S.

W. E. LANGDON.

JAMES SWINBURNE.

Ordinary Members of Council:

S. L. Brunton.
Henry Edmunds.
Professor J. A. Ewing, F.R.S.
W. P. J. Fawcus.
Robert Kaye Gray.
Major R. Hippisley, R.E.
Professor J. A. Fleming, M.A., D.Sc., F.R.S.
P. V. Luke, C.I.E.
E. Manville.
W. M. Mordey.
John S. Raworth.
Dane Sinclair.
A. A. Campbell Swinton.
Herbert Taylor.
Charles Henry Wordingham.

Associate Members of Council:

Sydney Evershed.
H. W. Miller.
Sydney Morse.

Honorary Auditors:

Frederick C. Danvers. | E. Garcke.

Honorary Treasurer:

Professor W. E. Ayrton, F.R.S., Past-President.

Honorary Solicitors:

Messrs. Wilson, Bristows, & Carpmael.

The President: I have further to announce that the following candidates have been duly elected:—

Associate:

David Smith.

Students:

Murray Thomson. | John Frederick Wakelin.

The Three Hundred and Seventh Ordinary General Meeting of the Institution was held at the Rooms of the Chemical Society, Burlington House, W., on Friday evening, December 17th, 1897—Sir HENRY MANCE, C.I.E., President, in the Chair.

The minutes of the Annual General Meeting, held on December 9th, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The SECRETARY announced that donations to the Library had been received since the last meeting from Professor E. Wilson and Messrs. Whittaker & Co., to whom the thanks of the meeting were unanimously accorded.

The PRESIDENT: Mr. Desmond Fitz-Gerald is not able to be here to-night, but has sent a written communication, which, with your permission, I will ask the Secretary to read, in continuation of the discussion on Mr. Epstein's paper.

Mr. D. G. FITZ-GERALD [*communicated*]: It has not occurred to the author of the paper, nor to those who have commented upon it, to compare the accumulators ostensibly available for traction some two or three years ago with those which undeniably exist at the present moment. Mr. Fitz-Gerald.

I think that it would be most interesting to do so, if there were time; and also to glance at the directions in which future improvement will probably be made. Certainly the development of the traction accumulator, in the face of the normal amount of inertia and stolidity which oppose progress, will be an instructive page in the history of applied science.

The following (Table I.) is a limited sample of the data upon which—even less than two years ago—traction by accumulators had, as we were told, to be based. Anything much in advance

Mr. Fitz-Gerald.

of these was wild and visionary—mere laboratory experimentation and "so-called" invention.

Table.	Name of Accumulator	Normal or Safe Rate of Discharge.	Specific Capacity.	Minimum Weight. Per H.P.	Minimum Weight. Per H.P.-Hour.
		Amperes.	Amp.-Hrs.		
		Lbs.	Lbs.	Cwts.	Cwts.
I	E.P.S. (price lists) L 23 ...	0·193	2·14	18·1	1·63
	E.P.S. (price lists) T 11 ...	0·66	2·2	5·3	1·60
	E.P.S. (price lists) K	0·35	1·17	10·0	3·0
	Epstein (Report of Alabaster and Gatehouse), Nov., 1894)	0·615	1·856	5·7	1·9
	Epstein (Report of Professor Ayrton), July, 1892	1 (nearly) $\left(\frac{50}{53\cdot5}\right)$	2 (nearly) Much higher rates of discharge for short periods of time might, or might not, be safe.	? 3·8*	? 1·76*
II.	Tommasi cell, Feb., 1895	0·454	10·0	...	...
		0·9	9·0	1·7	0·55
		2·7	7·0 (time, 2·6 hrs.)	(? 1·3)	(? 0·50)
		3·41	5·1 (time, 1·5 hrs.)	...	...
		Maximum for starting and on inclines, 6·82	(Speed attained, 12½ miles per hour.)		
III.	E.P.S. Faure - King (? Faure - Tommasi) cell; time, 5 hours	0·9	4·5	3·9	0·78

* Professor Ayrton's figures.

Now I will ask you to compare the figures next in order (Table II.), which were *approximately* verified three years ago in the case of an electric cab running in Paris, and nearly two years and a half ago under the difficult conditions of a scramble from Paris to Bordeaux.

These values, no doubt, are roughly approximate only; but they have not been disproved or contradicted.

Mr. Fitz-Gerald.

The main characteristics of this cell were the embedded supports and the external envelope of perforated celluloid.

Now, about two and a half years ago, a young electrical engineer was testing an accumulator which bore a strong family resemblance to that of Donato Tommasi, but which was called the I.E.S. cell. This he compared with an E.P.S. cell, and he found that, when the former was discharged at a rate 6·25 times the maximum safe rate of discharge of the latter, the capacity was, nevertheless, 3·4 times greater than that of the E.P.S. cell. Unfortunately, he made a double mistake, in that he selected for purposes of comparison the L type of E.P.S. cell, instead of the T or the K type, which would certainly have been more suitable for comparison, and he omitted to state which type he had selected; but his observations were, nevertheless, correct, and they have borne strange fruit. For now we have in actual use, working the electrical cabs in our streets, another battery bearing a strong resemblance to that of Tommasi, but called, on the *lucus a non lucendo* principle, the Faure-King, instead of the Faure-Tommasi, accumulator.

The figures in Table III. will allow of its being compared with the accumulators mentioned above.

It is true that perforated celluloid has quite recently been been replaced—temporarily, I think—by perforated vulcanite; but the above very encouraging results were obtained with the former material, and by the very gentleman who declared that he "had "used a great number of tons of celluloid, and had a large and "distressing experience of its use."

Although I took out a patent—No. 7636, 1886—for the use of perforated celluloid to retain peroxide active material, it must not be supposed either that I claim any credit for its introduction, or that I advocate its continued use. I regard it as a *pis aller*, which has proved useful, but which can and will be dispensed with.

It was stated at the last meeting that an American inventor has proposed to replace the perforated celluloid with soft vulcanised india-rubber, in which he encased the peroxide plates. A more mischievous suggestion it would be difficult to make,

Mr. Fitz Gerald.

excepting, perhaps, in the statement that "it is well known to "everybody that celluloid is absolutely useless in a battery." Because, outside as well as within this Institution, there evidently are people who are not aware that soft vulcanised caoutchouc—the use of which, for the purpose in view, was claimed in Faure's original patent—is a very oxidisable material, even in air, and that its rapid destruction may almost be regarded as a test of the presence of ozone. On the other hand, pyroxylin—the basis of celluloid—is, under the temperature of 300° Fahr. *bien entendu*, one of the most inoxidisable bodies known, as might be inferred from the fact of its being generated in a bath of the strongest nitric and sulphuric acids.

The camphor which is present in celluloid, and the impurities which may be associated with it, are oxidisable enough, but not the pyroxylin.

In regard to my own present idea of a traction battery, the use of very light supports for the active material—in the case of the peroxide as well as the spongy lead element—is the direction in which I think progress is to be made. The *prima facie* objections, of course, are—first, that the peroxide element would be rapidly destroyed by local action; and, second, that in the case of large plates with very thin lead (in my experiments the thickness is only 25 mils) the conductivity would ordinarily be insufficient. But there are two distinct methods which have been devised, after years of labour, for obviating the local action to any required extent; and a system of multiple contact with the plate has been thoroughly worked out. There are also minor difficulties, which, however, are much more readily overcome than the above.

Mr. Warden-Stevens.

Mr. F. J. WARDEN-STEVENS [*communicated*]: As I have lately been contributing a series of articles in the *Architect* on the subject of electric tramways, in which the question of accumulator traction has been discussed, there being many points on which I entirely agree with Mr. Epstein, perhaps I may be permitted to make a few remarks.

I think that the author's attempt to compare accumulator traction with the trolley system is not very clear. Although the trolley has the advantage of less attention along the route, the

accumulator system should require a smaller plant, and less attention in the central station; but I think it is very difficult to compare or copy the trolley system, as the conditions are so different. Mr. Warder-Stevens.

The idea of using a battery-car locomotive is certainly a good one; although it has been suggested often, there seems to have been no attempt to carry out the idea. As we no doubt shall have these bogie cars, I would express a hope that they be made to look a little better than the steam cars in Birmingham or the Brixton grip-cars. These ugly square-looking objects disfigure a street far more than any reasonable system of overhead work. Now that the London County Council appear to be reconsidering the question of the Embankment tramway, might not an attempt be made by the Institution to persuade them to adopt a combination lighting and tramway system, with, say, accumulator traction on the very latest lines? There is no reason why the results in Hanover and Paris should not be equalled or improved upon.

Referring to Mr. Epstein's criticism of the Hanover system, when he suggests detaching the accumulator when the trolley section is reached, this is very good from the dead weight point of view, but surely one of the great advantages of the Hanover system would be lost. I refer to the regulating action of the cells on the car while being charged. This equalises the pressure locally, which would not be done if the cells were being charged from the trolley wire at one point.

The new Tudor cells with enormous surface of plate as used on one of the Paris lines is a departure which will be watched with interest. When Mr. Epstein states that charging of these cells at constant potential causes very high current-densities, I believe he is mistaken, as, owing to the extraordinarily large surface of the plates, the current-density is never high, and is, in fact, less than that usually allowed for pasted plates. However, this mis-statement is probably an oversight, and the "current-" density" is used for current.

Referring to the use of series-parallel battery control, instead of series-parallel motor control, or the use of a double-wound

Mr. Warden-Stevens. motor, as in the electric cabs, I should say that it is in every way more convenient, and should be adopted, provided that no difficulty is found owing to the current not being shared equally when the battery is paralleled. If one is allowed to argue from what occurs in a single cell where there are many plates in parallel, there should be little difficulty on account of this.

Mr. Wallis-Jones. Mr. R. J. Wallis-Jones: I think the thanks of the Institution are due to Mr. Epstein for introducing, particularly at this time, a paper upon the subject of electric traction by accumulators, for I believe we are now well in the turn of that long and uphill road along which the pioneers of electric traction by the accumulator method have so long trodden. The labours of such men as Mr. Reckenzaun and Mr. Ellison, and others, I think are now about to be rewarded. I venture to make a few remarks to-night because I have had some years' practical experience in the working of a tramway system by accumulators. In my opinion the system of accumulator traction has never really had a fair trial in England. Except in the case of the electric cabs the accumulators have not been used under best conditions of working, such as are obtainable when the battery is kept as one complete unit, and series-parallel control adopted.

There is another point in connection with accumulator traction which I do not think has received the proper amount of attention in England. Practically, tramway cars may be worked on two distinct systems. Either you may have a set of cells having a large trip capacity, such cells being of very great weight, or you may have light cells sufficiently large to run the car for the complete trip. In the former case of course you have the condition of slow charge, and in the latter the condition of quick charge. Mr. Epstein referred to the Paris system which has recently been installed, which is worked on the principle of the lighter cells, and I think the results are likely to be well worthy of attention. I entirely agree with Mr. Epstein about the necessity of keeping the battery as one complete unit. You cannot work cells satisfactorily if you split the battery up into quarters and ring the changes on the various groupings for speed variations. One form of cell grouping formerly used at Birmingham was

exceedingly bad, and should be avoided. For the highest speed, two quarters of the battery were used in series, and the other two in parallel series: this was called the "tandem" grouping. In practice the following frequently occurred:—At the end of the Birmingham line, where the cars come into the depôt, there is a bad curve on an incline, and if a car had a battery in low condition on board, it was often unable to get into the depôt round the curve until the drivers had reversed the connections, so as to put the quarters of the battery which had been in parallel series on the return trip, in series. The car would then go round the corner, because the quarters of the battery which had been in parallel series were less exhausted than those which were in series; in fact, I have seen a difference of as much as 5 volts (tested under discharge) in the two halves of the batteries when measured on entering the depôt. There are several other advantages in keeping the cells as one complete unit, and in one containing tray. In the cases where the cells are put in the sides of the car itself a most objectionable feature is the smell which arises from the battery, and, do what you may, you cannot prevent the fumes from the battery rising into the car itself; whereas, if you suspend the cells in a separate tray below the car, this trouble is overcome. When cells are used in the car body, by far the most serious side of the question is the rotting of the cars. Car repairs under such conditions amount to a considerable item, especially after the cars have run a few years—indeed, may reach far above the figure suggested for the total maintenance of the cells in Mr. Epstein's paper. By keeping the cells in one complete case you also save the breakage of the ebonite boxes—a most important item. An ebonite box, I suppose, on the average is worth about 10s., and they are frequently broken if you place the cells in separate cases, and have to haul them either on stills or on elevators. I think another very important saving is made by charging the cells in one case on stills, instead of on elevators, as it is exceedingly difficult to keep the leakage low, on account of the damp and corrosion of all the contact points and in the cables of the elevators. I am of opinion that the traction cell of the future will be a cell which has for the positive plate a

Mr. Wallis-Jones.

Mr. Wallis-Jones.

Planté type of plate, and a negative of some form of pasted plate. The cost of cleaning such a type of plate is exceedingly low, whereas in pasted cells of any type it is exceedingly high, and a serious item in the upkeep of the system. Professor Ayrton made some calculations at the last meeting as to the weight of cells necessary to propel a car of a given weight. I think it may be of interest to state the total weights of some of the batteries used at Birmingham. A Planté type of battery —including all containing cases, &c., of the complete battery of 96 cells—weighed just under 6.500 lbs.; while the weight of a battery of the pasted type amounted to 6,700 lbs., and the speed at which the car was propelled was about 7·2 miles per hour, including stoppages. In the paper Mr. Epstein estimates the life of the battery at 75,000 car miles. I am afraid that that is a very sanguine estimate (and I notice that he has in this case gone in for cells of what I should call high trip capacity; that is to say, he estimates on each discharge the cars will run 50 car miles). I think, after allowing for the obvious defects of a system such as we have at work in Birmingham, and allowing a considerable margin for improvement of working conditions of rolling stock, electrical details, &c., with cars of same weight,—I think with pasted batteries, on that line at any rate, if you got about 8,000 car miles as the life of one set of positive plates, you would be doing very well; and if you took a Planté type of cell, I think if you obtained about 17,000 car miles as the life of the positive, it would be a good result. In estimating the cost of renewal of batteries, I see that Mr. Epstein allows for the renewal of the positive plate only. I do not know whether it is an oversight, but it would appear that he is reckoning upon the negative plate going on for ever, and that, of course, is quite outside the question. I think a fair estimate would be to allow one negative plate to every two positive plates. In that case, taking the negatives at the same price as allowed for the positives, the cost of the renewal of the plates would amount to something like 0·52 of a penny per car mile. In conclusion, I think that the system of the combined trolley and accumulators, as used in Hanover and other places on the Continent, is likely to prove of great service in this country,

in towns where the objection to the trolley wire is very serious. Mr. Wallis-Jones.

Professor R. H. SMITH: I think it is extremely useful to the Society to have heard the results of the Birmingham experience that the last speaker has just given us. I regret that more of this experience was not communicated at the fuller meeting when Mr. Epstein's paper was read. I myself cannot confess to any very sanguine feelings with regard to the future of accumulator traction, and for that reason I feel all the more bound to mention one other fact with regard to the Birmingham line on the Bristol Road. I understand that one of the many difficulties there was that the road was badly laid, and that the gauge of the rails varied according to the weather, causing at certain times and at certain places the sticking of the cars. The two great causes that handicap accumulator as against other modes of traction are—first, a low degree of total efficiency; and, second, an excessive proportion of weight to carrying capacity. The part of the inefficiency due to the ratio between the power put into an accumulator and that got out of it, is an ADDITION to those other items of inefficiency which are common to this and to most other systems. I am not an expert in accumulator construction, and I fully expect to see large improvements in efficiency take place within a few years, but I may point out that it requires a very large improvement in this respect to bring up this system to anything like a par with other systems. The other great handicapping influence is, of course, the great dead weight; and here again, although I am not a chemist, and therefore am unable to judge of the theoretically possible future reduction of accumulator weight, I am quite willing to take for granted—in fact, I do believe—that large reductions will take place; still, I must point out that it requires a reduction of perhaps 60 per cent. of the weight which was common at any rate two or three years ago, to bring down this difficulty to any reasonable proportion. That being so, it has been my opinion for some time past that this accumulator system has really more chance of success in those circumstances in which locomotives drawing a train are desirable, because in a locomotive Prof. Smith.

Prof. Smith. you want great weight in order to get the necessary grip on the rails. With regard to the Hanover lines, I have pondered a good deal as to the possible advantages of that system, and I cannot see them. It seems to me that the system combines many of the disadvantages of the trolley system and of the accumulator system. Why should you carry your accumulators about the line that is fed by a trolley wheel? and why should you charge your accumulators by so very inefficient a method as taking a current down from a line through a running trolley wheel? It would seem to me more reasonable to use small locomotives, charged while they are stationary from the trolley lines at the ends of the stretches over which trolley lines are permitted. Use cars without accumulators, and hitch on your accumulator locomotive at the beginning of the length where you are absolutely forbidden to use overhead lines. I have welcomed, with very great satisfaction, the appearance of accumulator cabs in the London streets. I can naturally predict nothing as to the financial future of the company, or the prospects of the shareholders; but of this I am perfectly sure—that these cabs, as we see them just now, are good enough to remain for some time on the streets, and I hope permanently, without any public protest that will drive them away. They are doing this: they are educating the man in the street to recognise the practicability of mechanical traction on the streets of a large city without public nuisance. That is a lesson which is extremely important. The public have had rather severe lessons in the contrary direction, which have to be corrected before there is any practical engineering future for mechanical traction in the streets of large cities. I confess that even in the short period since the cabs appeared one may notice that they have become a little more noisy than they were during the first few days they were running. That is a disappointing result, which I did not expect. That leads to the mechanical question of gearing. On that point I would say, firstly, that I should like there to be no gearing at all. That is my *beau ideal*; and whatever may be practicable with regard to accumulator traction, or any other kind of electric traction, I am tolerably certain that with oil, gas, or steam engines motor cars will do

ultimately without any gearing at all. A vehicle of any kind—a waggon, a cab, or a car—must have a frame that is, to a certain extent, elastic or flexible, especially if it is to run on ordinary road surfaces. Straining in the frame is very destructive to well-designed gearing of any kind whatever. This straining means change of the geometrical relative positions of the different bearings of the different shafts. I think that, although some very ingenious attempts have been made to use worm gearing for electric cars, this question of straining of the frame puts worm gear absolutely out of the range of practical possibility. It is not generally known that worm gearing requires far more exact setting of the worm to the worm-wheel—*i.e*, of the worm shaft to the worm-wheel shaft, or the one set of bearings to the other set of bearings—than does any other kind of gearing. If you have a little want of true adjustment the worm is ground to pieces, and so are the teeth of the worm-wheel, very rapidly indeed, and with a correspondingly large waste of power. The same objection applies also, but in a much less degree, to spur gearing. Then I would like the members to consider one other objection to gearing of this sort in these cars, namely, the swing of the nose frame. This nose frame must be allowed to joggle up and down to a certain extent. The gearing between the shaft upon this swing frame and the axle produces certain kinetic and dynamic results dependent upon this to-and-fro swinging. The resulting changes of velocity are, I think, of very small importance as regards electro-dynamic action between the armature and the field magnets; but, although they have not time to develop into any visible alteration of velocity, yet the rate of change of velocity may be quite high—is quite high—and that means a large acceleration of momentum, and that means very heavy and severe mechanical stresses between the different parts of the gearing. Let me ask you to consider what any mechanical engineer would think of the following arrangement. Take any ordinary stationary horizontal engine in which the cylinders and the crank-shaft are geared by spur gearing to the shaft upon which the driven machine is mounted, or from which the power, at any rate, is taken. Suppose you have such intermediate gearing, and that Prof. Smith.

Prof. Smith. you mount your cylinders and this gearing upon a frame which you hinge round the driven shaft as centre to the bed-plate of the whole engine, and which at the other end you support upon springs. Then oscillate the whole frame carrying the cylinders, &c. Now think of the engine standing still, if you please—not running at all—and this cylinder and shaft frame oscillating. The driven shaft is prevented from moving rotationally, and as the frame flies up and down the spur gearing causes rapid to-and-fro rotation of the crank-shaft, and rapid reciprocation of the connecting-rod, cross-head, and piston. The angular and linear reciprocating movements are of small range, but the accelerations of velocity and of momentum are large. The result is that heavy sectional and bearing stresses are caused, and you run very great risk of breaking your teeth, even although you make them very strongly. The actual effects are modified by the elastic give of the parts and by the slackness at the joints, but these are only evidences of severe stresses and injurious shock. These effects are more easily recognised when one thinks of their generation when the engine is not running, but the running of the engine leaves them wholly unaffected,—it makes them neither more nor less. On these grounds I think that chain gearing is really the only practicable kind to use successfully in this kind of machine; and I wish to mention with great commendation, if my commendation is worth anything at all, Renold's chains. These are the chains that are used in the electric cabs in London, and probably most of the members know the construction. It is an extremely ingenious construction of chain, which utilises the width of the chain very largely in giving bearing surface as between the chain and the teeth of the wheel which it drives. I think that improvements in chain gearing are still possible, and I look forward to improvements in the direction of diminishing the pitch and increasing the breadth of the chain,—towards the construction, let me call it, of steel belts in place of leather belts. Now a good deal was said about the question of mechanical resistance on the first day of the discussion, and Professor Ayrton and others mentioned measurements of tractive resistance in drawing carriages along roads of various surfaces. I see there is to-day a table on the

wall giving some such results. The resistance, however, to drawing a carriage over a definite kind of surface and under definite conditions is very different indeed from the resistance to self-propulsion under the same external conditions. Self-propulsion requires far greater horse-power than the horse-power exerted through the drawbar of a locomotive upon a train behind it. It is one of the commonplaces of railway engineering that the resistance in pounds per ton of the locomotive is far greater than the resistance in pounds per ton of the train that it draws. Think again of the difference between the pull that is necessary to tow a boat through water by a tug, multiplied by the velocity, giving the towing horse-power, and the horse-power that is necessary for a self-propelling ship at the same speed and under the same weather conditions. They are utterly different. The one may be three times as great as the other. Only a fraction of the difference is accounted for by the internal friction of the driving mechanism. To understand further the causes of the difference, one may consider the riding of a pneumatic-tyred bicycle with the tyre well blown up and with the tyre flabby. Most of us have felt the large difference in horse-power required. There is work done in two ways, both upon the ground and upon the tyre, by vertical compression and by the driving shear. So far as the vertical compression is concerned on tyre and road surface, you get very much the same resistance in self-propulsion as in dragging traction, but in self-propulsion you have a much larger development of shear on both tyre and on the road surface. The rubber tyre of a cycle is shorn tangentially in the effort to drive. The tyre is not permanently put out of shape by that process; each portion regains nearly perfectly its original shape after it has done its duty. That means that the work that has been done in shearing the rubber is restored again, primarily at least, as mechanical work; but, unfortunately, it is not restored in the way of doing driving work upon the vehicle—at least, only a small percentage of it is restored as useful work in driving. Again, the surfaces of macadam and many other kinds of road being inelastic, the shear-strain work done upon them in the process of driving is wholly lost, as is also that done on the Prof. Smith.

Prof. Smith. vertical compression of the track. Then there is the flange-and-rail resistance in tramways and locomotives. That is absolutely different in the two cases of traction and self-propulsion, and it is a very large item in the total of railway resistance. The self-propelling vehicle thrusts the flanges of the steering wheels against the rails and grinds against them. The rails cannot steer the vehicle except by that side pressure. The vehicle that is drawn by a drawbar is steered for the most part by the drawbar, and the rail may not have anything, or very little, to do with the steering. There is the same great difference on ordinary road surfaces. The steering of a self-propelled vehicle results in greater loss of power than the steering of a vehicle that is drawn. For road carriages and waggons, considering that one must design for the worst probable conditions of road surface and of weather, I think that Professor Ayrton's estimate of 140 lbs. per ton is not too much. If it errs, it is in the direction of under-estimating the resistance to be allowed for. A good deal has been said about smallness and negligibility of wind resistance. Well, wind resistance is small in these cases that we are especially considering just now, chiefly because the length of the train is very small. The length of the train has more to do with wind resistance than the exposed head surface. It is also small because the velocities attempted with road cars are small. The ordinarily stated law of wind resistance is that it varies as the square of the velocity, but that has been known for 40 years past to be inaccurate. It gives a too rapid increase of wind resistance for small velocities, and one not rapid enough for large velocities. I have a formula which, I think, represents more truly the actual rate of increase, viz.,

$$R = C\,V^{1+mV}.$$

This makes the power of the velocity go down to 1 when the velocity is nearly nothing; it gives a slow increase for low velocities, and the power goes up more and more as the velocity increases. If you compare that formula with the ordinary rule, $R' = C'\,V^2$, the proper values of the two constants are $m = 0{\cdot}03$ and $C = 5\,C'$. The constant C in my formula is five times the constant C′ that you should use in the ordinary one for the same set of conditions. These two values, $m = 0{\cdot}03$

and $C = C'$, make the results of the two formulas the same at 10 miles per hour, both as to magnitude of the resistance and as to rate of increase of resistance. In conclusion, I might mention two numerical measures of horse-power that are useful in this connection. These are— Prof. Smith.

$$1 \text{ H.P.} = 375 \text{ mile-lbs. per hour};$$
$$= \tfrac{1}{6} \text{ mile-ton} \quad „$$

That is, a pull of 375 lbs., or $\frac{1}{6}$ ton, dragging at a velocity of 1 mile per hour, exerts 1 horse-power.

Mr. E. WYTHE SMITH: The problem of electric traction, especially by means of accumulators, is one which has engaged my attention for some time past, and, therefore, I have read Mr. Epstein's paper and the discussion thereon with the greatest interest. As to accumulators themselves, I have had opportunities of testing different types under different circumstances, but have never come across one of so large a capacity as that Mr. Manby gave for the Marschner cells now employed on the Dresden tramways. I think Mr. Manby's figure was 77 lbs. per kilowatt-hour—that is, 13 watt-hours per lb. Speaking from memory, the lightest cell I have ever come across, in my own tests, is something like 7 or 8 watt-hours per lb. of cell—that is, gross weight. Mr. Wythe Smith.

The results of all my experiments with different types of cell, under different conditions, lead me to believe that the future cell which will be used for traction will be a Planté cell, or some modification of the Planté cell. The rough usage, such as short-circuiting and rough handling, these plates will stand, and the way in which one can easily bend them back again into the right shape when they have become buckled, puts them far ahead of the pasted cell, other things being equal, for traction purposes.

A good many speakers have referred to the figure, 60 lbs. per ton, given by Mr. Epstein as the friction on ordinary roads. My brother and I have made a number of experiments on this point on certain London roads, by which we endeavoured to get the average pull on an ordinary road under the worst conditions. We chose days on which the roads were heavy with mud, or on which they were rendered soft by heavy rain. Professor Smith

Mr. Wythe Smith. suggested that results obtained in this way were not of much value as regards the problem of self-propulsion.

Professor R. H. SMITH: I meant they are of great value; but there are two different sets of figures, and both are necessary.

Mr. E. WYTHE SMITH: If I understood him rightly, he said the true value might be three times that obtained from experiments such as ours, in which the force required for self-propulsion was obtained by observing the pull on a draw bar coupling. Foreseeing that the strains on the vehicle when self-propelled differed somewhat from those when towed by an external motor, because when pulled by a horse additional pressure comes on the front wheels, whereas when self-propelled by the rear wheels the strains are such as to tend to lift the front wheels, we attached the coupling to the phaeton at different levels—(*a*) low down, (*b*) about the level of the floor of the phaeton, and (*c*) above the seats. As there was no observable difference in the pull required, we concluded that our results might be relied upon. The steering was effected by hand independently of the coupling. Although these experiments lasted over four or five weeks, we were able to pick out days on which the conditions of the road were very comparable.

IRON-TYRED PHAETON ON MUDDY ROADS.

Traction on London Roads.

	Lbs. per Ton. A.	B.	C.
Asphalte	22	23	22
Wood (good) ...	22 to 29 (clean)	31	30
Wood	38	—	—
Macadam (good) ...	52	50	49
Macadam	60	51	50
Macadam (soft) ...	97 (new)	51	54
	—	80 (new)	68

The above table is arranged in the following way:—In the first column (A) are the results of experiments on days when the roads were very muddy (excepting the value 22 lbs. per ton, which was for clean good wood). The values given in columns B and C were obtained when the mud was more of the condition

of pea soup. The figures given in the different lines refer to the same road. Observations of the pull were recorded every 20 seconds, and the mean for a double journey was taken as the true value for that road. On good roads, by comparing the observations going and returning, one would see the rise and fall of the road. The results given in the table are the averages of observations taken over some six or seven miles of road, and may be relied upon to within about 10 per cent. There is one interesting point to which I will draw your attention. The first day we went over the road which gave us the results in the last line but one, was only a day or two after it had been left by the steam roller after repair, when the pull required was 97 lbs. per ton. For the same road three weeks later we obtained the value 51 lbs. per ton; and a little later still, the value 54 lbs. per ton. As the last figures were so different from the first, we looked about to find another road which was fairly a virgin road. We found one which was about four or five days old, and we obtained the value given in the last line (80 lbs. per ton); and a week later we got 68 lbs. per ton on the same road. Mr. Wythe Smith.

We next attempted to find what would be the average pull during an ordinary journey in London We chose a journey which consisted of about one part asphalte, two and a half parts wood, and four parts macadam. In that journey we included the hill running up to Hyde Park Corner. When the pull was negative—that is to say, when going down hill—it was always reckoned as zero. The average pull for the journey we found to be 45 lbs. per ton, which value we think may be relied upon as what is required for average London work; so that the figure given by Mr. Epstein in his paper, 60 lbs. per ton, is well above what should be necessary, and I think he might with safety reduce it to 50.

In order to determine the greatest effort likely to be required, we will take the case of a vehicle on a soft, newly made macadam road with an up gradient of, say, 1 in 25. The pull due to the incline would amount to 90 lbs.; to overcome road friction would require about 100 lbs.; giving us 190 lbs. Then we might have the wind blowing what we should call in London a good gale—

Mr. Wythe Smith. something like 20 miles—which would give us a pressure on the whole of the vehicle of, say, about 20 lbs., making in all something about 200 to 220 lbs. per ton as the maximum pull required, neglecting the additional pull to produce the acceleration at starting; so that, although by allowing 50 lbs. per ton we should have made sufficient provision of energy, we must design our motors, accumulators, &c., so that they will be capable of giving an effort equivalent to at least five times what we expect as the average.

Comparing the figures in the table with those given by Professor Ayrton for a bicycle on a good road, you will notice that they are considerably less. I was rather surprised to find these figures so small, but account for this by assuming that, in the case of a pneumatic, the back part of the tyre—the part behind the axis—in contact with the road, is acting, as it were, as a sucker holding the wheel down, thus producing a downward force behind the axis of the wheel, to overcome which requires an additional driving torque. I do not say this is the case, but only put it forward as a suggestion. (The figure given by Professor Ayrton for a bicycle on a good road was, I think, between 45 and 60 lbs. per ton.)

With regard to the first part of Mr. Epstein's paper, I cannot say that I am converted to believe that accumulators on a tramcar is the proper solution for the overhead trolley system difficulty, for I am still in favour of the form of underground trolley in which there is no slot; but in this I suppose I am of biassed opinion. My thanks are due to Mr. Epstein for his paper, especially as it, and the discussion which has followed, have brought our knowledge of accumulators and motor cars up to date.

Mr. Bell. Mr. J. R. BELL: I had not the least intention of speaking this evening, beyond conveying to you privately, Sir, my thanks for the courtesy of inviting me to hear these discussions. I have heard them with a great deal of interest, and I know of no technical Society so full of young blood and so thoroughly alive and keen. Personally, being solely a mechanical and civil engineer, I have taken more interest in the remarks which have fallen from Professor Smith than perhaps any other details of

this question. It has appeared to me for a long time, as I suppose it has appeared to the inventors of the much-discussed pneumatic tyre, that when you come to mechanical propulsion of vehicles meant to carry human beings, light goods, &c., you must have *springs;* and that the best thing to do is to follow the bicyclists and insert your springs between the felloes of your wheel and the road, rather than between its axle and its frame. That, I take it, is what the pneumatic arrangement essentially consists of: it practically consists of a multitude of small transverse arched springs, each of which gives way to the little irregularities of the road; and you can propel the machine with perfect safety, because the axle does not rise or fall, or vary its relation to the motive power and the frame. In supplement to what has just fallen from Professor Smith, I may perhaps say that, so far as practical railway men know or believe, the difference between the friction of a steam locomotive and that of the vehicles it draws is not occult, but is due to the very serious mechanical friction of the propelling machinery. We have the friction of the cylinders, of the piston rods, the connecting- and side-rods, &c.; and a locomotive, as a rule, has a very long and rigid wheel-base, which intensifies curve resistance. I am sorry, Sir, that these remarks in reply to your kindly invitation are so little apposite. Coming to the question in hand, I do think that, if the more mechanically inclined members of your Society would devote themselves to the question of interposing springs between the felloe and the tread of the wheel, they would very shortly make a very material improvement in that promising institution, the electric cab. Mr. Bell.

Mr. ALFRED DICKINSON [*communicated*]: I have read with much interest the reported discussions and observations on Mr. Epstein's paper. I am sure that the whole of the gentlemen who have discussed this, particularly those who have advocated the use of accumulators, have done so believing in what they have advocated; but is it not difficult for those gentlemen who are largely interested in the manufacture of accumulators to avoid bias in their favour? I take it that my own experience is not exceptional. The representatives of various manufacturers have from time to time called upon me and discussed the question of accumulator Mr. Dickinson.

Mr. Dickinson.

traction, and a more sanguine body of men I have never met. Almost invariably each representative will claim that his battery is less bulky, of less weight, of greater efficiency, will give more rapid discharges, and stand the wear and tear of tramway working much better than the battery of any of his competitors. For the edification of the representative of a firm who recently called upon me, I ran through the claims of a few of the principal manufacturers, including his own, and demonstrated therefrom that, taking as a basis the oldest established accumulator, we could, if his claim was good, get greater efficiency, more capacity, and better results with a minus quantity in both size and weight.

No method of traction as an ideal system offers the fascinations of accumulator traction, but I know from bitter experience that up to the present it has proved an illusion and a snare.

Mr. Epstein, Mr. Parker, and others have discussed the question as though the whole of the difficulties of accumulator traction had been overcome. Both gentlemen have had considerable experience, and in the light of that experience it is difficult to understand their assertions that electric traction by accumulators on rails and roads can be successfully worked. That accumulators will propel cars and road vehicles is perfectly true, but it has yet to be demonstrated that they will do so commercially.

That there is a large sphere of usefulness for accumulators I am prepared to admit, but in my opinion that sphere does not at the present moment embrace the successful operation under normal conditions of tramways and road vehicles.

I suppose I may without egotism say that in the various classes of tramway traction I have had as great experience as any one in the United Kingdom.

On the Birmingham Central Tramways, to which reference has been made in the course of discussion, I think the batteries of all the principal makers up to the present time have been tried. All have known the conditions which have to be fulfilled in respect to this system, and my recollection is that all have in the first instance entered upon their responsibilities with a light

heart, promising magnificent results; each one after a very short period of working has found fault with everything in connection with the installation, but never have they admitted that their batteries have been at fault. Mr. Dickinson.

I am sure Mr. Parker must have forgotten the facts when he suggested that if he had been allowed a free hand he could have made a success of this line.

What is success? If this means satisfactory running of the cars, then this has all along been an accomplished fact. The success which is wanted is a commercial success, which leaves a profit to the investor.

So anxious were the directors of the Central Tramway Company to make a success of this system, that, even after the commercial failure of the bogie cars, they gave Mr. Parker's company a free hand to build a car to their own design, they to guarantee—

(1) Its satisfactory running and maintenance for 10,000 miles.

(2) That the electrical horse-power used under ordinary conditions should not exceed per passenger carried that of the existing bogie cars.

(3) That it should travel at the same rate of speed, be capable of running in the same service and to the same time-table as the bogie cars.

It is unnecessary to say more than that the performance of that car was not sufficiently encouraging to warrant the directors in extending its use.

I take it that no one—not even a manufacturer of accumulators—is prepared to argue that accumulators can compete with a direct transmission method; that being so, to my mind it would be only beating the air to discuss the matter from that standpoint.

If accumulator traction is to be a success upon tramways, it must be a system which can be placed in the hands of an ordinary driver, be perfectly reliable, and work at a cost not exceeding the present methods.

Can these conditions at the present time be fulfilled by any

Mr. Dickinson. known accumulator? I think that anyone will find, after impartial investigation, that the answer will be a negative one.

Everyone who has the true interests of traction at heart must long for the time when a battery will be discovered which will give such results; and if I may be permitted, without offence, I would suggest that it would be much better if members of this Institution, rather than discuss what it is possible to do with certain batteries, would give to the Institution the commercial results in actual working of such batteries. The old adage says that it is unwise to prophesy unless you know, but I venture to say that if such a course were pursued the results would not be an all-round congratulation and hand-shaking.

In my opinion the crux of this question is not the size of the car, but the ratio of dead weight between the carrying capacity and the battery capacity; the result of my experience is that the dead weight is greater with a small car than with a large one.

If carefully studied, the balance-sheets of the Birmingham Central Tramways Co. will provide food for reflection for some of the ardent enthusiasts for accumulator traction.

The average carrying capacity of the steam cars of that company is 60 passengers, and of the accumulator cars 50 passengers. The cost from 1891 to 1895 of fuel for the steam engines averaged about 12s. per ton, and for the electric system about 8s. per ton, and yet the average cost per car mile was for the steam engines 1·74d., and for the electric accumulator cars 1·73d.

During the same period the cost for cable traction, with fuel costing the same as for the electric system, and the same carrying capacity of the cars, was 0·55d. per car mile. The lines could have been worked by a direct system of electric traction for a little less than by cable traction. This is only one item, and that not the big item, in the expenses of accumulator traction.

Mr. Harrison. Mr. HAYDN T. HARRISON [*communicated*]: As it is obvious from Mr. Epstein's most interesting paper that there is no more important factor in accumulator traction than the secondary batteries themselves, it may be interesting to you to know some of the results I have obtained during the last two years on batteries

which I have been called upon to test and report, more particularly as many of these batteries are not at present on the market. Although I quite agree with Mr. Epstein that Planté type batteries, on account of the rough treatment they will stand, would be no doubt the best for traction purposes, nevertheless, in my experience I have never found them capable of giving the same output for weight as the more modern types of paste cells. This no doubt accounts for the fact that most of the modern researches are being made in paste cells; and I should consider that it would be a bold thing to prophesy that paste, by some process or another, could not be made sufficiently hard that it will not disintegrate under the conditions which it will be submitted to in traction cells. Mr. Harrison.

One of the most successful of this type of cell I have tested was invented by Mr. W. H. Smith. These cells, containing five plates, will give a discharge of 11·5 watt-hours per lb. (of gross weight) of cell at a discharge rate of 1 ampere per lb. (gross). Considering that this battery is designed in such a manner that it is suitable for traction purposes, I consider that it shows the great advance which has taken place in the last two years.

The improvement in Planté cells has no doubt also been great, but the output for weight, as far as my experience goes, of this type of cells has never exceeded 8 watt-hours per lb. (gross) at 1 ampere per lb. discharge rate.

Considering the advance above mentioned, there is in my mind very little doubt that the opening of the market for motor-car cells will cause makers and inventors to so improve their cells to the extent that it will be possible to make accumulator traction not only a pleasant mode of conveyance, but also a financial success; and this happy stage is no doubt brought nearer to us by the interesting discussion we have just had on Mr. Epstein's paper.

The President. The PRESIDENT: I much regret that, in consequence of indisposition, Mr. Epstein is unable to be with us to-night; he has, however, sent his written reply to the discussion up to the time of its adjournment at our last meeting. This will be read by the Secretary, and a copy of the remarks made this evening

The President. forwarded to Mr. Epstein, so as to enable him to supplement his reply accordingly. With reference to what Mr. Dickinson has just said in his communication, I do not think there is any dispute as to whether traction by overhead wires is or is not cheaper than accumulator traction. I think we all know and admit that the former is the most economical system, but it may be very desirable to introduce accumulators where the civil authorities will not hear of overhead wires being erected. There are many places in the United Kingdom to which this remark applies. With regard to the Birmingham installation, I remember going to see it on several occasions soon after electric traction was introduced, and I quite agree with Mr. Parker that the cars might have been more suitable for the work. I remember well that the road was pronounced to be out of gauge. There was a very severe curve at one end of the line, and the storage batteries probably were not so good in those days as they are at present. I mention this because I do not think accumulator traction has had a thoroughly complete and fair trial up to the present. I remember also an experiment which was made about the same time at Barking. The reports of the working were submitted to me every week. At the time of my first visit the cost of working was certainly much higher than that of horse traction, and great trouble was experienced in consequence of the axles of the cars breaking—a fact for which I might mention the electric company was in no way responsible. Here again the permanent way was not entirely satisfactory. But as they went on the cost of working steadily decreased, and I came to the conclusion that if the installation could have been increased to, say, 18 cars, instead of the five or six then in use, a very fair profit would have resulted.

I should be failing in my duty if I did not thank Professor Smith for having responded to my invitation to come here to-night and speak with reference to the gearing and mechanical details—points which have been ignored by many of the speakers, who are more especially interested in the battery.

Mr. Epstein's paper has led to a discussion which goes a good way to bring us up to date as far as accumulator traction is con-

cerned. There is one point mentioned by Mr. Epstein which I agree with him is most important, and that is, the desirability of manufacturing and arranging the battery so that, as far as possible, there should be no chance of bad contacts. In an experiment made a short time ago at Hartlepool, where they have an overhead system, the combination of overhead with accumulators was tried by running one car with storage batteries, which were used at intervals independently. The results were perfectly satisfactory, except that in the earlier trials the loose contacts gave a good deal of trouble; of course this defect was soon overcome. The experiment at Hartlepool appears to me to roughly confirm the results obtained on the Continent. I wish Mr. Epstein had stated in his paper whether the cost of accumulators per car mile had been calculated on the whole length of the line, or only on that portion of the system on which the accumulators were brought into action. On the whole, I should be inclined to think that Mr. Epstein's figures are just a little too favourable; but, even making allowance for this, the results must be considered extremely satisfactory. There is, I think, a weak point in the battery system which has not been mentioned. You never know exactly when the cells are properly charged up, and you do not know (which is still more important) when they are about to give out. I am informed that in some of the American cars they have meters which register forward during the charge, and in the reverse way during the discharge, but I do not know whether this arrangement is quite satisfactory. The President.

Mr. EPSTEIN [*communicated*], in reply, said: If devoid of any other merit, my paper may at least lay claim to having elicited a very interesting discussion. Considering the time occupied so far with the one and the other, I anticipate meeting your wishes best if I condense my reply into as small a compass as possible. Mr. Epstein.

The accumulator mentioned by Mr. Manby is certainly very interesting, mainly on account of the positive electrodes also being composed of the active material alone. We all remember similar previous attempts, which, however, did not lead to satisfactory results, for, although the capacity of such plates was high, it was necessary to discharge them at a very low rate. If this difficulty

Mr. Epstein. has been overcome in the Marschner accumulator, it would certainly mark a very decided progress; but statements with regard to accumulators possessing extraordinary qualities have been so frequent, and in so many cases unfounded, that one cannot help feeling somewhat sceptical in the absence of absolute proofs.

As regards the accident mentioned, when the whole of the town current, which is given as 500 volts, was by mistake sent through the cells, it would have been more valuable to know what the strength of the current was. Incidentally I may remark here that such an accident would not have happened if the precaution of inserting cut-outs or safety fuses in the circuit had been adopted. The assertion that no other cells would have stood such an accident is unfounded, as many other accumulators, especially of the Planté type, may be short-circuited without suffering any damage.

In Planté's original researches, published in his "Récherches "sur l'Electricité," the effect of a cell of given dimensions and weight is generally expressed by the time during which it kept a short wire, connecting the poles, at a red (or even white) heat.

Another statement which calls forth criticism is the efficiency, which I take to mean energy-efficiency, and which is said to exceed 90 per cent. Although such an efficiency may be obtained in the laboratory under exceptional circumstances, there is no accumulator in existence giving such a good result in actual practice. It would be very interesting to learn what results are obtained with that type of cell in the hands of some independent authority in this country.

I am afraid that the American invention mentioned by Mr. Crompton, to prevent the crumbling of material, will not be found to answer its purpose, as the soft rubber envelopes will, in course of time, get hard and crack. It seems also only too likely that the perforations, if large enough to admit the electrolyte, are also of sufficient size to allow disintegrated material to be blown out through them.

I must express my thanks to Major-General Webber and Mr. Manville for having referred to the Birmingham Central Tramway Company. As will probably have been noticed, I have inten-

tionally abstained in my paper from alluding to anything of a personal nature. I am all the more obliged to these gentlemen for their remarks that the accumulators have not had a fair chance on the Birmingham line, and that the unsatisfactory results there should not be considered a criterion as to the merits or demerits of accumulator traction. It is a deplorable fact that the bad financial results on the line mentioned have considerably retarded the adoption of secondary batteries for traction purposes. Mr. Epstein.

I should have been glad to comply with Major-General Webber's wish to deal in my reply principally with the question of tramways, but this matter, if properly treated, would be so voluminous that I was afraid to introduce it on the present occasion. I may, however, take a future opportunity for bringing the desired facts and figures before the Institution.

Nothing could have been more gratifying to me than Mr. Manville's remarks that my ideas correspond exactly with his own, as such endorsement proves them to be correct, and as this valuable support justifies the hope that we shall in all probability soon witness the more general adoption of batteries for traction purposes.

The apparently surprising result that cells of the same make should not deteriorate more when vibrated than when still, may be explained on the hypothesis that the expansion and contraction of the material during charging and discharging does exert much more energy, tending to disintegration, than the vibrating of the whole cell.

The tractive force on ordinary level roads, which I have assumed to be 60 lbs., will be found, on comparing the figures obtained from different quarters, to represent a fair average; and the instance which Mr. Manville gave from his own experience—that on wood pavement 30 amperes and 80 volts were taken in one of the London cabs—corresponds almost exactly with the average of 2,500 volts mentioned by me.

As regards the variation of speed obtained by dividing the batteries into groups which may be used in parallel or series, there is not the slightest objection to this arrangement, provided the sections are arranged symmetrically. Tests made by me of

Mr. Epstein. such an arrangement showed that the difference in the discharge of the different groups was so small as to be quite negligible.

Complying with Professor Ayrton's wish, I can add some particulars relating to the Hanover line. There are on this line 12·8 miles with overhead conductors, and 10·6 miles worked by accumulators. The charging is effected over distances varying from 1·7 to 4·8 miles, the discharging taking place over distances from 3 to 7·2 miles; but, as there is not sufficient pressure at all parts along the trolley wire, a supplemental charge is given after the car has returned to the shed. Each car carries 208 cells; when the battery is switched on they are first arranged in two halves in parallel, with resistances inserted; then the resistances are gradually cut out, and finally the two halves arranged in series. The discrepancy between 0·266d. and 0·4d. is easily explained: the former represents the cost of maintenance—the actual outlay—while the latter figure includes a superadded margin of safety at the rate of 6 per cent. per annum on the first outlay, representing the rate of depreciation common to all parts of the machinery.

Professor Ayrton will allow me to thank him for his courtesy in testifying to my love of truth, which (bearing in mind Mr. Edison's remark *re* the veracity of accumulator men) should be considered all the more meritorious in a man who has been connected with accumulators for so many years. It offers a novel illustration of the old proverb that there is an exception to every rule.

The weight of a car, with motor, or motors, and gearing, and the full complement of 52 passengers, driver, and conductor, is 8·5 tons; the weight of dummy and battery, 3 tons; total, 11·5 tons. Still, assuming that 80 watt-hours from battery terminals are required per ton-mile (a figure which I have good reasons for considering excessive), and at an output of 10 watt-hours per lb. gross weight of battery, we find that, including stoppages, the battery will be sufficient for an eight hours' service.

I have only one observation to offer in connection with Mr. Thomas Parker's remarks, when he says the trouble with cells is that they lose their capacity when subjected to abnormal

discharges; as cells can be produced which do not lose their capacity even when discharged at the highest rates necessary for propelling the tram-car. Mr. Epstein.

Adverting to Mr. Cuthbert Hall's remark as to the relation between volume and surface, I left it to be inferred, when speaking of the relation between the capacity and the volume of active material, that this active material is arranged in a suitable manner, and the plates do not exceed a reasonable thickness. It is satisfactory and encouraging to the advocates of accumulator traction that Mr. Swan considered the causes of past failures as of a remediable kind.

The discussion was appropriately wound up by Mr. Holroyd Smith and Mr. Brown, who advocated caution and a margin of safety as a preventive of failure.

As regards the interesting communication from Mr. D. G. Fitz-Gerald, it would have been instructive to learn why cells by different makers, but all belonging to the same family, should differ within such wide limits in their capacity.

The figures relating to a Tommasi cell show, amongst other results, a capacity of 10 ampere-hours per lb. of electrodes at a discharge rate of 0·454 ampere per lb., and of 9 ampere-hours at a discharge rate of 0·9 ampere.

Through the connecting link represented by an I.E.S. cell (of which no absolute figures are given in the communication), we arrive at the Faure-Tommasi, *alias* Faure-King cell, with a capacity of only 4·5 ampere-hours, or exactly one-half per lb. of electrodes at the identical discharge rate of 0·9 ampere per lb.

Now the question which I think obviously suggests itself is, What is the reason that, in the evolution just witnessed, instead of the improvement which one would naturally expect, an actual deterioration of the race has taken place?

Or is this falling off more apparent than real? the descendant possibly compensating by a stronger backbone and sounder constitution (for which read metallic support)—the guarantees of a longer though quieter life—for the absence of that excessive activity which, while it distinguishes his progenitor, predestines him for an early grave (for which read high capacity and short life).

Mr. Epstein. Adverting to the excellent results obtained with Tommasi cells three years ago in the case of an electric cab running in Paris, and nearly two years and a half ago under the difficult conditions of a scramble from Paris to Bordeaux, is it not rather surprising that we do not find after the lapse of all those years that type in the front rank, wherever batteries are used for traction purposes?

As regards the properties of celluloid to withstand electrolytic action, my own experience has taught me that this material gives very uncertain results, and cannot be depended on. Sometimes it deteriorated very rapidly, while in other instances it lasted for years without apparent deterioration.

But, apart from the general question, I think it may safely be granted that if a maker finds it necessary to discard a certain material he has very valid reasons for it. As to crediting the material of which the separators were made with the encouraging results obtained from the cell, this is, I think, beyond the mark; for it is the electrodes in the first instance, and not the accessories, which are responsible for the results. Besides, is it not just possible that at the time of the tests the separators were comparatively new?

Differing from Mr. Fitz-Gerald in the main issue, I believe that the solution of the best traction cell will be found in a Planté positive of highly developed surface, with a relatively thin layer of active material, which—formed from the metallic support itself—adheres so firmly to it that envelopes become absolutely superfluous.

Time and experience, and these alone, will show which of the prognostications was correct.

Supplement to my "Reply" [communicated].

I have only a few additional remarks to offer.

While entirely concurring in the opinions expressed by Mr. Wallis-Jones in general, there is one point regarding which I strongly disagree with him, and that is, his estimate of the life-time of cells. I am absolutely certain that the positive sections of a properly constructed traction battery will give considerably

more than the 17,000 car miles anticipated by him; and to say that the negative sections—having twice the lifetime of the positives—will be worn out after about 34,000 car miles, is, in my opinion, a conspicuous error on the safe side. Incidentally I should like to remark that with many types the negatives do not deteriorate by reason of their losing material, but in consequence of their surface becoming clogged up, thus rendering the material more or less inaccessible to the electrolyte, especially at high rates of charge or discharge. The lifetime of well-constructed and properly treated negative plates is practically unlimited, and a small allowance for depreciation will be found quite sufficient. Mr. Epstein

I trust that Professor Smith will not consider it presumptuous if I offer him my personal thanks for the valuable information regarding gearing and other "mechanical" arrangements, the importance of which is perhaps occasionally not fully appreciated by a purely "electrical" engineer.

Turning to the "handicap" question, I think that, when taking a bird's-eye view of the different systems of traction, it will be found that the allowance which accumulator traction may justly claim is not nearly so considerable as might appear at first sight.

The loss caused by the difference between input and output of the batteries can hardly be considered an exact *addition* to those other items of inefficiency which are common to all systems, as this loss is largely compensated by advantages offered by the accumulator system, but absent in others, both as regards first outlay and the cost of working.

When estimating the plant for an overhead or underground system, we must not only consider the energy required when the maximum of cars with their heaviest loads are running under the most unfavourable conditions of the road, but also that—as our output varies very quickly and between very wide limits—the efficiency will not be high; and we must, further, provide for an indispensable reserve.

On the other hand, as with an accumulator system we can work our plant at a uniform rate at or near its maximum output, and therefore at a high efficiency, and as any excess over the normal energy required is supplied by the batteries, a much

Mr. Epstein. smaller plant at the power house will be found sufficient for the same service. Where it is a question of converting an already existing line into one worked by electricity, the balance in favour of an accumulator system is still more strongly pronounced.

Considering the working of the different systems, we find that the plant of an overhead or underground installation has to accommodate itself continually to the varying demands of the traffic, leading to an incessant variation of the load, and the inherent losses and other disadvantages.

The self-contained car, on the contrary, balances those variations without reacting upon the generating plant, which can be worked uniformly, and, if desired, day and night, thus ensuring a high efficiency.

Another source of loss, viz., the fall of potential in the feeders, is also obviated with batteries, which can be charged at or near the power station.

N.B.—The disadvantages of the direct systems can to a certain extent be mitigated by using stationary batteries at the power house or at sub-stations as auxiliaries to the generating plant.

As regards handicap No. 2, viz., the dead weight of the batteries, is this not more imaginary than real? Surely, if the tremendous difference in weight between any electric tram-car and the horse-drawn car has not prevented the great technical and financial success of the former, the comparatively small additional weight, especially if carried separately, cannot by itself turn success into failure. Recent experience, such as supplied from Hanover, confirms this view.

I am glad to learn that the experiments made by Mr. Wythe Smith have proved that the figure which I gave as the average resistance to traction, viz., 60 lbs., is well above the average.

As regards Mr. Dickinson's reference to the cost of running battery cars at Birmingham, gentlemen who unite the highest professional reputation with a special knowledge of the mode of working there, have unanimously expressed their opinion that the unsatisfactory financial results obtained cannot possibly be considered a criterion for accumulator traction elsewhere.

In all probability we shall ere long see accumulators used on several lines in this country, either by themselves or in connection with other systems. There should be little doubt that with suitable types and proper modes of working satisfactory financial results will be attained. Mr. Epstein.

Mr. President and gentlemen,—I cannot help feeling highly gratified at the general spirit of friendliness which has pervaded the discussion, several of the speakers even going a little out of their way in order to make a laudatory or encouraging remark in my favour.

I thank you for this very kind manner with which you have received my paper, and I crave your indulgence if I conclude by expressing my firm conviction that the time is close at hand when accumulators—honest, good, serviceable accumulators—will be largely used for traction on rails and on ordinary roads.

The PRESIDENT: I will now ask you to accord a hearty vote of thanks to Mr. Epstein, with an expression of our regret that he has not been able to be with us to-night.

Carried with acclamation.

The PRESIDENT: I have to ask you to pass a vote of thanks to the Chemical Society for so kindly lending us the use of this room this evening.

Carried unanimously.

ABSTRACTS.

T. A. GARRETT and W. LUCAS—A NICKEL STRESS TELEPHONE.

(*Philosophical Magazine, Vol.* 44, *No.* 266, *p.* 26, *et seq.*)

This instrument is an application of the fact that, if a magnetised nickel wire be subjected to longitudinal stress, variations of this stress produce variations in the magnetisation of the wire. If the stress be a pressure, an increase in the pressure produces an increase in the magnetisation, a decrease in the pressure producing a decrease in the magnetisation.

An arrangement which gave good results was constructed as follows:—A nickel wire 10 cm. long and 1 mm. in diameter was secured at one end in a *massive* clamp of gun metal, and the other was stuck into a hole formed in the centre of a disc of pine wood 3·5 mm. thick and 12·5 cm. diameter, and secured in position by sealing-wax, the diaphragm being entirely supported by the nickel wire. Round about 7·5 cm. of this wire was wound a coil consisting of 3·9 grams of No. 40 B.W.G. silk-covered wire, the ends of which were connected through a line of small resistance to an ordinary watch-telephone of 136 ω resistance acting as a receiver. The nickel wire worked equally well whether annealed or not, and was magnetised by stroking with a magnet or by passing a current through the coil. Pine-wood diaphragms gave decidedly better results than metal ones.

When iron or strongly magnetised steel wires were used in place of the nickel wire, comparatively eeble results were obtained, showing that the effects described were not due to the relative motion of the nickel wire and the coil. The instrument did not work satisfactorily as a receiver.

ROLLO APPLEYARD—THE FORMATION OF MERCURY FILMS BY AN ELECTRICAL PROCESS.

(*Philosophical Magazine, Vol.* 44, *No.* 266, *p.* 74, *et seq.*)

The author has discovered that, if a sheet of gelatine, damp leather, or similar permeable substance be used as a separating diaphragm between two bodies of mercury and a current passed through it, an adherent film of mercury is deposited upon the surface connected to the positive pole of the battery. On replacing the diaphragm and reversing the direction of the current, the film disappears, and a second film appears on the otner side of the diaphragm, which is now connected to the positive pole of the battery.

In this manner the author has deposited films on diaphragms of filter-paper, plaster of paris, Woodbury-type gelatine, porous earthenware, asbestos paper, and sheep's skin, the only preparation of the substances, in most cases, being a preliminary damping with distilled water.

The author describes the following experiment, amongst others, viz.—A

sheet of filter-paper, damped with distilled water, is folded upon itself four or five times, and then laid on a conducting surface. An india-rubber ring is pressed down on the folded paper and mercury poured in to fill the ring; after this a battery of 200 volts is connected for about three minutes between the mercury and the lower conducting plate, the mercury being made positive. After stopping the current and removing the superfluous mercury from the filter-paper, a circular film of Hg will be left on the upper surface of the paper. On unfolding the sheet, a blackish substance is found distributed through it, graduated from the positive to the negative pole.

The author suggests that the process may be of practical utility where large surfaces are required to be coated with Hg, as in gold extraction, and possibly also in the preparation of plates for primary or secondary batteries.

L. HOLBORN—ON THE MAGNETISATION OF STEEL AND IRON BY SMALL MAGNETIC FORCES.

(*Wiedemann's Annalen*, 1897, *No.* 6, *p.* 281, *et seq.*)

The author proceeded to investigate the experimental fact—first observed by Lord Rayleigh—that, for small magnetising forces, the susceptibility, k, is practically constant, and also to confirm the observations of C. Baur, that the susceptibility, with all kinds of iron, varies according to a straight-line law as the field strength increases.

The intensity of magnetisation was measured in the present case by the deflections of the magnetometer. The samples examined were in the form of cylindrical rods 13 cm. long and 0·3 cm. diameter, and the middle of the rods were at least 17 cm. from the needle. The magnetising coil was wound on a glass tube of about 0·4 cm. internal diameter, and extended over 30 cm. length of the tube; the said coil containing, in two layers, 1,486 turns of a wire 0·025 cm. diameter. The action of this magnetising coil on the magnetometer was compensated by a second similar coil, the magnetometer being calibrated by means of a standard coils and the intensity, I, calculated from the observed magnetic moment, on the assumption that the same demagnetising factor applied for the cylindrical rods as for an ellipsoid of corresponding dimensions. The test rods were freed as much as possible from permanent magnetism, before the experiments, by annealing and jarring, and also by decreasing reversed currents.

The following results were obtained :—

No.	———	Carbon.	Tungsten.	k.
1	English tungsten steel	1·1 per cent.	2·1 per cent.	$8{\cdot}90 + 0{\cdot}264\ \mathfrak{H}$
2	Silver steel	1·1 ,,	—	$8{\cdot}66 + 0{\cdot}384\ \mathfrak{H}$
3	Tool steel (Jonas & Colver)	0·9 ,,	—	$8{\cdot}30 + 0{\cdot}400\ \mathfrak{H}$
5	,, (Marsh Bros. & Co)	1·4 ,,	—	$8{\cdot}27 + 0{\cdot}210\ \mathfrak{H}$

In these experiments $\mathfrak{H}$ varied from 0 to just over 3·0 C.G.S. units, and the variations from the straight-line law in most cases did not amount to more than two-thousandths of the whole.

Similar results were obtained for hardened cast tungsten steel (No. 5) and cast iron (No. 6).

$$\text{No. } 5 \quad \ldots \quad k = 2{\cdot}23 + 0{\cdot}032\,\mathfrak{H}.$$
$$,, \; 6 \quad \ldots \quad k = 3{\cdot}16 + 0{\cdot}236\,\mathfrak{H}.$$

Specimens of soft refined steel, high carbon iron, cast steel, wrought iron, and hard iron wire were also examined, but did not follow the straight-line law so closely as specimens 1 to 6: the hard iron wire, however, being nearer than the others.

W. DUANE and W. STEWART—ON THE DAMPING ACTION OF THE MAGNETIC FIELD ON ROTATING INSULATORS.

(*Wiedemann's Annalen*, 1897, *No.* 7, *p.* 436, *et seq.*)

The authors made some further experiments in pursuance of those made by Herr Duane about three years ago. In these earlier experiments, Duane found that a damping force was exerted on an insulator which rotates in a magnetic field about an axis perpendicular to the lines of force, this damping force being nearly proportional to the angular velocity.

From the present experiments the authors deduced that this damping force was due to the presence of impurities in the insulator. This is well shown by the following results, showing the decrease in the part (E_l) of the logarithmic decrement for the oscillations of sulphur cylinders, due to the magnetic damping, after successive redistillation of the sulphur:—

Cylinder.	Value of E_l.
Undistilled	0·00391
Once distilled	0·00046
Twice ,, ...	0·00026
Thrice ,, ...	0·00021
Five times ,, ...	0·00009

From various considerations, amongst which was the fact that the damping, for any given cylinder, in fields of gradually increasing strength, rose to a maximum and then gradually fell, it was suspected that the damping was due to the presence of iron. This was proved to be the case with the cylinder of undistilled sulphur, but with the cylinders of distilled sulphur Professor Dr. Landolt was not able to detect any traces of iron by chemical methods. The authors confirmed their hypothesis by melting half a milligram of iron filings into the bottom of a cylinder of sulphur, with which they obtained similar, but magnified, results. In this manner they were able to roughly estimate the amount

of iron present in the specimens of distilled sulphur, assuming that the damping was proportional to the mass of the iron present.

The authors attribute the damping effect of the iron in the field to hysteresis, and not to induced currents.

F. A. WEIHE—ON THE HEAT GENERATED IN IRON IN ALTERNATING MAGNETIC FIELDS BY REASON OF HYSTERESIS

(*Wiedemann's Annalen*, 1897, *No.* 7, *p* 578, *et seq.*)

The author investigated the relation which exists between the heat generated in iron, due to hysteresis, when subjected to the action of alternating magnetic fields, and the amount of heat calculated from measurements of a hysteresis loop obtained by magnetostatic experiments; the magnetic field varying between the same limits in both cases, and Foucault currents being eliminated as far as possible.

The principal features of the method employed by the author were, that the apparatus was so arranged that the heat due to hysteresis was very accurately measured by means of an ice calorimeter, and the heat due to the passage of the current through the magnetising coil was carefully insulated from the calorimeter. The alternating field was produced by means of the current from an eight-pole Siemens alternator driven by a gas engine, the current-curve being carefully determined. The hysteresis loops were obtained by the method employed by Warburg and Hönig in their researches on this subject. The following are the results obtained, tabulated with those given by earlier investigators, viz. :—

——	Ratio of Heat measured to Heat calculated from Hysteresis Loop.	Greatest Value of Average Intensity of Magnetisation.	∿ per Second.
Warburg and Hönig (1883)	68 %	2,370	4·1 and 2·05
Tanakadaté (1889)	80 %	1,370	28 – 400
Weihe (1896)	80 % (Iron)	260	55·8
	75 % (Very hard steel)	140	55·8

Full details of the apparatus employed and methods of calculating the results are given in the original paper.

A. COEHN—THE ELECTRO-CHEMICAL EQUIVALENT OF CARBON.

(*Zeitschrift für Elektrochemie*, 3, *pp.* 424–425, 1897; *Beiblätter*, 1897, *No.* 6, *p.* 533.)

It is known that carbon anodes are rapidly destroyed when in electrolytes which generate oxygen at the positive electrode, chemical combination taking place. This reaction apparently does not follow the same simple law as the

bringing into solution of metals by the current, since it is dependent on the concentration of the electrolyte, the temperature, &c. Notwithstanding that there is little prospect of bringing carbon within the range of Faraday's law, the author has electrolysed pure carbon in a solution of equal parts by volume of H_2SO_4 and H_2O at 100°, and compared the loss in weight of the carbon with the amount of copper deposited in a copper voltameter. By this method a value of $C = 3 (H = 1)$ was obtained for the electro-chemical equivalent of carbon.

S. M. LOSANITSCH and M. Z. JOVITSCHITSCH—ON CHEMICAL SYNTHESES BY MEANS OF THE SILENT DISCHARGE.

(*Chem. Ber.*, 30, *pp.* 135–139, 1897; *Beiblätter*, 1897, *No.* 6, *p.* 547.)

Whilst the electric current can be used both for the synthesis as well as for the decomposition of organic compounds, the dark or silent discharge effects syntheses exclusively. The authors allowed a number of simple substances to react together in an ozoniser, and thus obtained a large number of interesting syntheses; for example:—

$CO + H_2O = H.COOH$ (formic acid).
$CO + H_2 = H.CHO$ (formaldehyde).
$CO + NH_3 = H.CO.NH_2$ (formamide).
$N_2 + 2 H_2O = NH_4NO_2$ (ammonium nitrite).

F. W. KÜSTER and F. DOLEZALEK—A SMALL ELECTRIC FURNACE.

(*Zeitschrift für Elektrochemie*, 3. *pp.* 329–332, 1897; *Beiblätter*, 1897, *No.* 6, *p.* 552.)

From pieces of lime as free from cracks as possible, are cut two rectangular blocks of 15–20 cm. by 12 and 14 cm. high respectively. Through the middle of one of the larger faces of the first block a hole is bored, and in this hole is placed a round carbon so as to leave half of the hole as the space in which reactions can take place. The second higher block serves as a cover. Its functions are, to prevent as much as possible the access of atmospheric air, to serve as a support or guide for the second carbon, to permit the introduction of the materials to be acted upon without opening the reaction space and without interrupting the operation, and to permit the escape of the gases generated. These objects are attained by forming suitable apertures therein.

ANON.—THE REGNOLI FURNACE FOR THE MANUFACTURE OF CARBIDE OF CALCIUM.

(*L'Éclairage Électrique*, *Vol.* 12, *No.* 30, *July*, 1897, *p.* 174.)

In February last, the first works in Italy for the manufacture of carbide of calcium were opened at Pont-Saint-Martin, near Ivrea, in the Aosta Valley.

The furnace employed is on the Regnoli-Memmo system. Its action is continuous, and it allows of a rational use of electrical energy. The materials are heated before being introduced into the actual furnace.

The electric current employed is regulated by means of resistances placed in an air chamber round the crucible; by this means is utilised a part of the energy dissipated in these resistances.

The trials of this furnace showed that 200 grammes of carbide of calcium were produced for an expenditure of electrical energy equivalent to 735 watt-hours, or 1 kilogramme of carbide for five electrical H.P.-hours. The furnace employed required 50 kilowatts.

This plant was installed in the works of the original Electro-metallurgical Society, where 2,000 hydraulic horse-power are available.

E. DIEUDONNÉ—THE ELECTRIC TRAMWAY OF ENGHIEN-ST. GRATIEN-MONTMORENCY.

(*L'Éclairage Électrique, Vol.* 12, *No.* 36, *September,* 1897, *p.* 433.)

The total length of this track is 6,436 metres, and the gauge 1 metre. Some of the gradients amount to as much as 7 cm. per metre; and the track has curves of very small radius.

The rails are of the Broca type, weighing 36 lbs. per metre. The Dickinson overhead trolley system is employed, by which a multiplicity of span wires is avoided. Steel telescope tubes are employed. The trolley wire is 9 mm. diameter, and is run parallel to the track. The cars are of the symmetrical type, with no outside seats. They are designed to carry 40 passengers, and are divided into first and second class compartments.

The author gives a table stating the dimensions of the car. The weight of each vehicle when empty is 6,600 kilogrammes, and it is driven by two 25-H.P. single-reduction motors.

The cars are lit by five incandescent lamps connected in series.

The well-known system of series-parallel controllers is employed for the motors.

The rolling stock of this line consists of six service cars, together with reserve cars.

The power house contains two boilers, with room for a third. These boilers are of the semi-tubular type, having a heating surface of 120 square metres each, and working at a steam pressure of 8 kilogrammes.

The water level in the boilers is indicated by a magnetic device.

Horizontal engines of the Corliss condensing type are employed, developing 150 H.P. (effective, each when running at a speed of 160 revolutions per minute.

The two dynamos are belt-driven, and work at 500 volts, over-compounded for 550 volts at full load.

The dynamos are run in parallel, under which conditions an equalising wire is employed.

E. SARTIAUX—THE ELECTRIC LIGHTING OF TRAINS ON THE "CHEMIN DE FER DU NORD."

(*L'Éclairage Électrique, Vol.* 12, *No.* 39, *September*, 1897, *p.* 599.)

On the above line, electric lighting is employed in a certain number of carriages of all classes.

On the ordinary railway carriages current is obtained from a battery of accumulators consisting of 16 cells connected in series.

Each cell is contained in a box built of an insulating material, and consists of 7, 9, 11, or 13 plates, 10 cm. wide and 20 cm. high; the number of plates depending on the desired period of discharge, which varies from 30 to 50 hours. The negative plates are 6 mm. thick, and the positive plates 7 mm. thick.

The author gives a table of the weights of the above cells containing different numbers of plates. These cells are placed in pairs in wicker baskets, two of these baskets being placed in a chamber fixed under the frame of the carriage. Each carriage has four of these chambers, placed near the middle of the carriage. On the side of one of these boxes is a switch for the purpose of connecting the batteries together. During discharge, 14, 15, or 16 cells are used to maintain a constant potential at the lamp terminals. The above switch is enclosed in a box. Outside the railway carriage is fixed a switch for controlling all the lamps.

The lamp fittings are so designed that they can be replaced by oil lamps, and allowing of changing from one system of lighting to another.

The lamps work at 25 volts, and are of 6 to 8 candle-power. The conductors for the lighting circuit, consisting of rubber- and lead-covered cables, are fixed along the roof.

In the case of vans, the accumulators are placed in the van themselves, and contain three lamps. For mail carriages the accumulators are arranged as for ordinary carriages, 10 lamps being used. The accumulators are always charged in position from either underground or overhead conductors. The cells are charged immediately after each journey. This is generally performed at constant potential, and requires from three to five hours at most, depending on the condition of the batteries. The cells are only removed from their boxes at fixed intervals for cleaning purposes. The costs of maintenance are thus reduced to a minimum.

RICCARDO MALAGOLI—ON THE DIFFERENCE OF PHASE PRODUCED BY A POLARISER CARRYING AN ALTERNATING CURRENT.

(*L'Éclairage Électrique, Vol.* 12, *No.* 29, *July*, 1897, *p.* 110.)

The author has experimentally verified both the periodic curve of the polarisation of a voltmeter and the importance of difference of phase in electrolysis by alternating currents.

The method employed was that due to Joubert for the direct tracing of periodic curves.

The polariser consisted of a glass tube containing acidulated water in the proportion of 1 to 500 or 1 to 18.

A number of experimental curves are given.

It was found that an important difference of phase exists between the potential difference at the terminals of a voltmeter, and the alternating current passing through it, not only in the case mentioned by M. Dolivo-Dobrowolsky, where there are no free products, but also in the case in which the products of electrolysis are liberated ; and it is indeed in the latter case that the greatest difference of phase has been observed.

From these researches the author arrives at the following conclusions:—

(1) There does exist a difference of phase between the strength of current and the difference of potential at the terminals of a polariser, as M. Mengarini had previously shown.

(2) That the contradictory experiments of M. Peukert are not conclusive, the conditions under which they were made rendering the phenomenon inappreciable. By the same method, and under more suitable conditions, the author succeeded in obtaining a lag, and in measuring it.

(3) That some of M. Dolivo-Dobrowolsky's considerations were theoretically incorrect, and that the above experiments have been of help in explaining them.

(4) That the theory of electrolysis by alternating currents, which the author published in 1892, and which helped to explain the laws obtained experimentally by MM. Maneuvrier and Chappuis and by M. Mengarini, is now confirmed for the first time by the above experiments, with regard to the variations of back electro-motive force of polarisation.

— GARBASSO—THE MANNER IN WHICH A CONDENSER DISCHARGES THROUGH TWO PATHS: MECHANICAL ILLUSTRATION OF THE PHENOMENON.

(*L'Éclairage Électrique, Vol.* 12, *No.* 39, *p.* 602, *September*, 1897.)

1. In dealing with the problem of the discharge of a condenser, the author starts with the equation which expresses that the heat (joule) liberated during a period $d\,t$ is equivalent to the sum of the variations of the electrostatic energy of the condenser, and to the magnetic energy of the fields produced by the currents.

He further states that the ratio of the strengths of the currents is such that, for a given value of their sum, the above energy has its minimum value : and from this condition it results that the currents i_1 and i_2 in the self-induction circuits L_1 and L_2, of which the coefficient of mutual induction is M, are in the ratio.

$$\frac{i_1}{i_2} = \frac{M - L_2}{M - L_1}.$$

The equation which satisfies the quantity of electricity, q, which is on one armature of the condenser, is the same as in the case of the discharge by a plain wire, of which the coefficient of self-induction would be

$$\frac{L_1\,L_2 - M^2}{L_1 - 2\,M + L_2},$$

and the resistance
$$\frac{[R_1\,(M - L_2)^2 + R_2\,(M - L_1)^2]}{(L_1 - 2\,M + L_2)}.$$

These results are simplified when the mutual induction is *nil*.

2. As the current itself explains the phenomenon of the electrical circuit, so the author believes that the motion of a completely linked system could be employed for illustrating these phenomena.

The author chooses the rotation of a solid around a fixed axis: the angular velocity would be analogous to the strength of current, the moment of inertia to the coefficient of self-induction. &c.

Passing to the case of two wires in parallel, the author describes a system in which a body rotating around an axis assumes, by the aid of gearing, an angular velocity which is always equal to the sum of the angular velocities of rotation of two other independent bodies.

— DUBOIS — PHYSIOLOGICAL ACTION OF A GALVANIC CURRENT AT THE MOMENT OF CLOSING CIRCUIT.

(*L'Éclairage Électrique, Vol.* 12, *No.* 31, *p.* 222.)

In carrying out these experiments the author dealt with the following questions :—

(1) Does the same (minimum) muscular contraction always manifest itself at the same voltage or at the same current?

(2) What is the influence of the true resistance of the body on the physiological action due to closing circuit?

(3) What is the effect of the rheostat in the main circuit used for the purpose of measuring the current?

The voltage and current are regulated by means of a rheostat connected as a shunt.

The volts are measured by means of a condenser and ballistic galvanometer, and the current by means of a Lipmann capillary electrometer connected to a 50-ohm portion of the main rheostat.

These results of the experiments showed—

(1) That the physiological effect depends much more on the voltage than on the current.

(2) That the true resistance of the body—on which, of course, depends the current—has practically no effect on the physiological action produced by closing circuit.

The resistance of the body may by the action of the current itself fall from 271,600 to 72,234, producing an increase in the current from 0·5 to 0·188 without a decrease in voltage. In order to obtain the minimum contraction at 13, 16, 12·55, and 12·34 volts. it is necessary to employ artificial means, and to still further lower the resistance in order to apply still stronger currents. With regard to the third question above, the results of experiments show that: The rheostat resistances, placed in the main circuit, prevent the physiological effect due to making circuit, even though the ohmic value of these resistances is absolutely negligible with regard to the resistance of the body; the addition of 100 to 200 ohms to the rheostat may prevent contraction, although the current has increased owing to the

diminution of cutaneous resistance. Numerous experiments have all shown that this weakening effect of additional resistances is not confined to metallic rheostats, but also with non-polarised liquid rheostats, with kaolin and graphite rheostats, and with a resistance consisting of a pencil line on ground glass.

The introduction of a solenoid has a still more marked effect than that of a rheostat of same ohmic value, but the difference is not very great.

From this the author concludes that so-called non-inductive resistances have a considerable coefficient of self-induction, capable of annulling the physiological effect, not only when the current remains the same, but also when it increases.

The author endeavoured to prove that the self-induction of a rheostat prolongs the period of charge by connecting a conductor to the terminals of the additional rheostat. This clearly confirmed the above views.

The physiological effect depends on E as a function of Ra, the apparent resistance of self-induction.

The body has a great ohmic resistance, but an almost negligible resistance: it is for this reason that the physiological effect should be measured with a voltmeter, and not with a galvanometer.

A. RIGHI—ON THE ABSORPTION OF ELECTRO-MAGNETIC WAVES.

(*L'Éclairage Électrique, Vol.* 12, *No.* 38, *September*, 1897, *p.* 573.)

The waves emitted by an oscillator, fitted with its parabolic mirror, pass through a glass plate, A, in an oblique direction, and are then directed normally to a metallic mirror, S, which reflects them back to the glass plate A, a portion of them returning to the resonator.

If a dielectric disc, P, be applied to the plane mirror S, the radiations will be partly reflected by the disc, and partly transmitted; the transmitted portion will again be reflected by the mirror S, and directed towards the resonator. If the introduction of the dielectric P does not sensibly weaken the action of the resonator, this indicates that the dielectric does not absorb the radiations.

The accuracy of the experiment depends greatly on the dimensions of the disc and of the mirror, which it is advisable to make large. It is necessary that the disc should not be smaller than the mirror.

With apparatus giving a wave-length of 10·6 cm. the mirror must have a surface between 1 and 6 sq. cm. The ratio by which is reduced the intensity of radiations acting on the resonator, due to the action of the dielectric, is $\frac{\text{Cos}^2 \alpha}{\text{Cos}^2 \beta}$, α and β being the angles through which the resonator must be turned round the direction of propagation of the waves which it receives, to cause the sparks to disappear successively when the dielectric is removed and then replaced.

The author gives a table of the values obtained.

Wood acts in the same manner as a circuit of metallic wires parallel to its fibres, but the absorption is not so great.

ANON.—THE L. W. DOWNES AND W. C. WOODWARD SYSTEM OF LEAD FUSES.

(*L'Éclairage Électrique*, *Vol.* 12, *No.* 36, *September*, 1897, *p.* 467.)

The faults of ordinary lead fuses may be summed up under two headings—firstly, the irregularity of the point of fusion; and, secondly, the tendency to establish an arc at the moment of fusing. A conductor carrying an electric current can only attain its equilibrium temperature at the end of a certain time, depending on a good many causes, such as strength of current, source of heat, and external circumstances, producing a more or less rapid dissipation of this heat. An arc often forms because the distance between the terminals is too small, and owing to the volatilisation of the wire or of the terminals.

To obviate these troubles the inventors use a straight fuse wire soldered to terminals, and enclosed in a tube of insulating material, protecting it from currents of air and from injury.

To prevent the formation of an arc, the tube is filled with a special insulating material in the form of a powder, which is a bad conductor of heat.

The tube is enclosed by metallic ends, which are perforated, to allow of the escape of metallic vapours when the fuse is melted.

In the first types the above insulating powder filled the whole tube: this produced irregular results. This disadvantage was got over by leaving an air space round the middle of the conductor, which is done by surrounding it with a small paper bag before filling the tube with powder.

Sand was at first used, but it was found to vitrify under the effect of heat, and the vent-holes got blocked up. The powder at present employed remains the secret of the inventors.

The fuses are designed to carry their normal current indefinitely at a temperature varying between 20° and 25° C. The fuse would carry a 10 per cent. increase for a very long time, whereas an increase of 30 per cent. would cause fusion in less than one minute.

These conditions are specially necessary in the case of fuses used on motor circuits, and are better adapted to this purpose than magnetic cut-outs.

CLASSIFIED LIST OF ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM

Appearing in some of the principal Technical Journals during the Months of June to November, 1897.

S. denotes a series of articles. I. denotes fully illustrated.

ELECTRIC LIGHT AND POWER.

W. A. Price—Alternating Currents in Concentric Cables.—*Phil. Mag.*, No. 266, 5th series, July, 1897, p. 61 (I.).

G. Claude—Observations on the Working of Alternating-Current Arc Lamps.—*Bull. de la Soc Int. des Electriciens*, vol. 14, No. 139, June, 1897, p. 331.

A. Blondel—On the Phenomenon of the Electric Arc.—*C. R.*, vol. 125, No. 3, p. 164.

M. Travailleur—The Electric Light Installations of the City of Brussels.—*Ecl. El.*, vol. 11, No. 24, June, 1897, p. 496.

— van Vloten—On Central Stations for Electric Light as well as Traction Purposes.—*Ibid.*, No. 26, p. 594.

G. Richard—Arc Lamps.—*Ecl. El.*, vol. 12, No. 25, July, 1897, p. 52 (I.).

J. Bijur—The Regulation of Transformers.—*Ibid*, No. 31, July, 1897, p. 214 (I.).

J. W. Howell—On the Use of Incandescent Lamps on 110- and 220-Volt Circuits. —*Ibid.*, No. 31, p. 217.

C. Jacquin—The Electric Lighting of Carriages on the Jura-Simplon Railway.—*Ibid.*, No. 32, p. 248 (S. I.).

Paul Bernard—The Applications of Electricity for Motive Power in Agricultural Work.—*Ibid.*, No. 32, p. 271 (I.).

T. C. Martin—The Daily Use of Central Electric Lighting Stations.—*Ibid.*, p. 276.

S. Hanappe—The Two-Phase Installation in the Laboratory of the Special School of Mons.—*Ibid.*, No. 34, August, 1897, p. 340 (I.).

Anon.—The New Bardon Lamp for Constant Potential.—*Ecl. El.*, vol. 12, No. 34, p. 353.

F. R. Low—On the Causes for Exaggerated Coal Consumption in Central Electric Lighting Stations.—*Ibid.*, p. 360, August, 1897.

Ch. Benjamin—The Electrical and Mechanical Transmission of Power in Workshops.—*Ibid.*, No. 35, August, 1897, p. 418.

J. E. Woodbridge—The Running of Alternators in Parallel.—*Ibid.*, No. 36, September, 1897, p. 468 (I.).

G. Pellissier—The Management of Central Stations: Tariffs.—*Ibid.*, No. 38, p. 537 (I.).

E Sartiaux—The Electric Lighting of Trains on the "Chemin de Fer du Nord." —*Ibid.*, No. 39, p. 599 (I.).

E. Gumlich—The Manufacture of the Aron Arc Lamp.—*Wied. Ann.*, 1897, No. 6, vol. 61, part 2.

Anon.—The Weber Arc Lamp.—*Ibid.*, vol. 13, No. 40, p. 24 (I.).

Anon.—High-Tension Incandescent Lamps.—*Ibid.*, No 41, vol. 13, p. 76 (I.).

G. Richard—Incandescent Lamps.—*Ibid.*, No. 44, vol. 13, October, 1897, p. 201 (I.).

W. Emmott—The Regulation of the Brilliancy of Incandescent Lamps.—*Ecl. El.*, vol. 13, November, 1897, No. 45, p. 266 (I.).

J. Howell—The Conductivity of Incandescent Lamp Filaments.—*Ibid.*, p. 267. (I.).

G. J. Shepardson—The Tests of 200-Volt Incandescent Lamps.—*Ibid.*, No. 45, p. 268 (I.).

H. Eisler and M. Reihtoffer—On the Deformation of Alternating Currents by Unsymmetrical Self-Induction.—*Ibid.*, No. 46, p. 322.

W. Beckit-Burnie—On the Factors which determine the Efficiency of the Alternating-Current Arc.—*Ibid.*, No. 47, November, 1897, p. 365 (I.).

H. Cahen—Calculation for the Conductors in Electrical Installations employing the Drehstrom System.—*E. T. Z.*, June, 1897, No. 22, p. 311 (S. I.).

Von H. Rentzsch—A New Fitting for Arc Lamps.—*Ibid.*, No. 29, p. 419 (I.).

Anon.—Safety Regulations for High-Tension Electric Installations.

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F Bedell—The Distribution of an Alternating Current in Parallel Conductors with Mutual Induction.—*Ibid.*, No. 11, p. 1005.

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L. Silberstein—The Electric Water-Pumping Plant at the Memel Delta.—*Ibid.*, p. 597.

M. Gruhn—Circuit Arrangements on the Three-Wire System, for employing the same Machine either as Additional Machine or as Reserve Machine on either Side of the Three-Wire System.—*Ibid.*, p. 676, No. 44 (I.).

R. Herzfeld—On the Electric, Carbon Arc Light.—*Wied. Ann.*, vol. 62, part 3, No. 11, October, 1897, p. 435.

A. Blondel—On the Phenomenon of the Electric Arc.—*Jour. de Phys.*, vol. 6, 3rd series, October 1897, p. 513.

DYNAMO AND MOTOR DESIGN.

A. Potier—Note on Asynchronous Motors.—*Jour. de Phys.*, 3rd series, vol. 6, July, 1897, p. 341 (I.).

J. L. Rontin—On Rotatory Transformers.—*Ecl. El.*, vol. 11, No. 25, June, 1897, p. 531 (I.).

C. CAMICHEL—Determination of the Number of Turns in the Winding of a Dynamo.—*Ibid.*, vol. 12, No. 29, p. 116, July, 1897.

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J. FISCHER-HINNEN—On the Influence of Fall of Potential in the Design of Alternators.—*Ibid.*, No. 43, p. 145; and *E. T. Z.*, No. 41, October, 1897, p. 633 (I.).

H. CARHART—The Construction and Tests of a Transformer of 1,250 Watts.—*Ibid.* No. 43, p. 176 (I.).

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W. M. STINE—On a Dynamo Characteristic.—*Ibid.*, vol. 13, November, 1897, No. 46, p. 321.

ANON.—The Wydts and Rochefort New High-Tension Transformer.—*Ecl. El.*, November, 1897, No. 47, p. 362 (I.).

L. FLEISCHMANN—Remarks on Hole and Tooth Armatures.—*Ibid.*, p. 363.

L. FLEISCHMANN—A Method for Determining the Compounding of Dynamos.—*Ibid.*, p. 364; and *E. T. Z.*, No. 43, October, 1897, p. 663 (I.).

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F. G. BAILY—The Magnetic Field in Hole-Wound Armatures.—*Ibid.*, p. 415 (I.).

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